Four Basic Aspects of Preventive Psychiatry

REPORT OF THE FIRST
INSTITUTE ON
PREVENTIVE PSYCHIATRY

Edited by Ralph H. Ojemann

State University of Iowa
1957

TABLE OF CONTENTS

Foreword

This publication is a report of the first Institute on Preventive Psychiatry held at the State University of Iowa on April 3, 1957. The institute was planned by the University's Committee on Preventive Psychiatry and was supported by grants-in-aid from the National Institute of Mental Health, the Grant Foundation, and the Iowa Mental Health Authority. The members of the State University of Iowa Committee on Preventive Psychiatry are the following:

Chairman—Dr. Ralph H. Ojemann, Professor, Child Welfare Research Station
Dr. John Chantiny, Assistant Professor, Child Welfare Research Station
Dr. Harvey H. Davis, Provost, State University of Iowa
Mrs. Maxine Delmare, Research Associate, Child Welfare Research Station
Mrs. Margery Hoppin, Research Associate, Child Welfare Research Station
Dr. Kenneth Hoyt, Assistant Professor, College of Education
Dr. Paul Huston, Professor and Head, Department of Psychiatry, College of Medicine; Director, Psychopathic Hospital
Dr. Walter F. Loehwing, Dean, Graduate College
Miss Mary Lohr, Assistant Professor, College of Nursing
Dr. Lloyd Lovell, Assistant Professor, Child Welfare Research Station
Dr. Boyd R. McCandless, Director, Child Welfare Research Station
Dr. Carlton M. Singleton, Assistant Professor, College of Education
Dr. Bill Snider, Research Assistant Professor, Child Welfare Research Station
Dr. Franklin H. Top, Professor and Head, Department of Hygiene and Preventive Medicine, College of Medicine; Consulting Director, State Bacteriology Laboratory
Mrs. Alberta Wells, Research Associate, Child Welfare Research Station
Dr. F. Eugenia Whitehead, Professor and Chairman, Home Economics

Details of the physical arrangements were under the supervision of Dr. William Coder, coordinator of conferences and institutes for the University.

A statement of the purposes of the institute was prepared well in advance of the meetings and copies were supplied to all discussion participants. This statement is reproduced in Chapter I. Chapters II through V contain the prepared papers which were presented at each session and the subsequent discussions.

Approximately 150 persons from the fields of psychiatry, education, pub-

lic health, child and social psychology, sociology, pediatrics, and journalism were in attendance. They came from twenty states and Canada.

As indicated in the statement of purposes, it was recognized that a fruitful study of the problem of prevention of mental illness and the fostering of creative development in children would require the cooperation of a number of disciplines and the use of a variety of approaches. In planning any one institute, which is necessarily limited as to time, it is not possible to have all the approaches fully represented. Therefore, this institute was devoted to four aspects of the problem, namely: (1) a clarification of the concepts of prevention and creative development, (2) an examination of recent data on factors associated with the prevalence of mental illness, (3) a critical analysis of recent research on the effects of education in human development, and (4) suggestions as to next steps in research. It is hoped that this first institute will be one of a series and that, in the course of time, all pertinent aspects will be represented and examined.

When one considers, on the one hand, the tremendous cost in money, time, human energy, and human happiness that emotional disturbances entail, and, on the other, the extensive possibilities represented by effective preventive measures, if such can be developed, it is not difficult to grow enthusiastic. But the development of preventive measures will require extensive and careful research. It was hoped in this institute to capture the stimulation that comes in sensing some of the possibilities of work in this sphere and to reaffirm our dedication to careful research that may push forward the boundaries of knowledge in this area as it has in other areas. How well the institute succeeded is perhaps recorded in part in the following pages and in part in the experiences of those who attended.

The proceedings of the First Institute on Preventive Psychiatry were recorded on tape by the University Audio-Visual Service. After the recordings—except for those covering the prepared speeches—were transcribed, the discussions were edited by the following members of the Child Welfare Research Station staff: Paul Bruce, Maxine Delmare, Sheldon Friedman, Margery Hoppin, Frances Horowitz, and Alberta Wells. The manuscript was prepared for publication by Esther Tuttle.

The typographical design was by Dale Ballantyne of the University's Department of Publications.

The Institute on Preventive Psychiatry was made possible through the cooperation of many agencies and individuals. Sincere appreciation is expressed to all who helped in this enterprise.

Ralph H. Ojemann, Director
Preventive Psychiatry Research Program

Program

FIRST INSTITUTE ON PREVENTIVE PSYCHIATRY

April 3, 1957

Iowa Memorial Union
State University of Iowa
Iowa City

8:30 a.m.	Registration	North Lobby
9:00 a.m.	Morning session	Pentacrest Room
	Presiding	Dr. Paul Huston
	Presentation of Topic—The Concepts of "Prevention" and "Creative Development" as Applied to Mental Health	Dr. Jacques S. Gottlieb
	Discussion led by	Dr. Ivan C. Berlien
10:30 a.m.	Intermission	
10:45 a.m.	Presiding	Dr. Franklin H. Top
	Presentation of Topic—Factors Associated with Prevalence of Mental Illness	Dr. August B. Hollingshead
	Discussion led by	Dr. Ivan C. Berlien
12:30 p.m.	Luncheon	River Room
2:00 p.m.	Afternoon session	Pentacrest Room
	Presiding	Dean Elmer T. Peterson
	Presentation of Topic—Recent Research on Effects of Education in Human Development	Dr. Orville G. Brim, Jr.
	Discussion led by	Dr. Milford E. Barnes, Jr.
4:00 p.m.	Meeting with Experimental Teachers	
	Introduced by	Dr. Ralph H. Ojemann
6:30 p.m.	Dinner Session	River Room
	Presiding	Dean Walter F. Loehwing
	Presentation of Topic—Next Steps in Research	Dr. Marian Radke Yarrow
	Discussion led by	Dr. M. Brewster Smith

CHAPTER I

*The Purposes of the Institute**

RALPH H. OJEMANN, PH.D.

In recent years the possibility of reducing the amount of mental illness and the prevalence of emotional disturbances by applying preventive measures and, moreover, the hope of increasing the amount of emotionally satisfying experiences in individuals have attracted the interest of many persons. The number of persons who are emotionally so seriously disturbed as to require care and treatment and the amount of worry, fear, and illogical social conflict in our society have challenged workers in the fields of medicine, education, psychology, sociology, and many others.

Studies of many types of behavior disturbances have indicated that preventable environmental stresses and personal misinterpretations of experiences often seem to play an important role in their development. Such findings suggest the possibility of developing preventive measures. Furthermore, many people in various professions have become interested in the problem of increasing the amount of "creative" and "satisfying" uses of human resources. Merely preventing a disturbance is not enough. In public "physical" health most students are not willing to stop with "freedom from disease." The search is for ways in which the energies of each human personality can be released in "creative" and "satisfying" achievement. The interest in the problem of prevention as applied to the mental health area is thus widespread.

However, when we analyze this general problem carefully, several misgivings at once arise. What exactly do we mean by an "emotional disturbance?" What do we mean by "mental health?" Who is healthy and who is disturbed? If we do not know what mental health means, how can we do anything about it?

A study of this question seems to suggest that the terms "mental illness," "mental health," and "creative living" are general terms with, consequently, limited scientific usefulness. They describe an area only in a general way, just as the term "electricity" covers a large area. No one can give *the* defini-

* This statement was developed with the help of the Preventive Psychiatry Research Committee. Appreciation is expressed to Dean Walter F. Loehwing, Professor Kenneth Hoyt, Professor Carlton M. Singleton, Mrs. Maxine Delmare, Mrs. Alberta Wells, Dr. Bill Snider, Professor Boyd R. McCandless, Dr. Paul Huston, Dr. Franklin H. Top, and Dr. Eugenia Whitehead. Appreciation is also expressed to Dr. Eugene E. Levitt and Dr. William H. Lyle, Jr., who reviewed the statement.

tion of "electricity." A variety of observable and measurable electrical phenomena have been studied; and, out of this, our conception of "electricity" has been and still is being built up. One of the tasks of students in the field of mental health, therefore, is to specify in measurable or reliably observable terms the various phenomena that may come under these general headings and then to investigate their nature and development. For example, we can define shyness in observable terms, and we can study how it develops in the growing organism, the effect of various types and amounts of shyness on the growth of the individual's personality and so on. Through a large variety of such studies, our concept of "mental health" and "creative living" gradually will be built up. It is one of the purposes of this conference to recognize this problem, to see where we are and to ascertain what our next steps might be.

In addition to this problem of general orientation, there are other questions. What are the basic approaches in prevention? Some workers in the mental health field have conceived of "prevention" as consisting of locating early the child who is in trouble and then treating him to "prevent" the difficulty from becoming more serious. But there are some students of the field who have suggested that this "waiting until the child gets into trouble" and then treating him is essentially therapy and not prevention. Some writers, such as Lemkau,[1] have used the term "secondary prevention" for this "therapeutic" activity and have reserved the term "primary prevention" for those activities which try to locate the factors producing the "troubled child" and then attempt to remove the "infective forces". The concept of prevention as it finally was developed in connection with such diseases as typhoid fever, yellow fever, and the like, was of the "primary prevention" type, as Lemkau uses the term. May we not expect a similar development in the mental health area?

A further question that arises relates to the approach to be used in studying the causes of mental or emotional disturbances and the development of mental health. Some investigators have suggested that the problem is primarily one of endocrinological and metabolic balance and imbalance and that the basic contributions will come via the chemical route. For some of these investigators, experiences make little or no basic change in personality; and they believe that, given an environment that provides food and shelter, the personality will unfold.

Some investigators have assumed that experiences in family, school, and other groups play an important role. Many feel that family experiences, especially during the early years, have a significant effect on development and that a study of these family experiences will be most productive. Some investigators have hypothesized that such experiences vary with the social

class to which the individual belongs and that we might obtain some insight into causes by studying the variation in prevalence of mental illness among social classes. Some students of the problem have suggested that society as a whole through its emphasis on such aspects as conformity plays an important part in the development of mental illness. Some have turned to the study of other existing cultures and have made cross-cultural comparisons. Still other investigators have recognized that the experiences a given culture supplies can be altered; thus they have been interested in devising altered or planned learning experiences and studying their effects.

An increasing number of investigators are calling for a more comprehensive and integrated approach. To them it appears that we are dealing with a highly complex organism of given physiological and psychosocial characteristics at a given moment and that this complex organism is in constant interaction with its environment. Through this interaction, both organism and environment may be changed. Thus the experiences the culture happens to provide at a given time have an effect on the organism. But these experiences are not fixed. Man, through his ability to think and imagine, can plan new or different experiences and study the effects of these planned experiences.

If we follow this line of thinking, we arrive at an approach that brings together a number of disciplines. The need for such an integrated approach has been well expressed by Dr. Robert Felix, Director of the National Institute of Mental Health:

"I think ten years from now we are going to see a number of large centers in the United States with a group of scientists from a number of disciplines—physiology, chemistry, psychology, sociology, education, psychiatry, and so forth—working together."[2]

In this institute we wish to recognize the complexity of the organism and its interaction with the environment. We wish to take the comprehensive view. However, since it is not possible, because of limitations of time, to have all the approaches fully represented, the strategy which underlies the planning of this First Institute on Preventive Psychiatry is to make a beginning by filling in some parts of the picture at these first meetings and giving attention to other parts in subsequent gatherings.

In addition to some attention to a clarification of the field, two approaches will be represented. The first is that of the study of the relation of prevalence of mental disorders to social class. What hypotheses as to infective forces do these studies suggest? The second approach is that of the study of the effect of training teachers, parents and the child himself. Many investigations have suggested that teachers and parents untrained in the dynamics of child behavior tend to produce stresses that may exceed

the child's level of stress tolerance. Suppose we train the teacher? Likewise the parent? And suppose we take cognizance of the relatively "non-dynamic" or "non-causal" approach[3] now taught through current curricular content in school and home and suppose we supply a "dynamically oriented" or "causally oriented" content for the child? To what extent can a change to a more causal orientation be made in the child; and if it can be made, what is the effect of a change from a non-causal to a more causal orientation?

Finally, we want to consider the question—what do studies from the several different fields suggest as to the next steps in research on prevention? Throughout the entire planning for the institute runs the consideration that answers to questions as to the effectiveness of preventive measures can be developed only through careful and comprehensive research. Hence the conference asks—what have research investigations thus far revealed and what are our next steps in the study of problems of prevention?

A further suggestion as to the purpose of the discussion sessions may be helpful. Each of the topics on the program could well serve as the theme for several days' discussion. The purpose of this institute is not to prepare a final solution for each issue raised, even if that were possible. The purpose is to bring together the thinking of workers in the many different fields involved in the study of prevention to see what the agreements and disagreements are. Participants in the institute are, therefore, asked to contribute to the discussion whatever they consider will be helpful to the extension and clarification of the basic issues.

REFERENCES

1 Lemkau, Paul V., *Mental Hygiene in Public Health,* McGraw-Hill, 1955.

2 *Proceedings of the Subcommittee of the Committee on Appropriations, U.S.* Senate, 84th Congress, Second Session, p. 591.

3 Ojemann, R. H., "An Integrated Plan for Education in Human Relations and Mental Health," *J. Nat. Assoc. Deans of Women,* 16: 101-108, Mar. 1953.

INTRODUCTION TO CHAPTER II

Dr. Huston: It is not so many years ago that I heard a very distinguished leader in the field say that all we needed to do was to train children properly in their toilet habits and there would be few problems later on in life. From the preliminary material that was sent out describing the general nature of this conference, I'm sure we all recognize that we have gone beyond that rather primitive conception.

When talking with Professor Ojemann about speakers for this conference, we thought first of the name of the person who is going to speak to us because he has long been interested in the field of preventive psychiatry. It was he who, as former chairman of the Committee on Preventive Psychiatry of the Group for the Advancement of Psychiatry, wrote a very significant report which was published several years ago and which has had one of the best circulations of all the GAP reports. It is, therefore, my great pleasure to introduce our former colleague, now director of the Lafayette Clinic in Detroit, Dr. Jacques Gottlieb.

CHAPTER II

The Concepts of "Prevention" and "Creative Development" as Applied to Mental Health

JACQUES S. GOTTLIEB, M.D.,
AND ROGER W. HOWELL, M.D.

I am happy to be here in my old stamping grounds and able to participate in this fine program.

The topic assigned to me, as the initial speaker, calls for an outline, in broadly defined terms, of some of the perplexing definitions and concepts which we so glibly use in everyday practice when referring to mental health. I do not want to be "Pollyanna-ish" in my remarks, so I hope what I have to say will stimulate you to take voice.

I suppose we might as well plunge right in and swim or drown as we try to wade through the muddy waters of mental health. The mental health of an individual, I am sure you all agree, is a state of being, without much question, but the nature of this state of being is in considerable dispute.

Various authors point out that this is a varying state according to age, circumstances, and culture, to name only a few of the variables. As a matter of fact, many authorities seem to have dismissed the subject, as if to say that it is unimportant to define this state of being—as if we were all in agreement about what it is! Perhaps we should try to accept the fact that in our present state of knowledge we are unable to define this phenomenon adequately; but we certainly should not allow this to "tranquilize our anxiety" to the extent where we stop trying to understand more about what mental health is and neglect research which will expand our knowledge of this important question. This is not meant to suggest that this should be an easy question to answer. If we think of general health as a positive goal to be achieved, we cannot even define this with respect to physical health, let alone mental and emotional health. Moreover, it is not sufficient, as all of you appreciate, I am sure, to define mental health in negative terms; that is, by the absence of mental and emotional illness. Perhaps we need to develop research which reaches into this area of darkness, upon which we can base future discussions about various aspects of mental health and the ways in which they can be enhanced.

Actually most of the workable definitions of mental health as a state of being are descriptions of overt behavior. One which seems relevant for the purposes of this discussion is given by Marie Jahoda,[1] in which she includes such qualities as *an active adjustment toward mastery of the environment, a correct perception of the real life situation,* and *the presence of a stable and integrated personality,* the latter reflecting a knowledge about what kind of a personality is most apt to handle life's stresses most effectively. More simply put, perhaps, is the description given by Boudreau,[2] in which "man's success in coming to terms with his environment" is given as a measuring stick of mental health, as contrasted to the disease processes which reflect man's inability to come to terms with his environment.

These are essentially definitions based on a theory of adaptation—the better the adaption of the individual to the physical, the interpersonal, the social, and the cultural environments, the better his mental health.

Other concepts, such as maturity, immaturity, emotional instability, illness proneness, and so forth, are frequently equated as partial descriptions of states of being with important relevance to mental health. Without boring you with further definitions, let me give you the pithy remark of my ten-year-old son, who, on the way home from a Cub Scout meeting, was talking about the immaturity of some of his fellow scouts. When asked what he meant by "maturity," he thought a few seconds and then replied, "Maturity means you're grown-up on the inside so you know how to handle yourself on the outside."

This curt definition clearly includes—as a prerequisite for adaptation to the exigencies of life—good mental health, the resultant effects of what has gone on, in point of time, into the development of the personality. This includes not only the assets and liabilities of the physical being, but the effects of all of life's experiences—the interpersonal, the social, the cultural—as they impinge upon the maturation process of the person. All that we know on the subject of mental health clearly points to the importance of the antecedent experiences in determining the mental stability or instability and health or illness of the person. Moreover, most of our evidence clearly supports the fact that the earlier the age, the greater the impact and meaning of each experience on the unfolding personality—again, for health or illness.

Within the concept of proper emotional development, there is contained implicitly the thought that such development should assure the person of full utilization of his capacities for his daily living. During the growth process, the experiences of the individual should be of such nature as to enhance his potentialities. This implies that, given a healthy baby at birth, the growth experience should not only maintain this state of health throughout life, but should constantly bring to full utilization the creative ability of the individual through the proper channeling of his drives and impulses for creation and productivity within the framework of his potentialities and capacities. In such a process, the individual is constantly strengthened to adjust to the various conditions of stress, to frustrations and to deprivations as they are encountered. In such a process, the development of internal emotional conflict is maintained at a minimum.

The goals of mental health may, then, be briefly summarized as the fostering of emotional development so that the individual may release his creative and productive potentialities and adjust and adapt—to use Freud's phrase—to the vicissitudes of life.

To be sure, the limits of these goals must be practical and realistic, rather than unrealistic although perhaps desirable. What are the boundaries of these goals? I raise this as a point for discussion and for you to decide. I am sure that immediately questions come to your mind. For example, what is the optimum point on the dependency-independency continuum at various age levels for proper mental health? Similarly, what should the self-image be at three, at twelve, at eighteen, and later in life? Perhaps we know more about psychosexual development than any other aspect of maturation and can better define maturity of function at various age levels; but even here one may ask what are the realistic limits of the partial sexual impulses compatible with good mental health? What is a healthy and the

optimum ego-defense structure? What are the optimum and most efficient ways of handling hostility?

In spite of difficulties in the definitions of the perimeters of mental health, the marked increase in popularity of mental health as a community issue during the last fifty years has led to many experimental programs. Attempts to construct programs and to justify them have become one of the biggest challenges faced by several professional disciplines. The literature describing these projects is voluminous. We do not lack for theories upon which such programs may be based. We do lack, however, the construction of programs where evaluation is possible. I must here compliment Professor Ojemann,[3] whose program is not only extensive and original but has the built-in devices for evaluation. His is one of the few courageous enough to look at itself.

Programming for mental health has become so popular and so much a movement that I cannot refrain from referring to John Seeley's[4] remarks. He pointed out that the "ground swell" of interest in the mental health field is a natural part of a social phenomenon in which mankind seeks a new and "scientific" approach to discomforting conditions in life. His admonition that we recognize that mental health has become a sort of "cult," subject to the many blind spots of a cult, and accepted by the majority as a belief rather than a science is a sound warning against just what we see happening. Program after program has been initiated on nothing more than that it seems "the right thing to do" and "we are helping children," rather than in terms of goals clearly defined with built-in devices for evaluation. As a member of the Committee on Preventive Psychiatry of the Group for the Advancement of Psychiatry, I have been appalled, as we reviewed one project after another, by the little attention paid to evaluation. I have been impressed by the great need for research.

You are all undoubtedly familiar with many of the mental health programs in operation in various parts of the country and I shall make no attempt to describe any here. All are programs which attempt to affect positively, in one way or another, the emotional development of the child. These projects include the rooming-in for the newborn, the well baby clinic, the preschool, the child-centered programs for the school child, teacher-centered programs for the child in school, parent-centered programs to affect the child at home, the public health nurse and social worker, and a number of other community projects oriented toward special groups. All these projects have the general goal of improving the mental health of the recipients within the general framework that we have outlined. The methods by which these programs attempt to achieve their goals are usually nonspecific but global in character, global in the sense that any noxious variable which is recognized may be dealt with.

I should like to emphasize this point for I consider it an important one. Mental health programs have as their goal the promotion of emotional development with the hope that the various recipients will be better adjusted as a consequence. These programs are not aimed per se at preventing illness. It is hoped by many of the program directors that prevention of illness may be a concomitant of their program. The goals, however, are not specific for the prevention of illness—nonspecific in the sense that they are not concerned with any psychopathologic sequence. Nor are the methods specific towards the eradication or prevention of psychopathological sequences. The question has been raised by many as to whether any of the mental health programs are preventive in a true sense. I am not aware of any evidence that they are. At the most they are presumptive primary preventive attempts. This does not mean that programs for the enhancement of personality function are not worth while, only that the limitations of such programs must be realized.

The problem of prevention of mental and emotional illness is certainly an important and critical one. Not only are many of our treatment procedures cumbersome and time bound but our ability to prevent illness is quite limited. True, some organically determined conditions are now under control. Pellagra, the scourge of the South, is a rarity, thanks to the vitamin niacin. General paresis, a form of syphilis of the central nervous system has been almost eradicated, one could say, by the promiscuous love of our population for antibiotics. Many important variables, such as the effects of infections, anoxia, nutrition, birth trauma, etc., on the mother and embryo, are now understood and techniques have been developed to limit their effect in producing such states as mental deficiency, cerebral palsy, and convulsive disorders.

The preventive techniques utilized in these instances conform to the general techniques of preventive medicine. The success of public health measures has been in large part dependent first upon the identification of specific important etiologic agents or other variables in the illness process. This is followed by the development of techniques that can be applied to the population at large for either the removal of the etiologic agent or the important variable, as in the case of protecting our water supply against contamination with the typhoid bacillus, or strengthening the defenses of an individual against the noxious agent, as in inoculation with polio vaccine. This general technique has a certain similarity to the objectives of our previously described mental health goals; that is, removal of conditions of stress, of frustration, of deprivation on the one hand (the etiologic agent) while strengthening the ego defenses on the other (the inoculation). In the preventive program for physical illness this can be readily done for the strategy is directed toward a specific objective. For mental

illness, unfortunately, we cannot isolate a single variable, a single point of attack, but must be prepared to deal with multiple factors of etiologic import. For prevention of physical disorders, success has come only after knowledge of the etiology or of important variables has been obtained. For mental and emotional disorders we may not have the knowledge as yet to really develop preventive programs.

Do we know how to train parents to raise their children? Is there good evidence to support this? I doubt it! By indoctrinating all those who live and work with children—parents, teachers, doctors, nurses and others—in the principles of mental health, would we be able to reduce the stress, the frustrations and deprivations that the children are subject to? I have grave doubts that "knowledge" in itself changes significantly the emotional response tendencies of a single person. Can every child be made to understand his own feelings and their significance? Yet if we wanted to develop a primary preventive program for mental illness, these would seem to be some of the things we would have to do. For primary prevention consists in the utilizations of techniques applicable to large groups or intervening directly with those factors of specific importance in the genesis of the disorder.

It seems almost impossible to conceive of a preventive program that would be all-inclusive for all forms of mental and emotional illness. Perhaps the approach should be focused around a single condition—one at a time—much in the same way as public health has had success with physical ills. Without belaboring the deficiencies of our system of nomenclature, let us select at random an illness and see how far we can take it in preventive terms. Let us take a syndrome which is more clearly defined than most—involutional psychosis.

It is fairly well established that a certain type of personality seems to predispose to the psychosis. There is a psychological formulation of the dynamic development of the symptoms. There is a course of the disease which is characteristic, and there seems to be a rather highly specific treatment which is most effective. In spite of the knowledge which we have available to us about this disease, there seems to be no adequate way by which we can prevent this disorder. Is it possible to make diagnoses of persons who seem to be predisposed to the disease? If so, do we have any methods of approaching these persons so that they can be "immunized" against this reaction? Better yet, does our knowledge suggest to us anything that might be done to prevent the pre-morbid personality from ever developing?

Since the environmental precipitating event is a loss of some type—usually the loss of a love object—could we develop techniques to be used by physicians and ministers for those in bereavement so that the impact of this

type of stress would be less severe? Could physicians be taught techniques of helping relatives work through their feelings for their lost one, rather than present a callous attitude as they so frequently do today? Could ministers, instead of eulogizing the dead one and hence all too frequently intensifying the guilt of the bereaved one, help that person to understand his true feelings? Could the proper handling of the significance of death then be the point of intervention in a preventive program for involutional melancholia? Certainly if this is the Achilles heel, it calls for—as Howard Rome[5] has pointed out—entirely different ways for physicians and leaders of the gospel to handle the relatives of the dying.

We could run through all the psychiatric illnesses and conditions and raise questions of similar import. Certainly we must consider the necessity of developing primary preventive programs around specific factors in the genesis of illness. It is, therefore, questionable as to whether our knowledge is complete enough as yet for us to have any effective programming.

I cannot help but mention that when we do have knowledge, little pressure is brought to have it applied. There seems to be good evidence, as summarized by Bowlby,[6] that emotional deprivation during the first year of life has devastating effects upon the emotional development of the child. Yet many—if not the majority of—agencies concerned with abandoned and adoptive children have failed to utilize these significant findings within their own planning.

Lastly, I should like to comment briefly upon treatment itself as a preventive activity. Early case-finding and treatment, before the illness has become most severe, is usually called a secondary preventive approach. In the case of tuberculosis this strategy has been quite successful. Early diagnosis and early treatment have become so popular as practices in mental health that hardly ever does a mental health program get started without its child guidance clinic as an integral part—and, all too frequently, its only part. Evidences which have been gathered seem to suggest that a successful clinic is always reflected by an increase in the amount of recognized illness in the community where it functions. There is no good evidence to suggest that the treatment available has resulted in a decrease in the number of problems which exist in the community. Also, those illnesses which cause the communities the most trouble are probably the ones least helped by the clinic. Follow-up studies which have been done indicate that those cases which have been successfully treated are those which might have worked out an adjustment for themselves, even without the clinic.

By these statements I do not wish to imply that the child psychiatrist is ineffective. To the contrary, I have witnessed startling improvement in illness processes. However, I do say that the therapeutic techniques are

difficult and tremendously time consuming in order to achieve a therapeutic effect. Unfortunately, the average child guidance clinic is tremendously understaffed for meeting the service demands ever before it. With such constant pressure for diagnostic services, intensive therapy becomes all too frequently an impossibility. The reality of the plight of many child guidance clinics calls for a reconsideration of whether this is the best way or not of meeting the needs of children. Treatment, then, does not seem in itself to be too effective as a preventive activity.

Although I have tried to raise issues to provoke discussion with respect to the impact of the mental health movement both in fostering emotional development and preventing illness, I do not wish to convey that this movement may not eventually achieve its desired effect. I do wish to emphasize that for this movement to reach its distant goals, a great deal more knowledge must be added to that we already possess.

First we must have knowledge of the effects of our current pilot projects. We must seek negative effects as well as positive ones. With ordinary men and women coming into possession of and using a new body of knowledge and techniques of analysis with reference to themselves and to one another, we must know whether this tearing away of the veil of privacy from what was heretofore private—our inner life—relieves tensions and difficulties or adds a new dimension of stress for the channeling of old anxieties. Currently we have no answers as to whether there is more relief from tension and difficulties and illness than there are negative effects.

Secondly, we urgently need to fill the gaps in our knowledge so that efficient methods and techniques may be developed for applying this knowledge to the community. Epidemiological studies, about which you will hear considerable, are of immense importance. The public health worker will tell you how such studies have revealed pathways of prevention for physical diseases long before specific etiologic agents had been identified. We need more fundamental knowledge of how the brain works in health and disease, of how the total person adapts and functions in society. We need a great deal more knowledge about illness, its manifestations, causations, and natural history. We need the combined contributions of all those disciplines which are concerned with man and his discontents.

And lastly, we need a genius to put all this knowledge from all the contributing disciplines together.

Fortunately the remainder of today's program will consist of presentations of new knowledge. All I can hope for is that my remarks have been sufficiently provocative to precipitate discussion to set the stage.

REFERENCES

1 Jahoda, Marie, "Social Psychology of Mental Health," from *Problems of Infancy and Childhood,* ed. Milton J.E. Senn (New York: Josiah Macy Jr. Foundation, 1950).

2 Boudreau, Frank, "Mental Health, The New Public Health Frontier," *Annals of Amer. Acad. of Pol. and Soc. Sci.*; 286, March 1953.

3 Ojemann, Ralph H., "Changing Attitudes in the Classroom," *Children,* Vol. 3, No. 4, July-August, 1956.

4 Seeley, John R., "Social Values, the Mental Health Movement and Mental Health," *Annals of Amer. Acad. of Pol. and Soc. Sci.*; 286, March 1953.

5 Rome, Howard, Personal communication.

6 Bowlby, John, "Adverse Effects of Maternal Deprivation," from *Maternal Care and Mental Health,* Part I, Monograph Series No. 2, World Health Organization, Geneva, 1952.

DISCUSSION

Dr. Huston: Dr. Paul Lemkau has been taken ill and will not be able to be here. It is my pleasure to introduce our friend, Dr. Ivan Berlien of Detroit, who is currently the chairman of the GAP Committee on Preventive Psychiatry. He will lead the discussion of Dr. Gottlieb's paper.

Dr. Berlien: I'm awfully sorry that Paul isn't here. I hadn't the slightest idea that I would be pinch-hitting for him. I came here with the idea of being an interested spectator, listener, and learner, and I'm certainly grateful for the invitation. I want to share in what I hope will be a real learning experience for all of us.

As I listened to Dr. Gottlieb's paper and the discussion of the definition of mental health, I'm reminded that we have tried in vain for many years to define this very thing and that apparently we are still trying in vain. I remembered the experience we had at a GAP meeting when Karl Menninger put us on the spot and asked us all to define psychotherapy, and a hundred and fifty guys dutifully wrote down what came to mind. He had promised before the meeting ended that he was going to tell us what we had said, and he did. He said we had agreed on only one thing, and that was that psychotherapy was something that took place between two people. Beyond that we didn't agree at all. So it's not surprising to find we have a struggle in trying to define mental health.

But it seems to me that mental health, certainly in the minds of the general public, has to do with behavior. Because I am in private practice and because I am, therefore, forced to deal with families of patients from time to time, I've come to the conclusion that mental health in a relative consists of that relative's doing what the family wants him to do. The defini-

tion as to whether I'm a good doctor depends on whether or not I can make him do it. So it seems as if mental health is behavior. Yet if we try to use this as an operational concept to arrive at a definition, we are reminded of the marked differences in behavior from place to place on the face of the globe as to what is considered correct deportment or behavior. For instance, the eating habit of the Arab is quite different from the eating habits of the people of Iowa City. And if an Iowa Citian were to be invited into the tent of an Arab and eat as he does in Iowa City, he would offend the Arab and vice versa.

Then I like to remind myself that a person is well at a certain time and in a certain place, in certain company under certain conditions. That isn't so simple as it sounds. I found out during World War II that every man had a breaking point, and this was at variance with our concept of the mentally well man who would never develop any symptoms referrable to the nervous system or show any lack of emotional well being. This theory was just not borne out. We learned, rather, that it was a matter of how much a man could take and for how long he could take it; and then, when he did start to break, how did he show it? The definition of Jahoda reads awfully good until it comes to that "stable and integrated personality." Then, of course, one must ask, what is a stable and integrated personality—and we're right back where we started from. We're like a hoop snake with his tail in his mouth going down hill. I like Dave's [Dr. Gottlieb's son] definition of mental health very well—"being grown up inside so you can be grown up outside." Operationally, that's about as good as I've heard.

When we talk of the development of symptoms, one is reminded of one of Freud's works, which really isn't referred to very much, except in a paper at a psychoanalytic convention perhaps; that is, *Civilization and its Discontents,* which contains Freud's concept of the vicissitudes of the instincts. Now I don't want to throw cold water on a Preventive Psychiatry Institute; but from time to time I do wonder if it is really possible to live in this world of ours with its civilization and not develop some symptoms which a purist might define as indicative of mental ill health. Don't we pay some price, in some way, for our civilization and for the stresses we endure? We must remember that one of the properties, one of the attributes of life itself, is irritability. From study of the amoeba, we learn that even this minute form of life has irritability. Isn't it possible that our problem is to arrive at a state of well being; that is, finding out how to live with the least amount of damage to our own emotional welfare and that of the people around us?

Now the question arises as to the desirability of being absolutely tranquil. I understand that some genius has invented an electronic machine which, if its balance is upset in some way, will make the necessary adjust-

ments so that it can lapse back into inactivity again, like a sleeping dog at the hearth. If the dog is punched, he may turn over and scratch for a moment and then lie down again, having gotten rid of the irritant. Is the property of being quite tranquil one of lying on the hearth doing nothing? Is that what we really want? The New York Academy recently put out a pronouncement on the use of tranquilizers and asked this question: ought human beings to be always free of all anxiety? Is this really a healthy situation, and one that we want to set up as our ultimate goal—that we are tranquil at all times? Ernie Gruenberg, who is one of the most irritating members of our Committee on Preventive Psychiatry of GAP, is always introducing a disturbing note by asking some question that disturbs our equanimity and makes us start all over again to try and solve something else. Ernie once stimulated us with this thought: shouldn't we think in terms of helping people to be sick? Now I know Ernie's techniques, and he was needling us. Yet, this is an interesting comment. Should we help people to be sick?

I'm not going to try and cover the waterfront and talk indefinitely of prevention and the best methods of prevention since I came here to find answers to that. One question I have concerns the concept of what is primary prevention and what is secondary prevention. Now we in psychiatry and in the school of psychoanalysis in our study of psychopathology have borrowed from the techniques of our colleagues in bacteriology and preventive medicine. And we have studied, in the course of treatment of mentally ill people, both psychotic and psychoneurotic, their symptomology from the standpoint of going back through their very personal, very emotional, very charged autobiographies and their psychosexual development. In a very high percentage you will always find, in psychoanalysis, that certain things happened at certain stages in people which caused certain behavior. I'm sure you're all more or less familiar with these concepts. This method, however, is a study after the fact, and, to a degree, it is *post hoc* reasoning. It's perhaps the best or the only way that we, in our setting, could study it and could come up with ideas of prevention. But it is *post hoc*.

One sometimes has the awful feeling when you go to bed at nights and don't sleep—what if this thing turns out to be organic after all? What if it does have to do with chemistry and biophysics? We learn there may be disturbance in copper metabolism, for instance, although we don't know much about it yet. People are struggling with the problem in laboratories. If it turns out to be organic, then even though we do succeed in ameliorating a mental illness with psychological, sociological, or other means, are we really talking about primary prevention? Are we talking about primary, or is this early treatment secondary? If it is a question of biochemistry, biophysics, or what have you, in the organic area, then what about our efforts

as we conceive them and carry them out in early child relationships with parents and in school, with teachers and peer groups? Is that really primary, or is it secondary? And even if it is not organic and the factors which bring about maladjustments and emotional illnesses are due to a disturbance or to a deficiency or deprivation in the early parent-child relationships, are we then talking about primary or secondary prevention? Aren't we already dealing with a disturbance in the psyche, in the emotional set-up of that individual and, therefore, talking in terms of secondary prevention? We like to tell ourselves that what we do in these relationships and the way we handle them is primary prevention, but it could be that even that is secondary.

Now Dr. Gottlieb raises the question of the difficulty in the experimental method in trying to isolate, in the classic manner, a single variable. But any of us who have been treating people with mental illness have learned to our dismay that symptoms are always overdetermined. Mental illness is not due to a single isolated thing—as the movies might have it—where the light shines and a traumatic experience is remembered and all is well again. It isn't that simple, except in the movies. In real life, when you have a sick patient on your hands, you find out that these symptoms are always multiplied and overdetermined. The particular syndrome that Dr. Gottlieb selected, involutional melancholia, does perhaps lend itself well to our purposes. I would be so bold as to say that I think we could, to a certain degree, with perhaps fairly good correlation seek out and identify pre-existing conflicts in the people who do develop involutional melancholia. It would seem to me that we could carry that a lot farther and go back and look into the past of the person with an involutional psychosis and point pretty accurately to the life experiences which would have had a great deal to do with that. It just happens that before I came out here I was at a social function and danced with a woman whom I've known for many years and who asked, while we were dancing, if I would see her. Then she told me she was having emotional difficulty. I said to Dr. Gottlieb last night that I had been unaware of this; but in the night the wheels must have been turning because this morning I'm quite sure I could look back now and select certain facts about her autobiography that would have been flashing red signals.

Thank you very much.

Dr. Huston: Before I throw the meeting open for discussion, I am reminded here of a solution that someone recently proposed—that all we need to do is tranquilize the drinking water over the country and our problems will be taken care of!

The meeting is now open for questions and discussion.

Dr. Spiegel: I find myself so much in agreement with what Dr. Gott-

lieb and Dr. Berlien said that I don't know if I can add to it. In view of the entire situation of the prevention concept in mental health, I will limit my comments, therefore, to just two aspects: one is the definitional problem and the other is the question of the appropriate unit of prevention or treatment. I think we all realize, as Dr. Gottlieb put it so clearly, that the concept of mental health is extremely vague. There is some danger, because of this vagueness, that individual definitions or prescriptions for mental health may be so colored by the individual's notion of what it consists of as to be really harmful to a long-range type of plan. However, I should like to defend this vagueness of concept at the present time because it is so easy to attack it. I think that there is some merit in a sort of optimum vagueness of a particular concept. Certainly it does away with premature closure and premature definition. Professor Ojemann, in the paper he distributed, pointed out that although the concept of electricity in physics was for many years extremely vague, there was a lot of good work done with it.

In association with the optimum degree of vagueness which we can tolerate, there should be introduced the concept of relativity of health. This was implied, I think, in the remarks by both Dr. Gottlieb and Dr. Berlien. Certainly perfect physical health is an ideal state, not an actual state. There are none of us who don't have colds, corns, flat feet, athlete's foot, or what not. Where physical health is concerned, none of us is completely healthy and the same thing is true with regard to emotional adjustment.

Now it seems to me that there are two ways you can go about the definitional problem. One, as Dr. Gottlieb said, is defined as a state of being. If you define it in this static, cross-sectional way, as a state of being, then the concept of the relativity of this state must be introduced into the picture. We can talk about this ideal state of health: but, certainly, if we talk about it to the public, it should be only in the contexts of an ideal and not with the notion that all of us are suddenly going to be transformed into perfectly healthy physical and mental specimens. The other way of defining the notion is in terms of process, which leads me to my second point: what is, actually, the appropriate unit of study? If mental health is some sort of process of adjustment between the individual and the environment, which again has been implied, particularly in Dr. Berlien's remarks, then it seems to me that we can and, of necessity, must raise the question: what is the appropriate unit of observation and treatment? It cannot be just the individual, although most of the writings and thoughts on the subject of mental health and mental illness have been directed and specified to the individual with a search for a specific etiological or pathogenic agent. Now we know

that this is useful because of the multicausality which is imbedded in the process, but further than that we run into the dangers of the "who-dunit" approach to mental health. I think we all know how extreme this search can get and how dangerous it can be in the sense of the identification of a particular specific pathogenic agent or person, such as the schizophrenogenic mother or the overpermissive or overprotective mother. These are all the villains in the piece which can be dramatically portrayed to the public in a completely unrealistic way.

As long as one tags the individual as the unit of observation and treatment, one is tempted to look for a specific cause. But if the process really has to do with mutual adaptation between the individual and his environment, then, it seems to me, our inquiries have to be directed to both poles of this process; and we have to ask the question: what is it in the socio-cultural environment to which people must adjust that is evocative of difficulties in adjustment and what is it in the individual which has gone wrong in the developmental process of relation to this mutual process of adjustment?

As Dr. Berlien pointed out, the Arabs have quite a different idea of table manners than we do; and unless we were forewarnd, we would insult each other if we ate together according to our respective customs. We have found that to a certain extent there is a distinction between families where everybody is getting along relatively well and families where children have become emotionally disturbed. Here we have found very prominent the factors of difficulty of adjustment of the child to the particular social and cultural environment of the family where that family's notion of how things should work is very much different from the typical American notion. The children would be well enough adjusted in their families and the outside world if the outside world only conformed to the family's expectations. Much could be said about this, but I should just like to raise the question as to whether the social and cultural environment, and particularly the family in that social-cultural environment, is not at least as important a goal of direction of inquiry as is the individual himself.

Dr. Dreikurs: Since the speakers in the discussion so far have mostly agreed, may I be permitted to throw in a note of disagreement. It seems to me that the idea of mental hygiene is now about thirty years old, and we have as yet made very little progress in a reliable form of prevention. This is not due to the fact that we don't define mental hygiene because physical hygiene is also very little defined. It is because we are not in agreement about the nature of the disease. It seems to me that right now we are in a state where some progress could be possible if we realized why progress so far has not been possible. In the case of mental

and emotional disease we have witnessed in psychiatry far less of scientific procedures than in other disciplines. There is no one particular idea about the development of diseases. We first tried to overcome this isolation by interdisciplinary research. The effect was that the various other disciplines accepted the same psychiatric outlook which was prevalent in the group which started the research. We come now to the realization that perhaps we need basic research in general. And I'm a little bit pessimistic about the validity of research to clarify the issue because the researcher is still a human being and, as such, has a biased perception of everything. To give an example, there are at least eight mental research studies which evaluate the assumption of development of psychosexual character. In each of these studies the researcher has his own frame of reference and is bound to work in one of those selections.

What makes me hopeful, however, is a conference like this and the development of psychiatry and particularly psychology within the last few years. Where before the isolation of the dominant group which did not get together with others of different opinions prevented an interchange of opinion and an integration of knowledge, we have now finally reached a stage where such an integration of knowledge would be possible. In the future we may come to a clearer realization of what a disease is. The prevention of typhoid fever would never have been possible if so many different ideas about what constitutes a disease had existed then as exists today in almost every area of mental disease. In other words, it seems to me that the exchange of opinions may help more toward integration of science and toward coming to a valid concept of disease than scientific research which is merely carried on from the various angles. That is the aspect which I wanted to present to you which might explain, at least in my opinion, why we made so little progress in the past and may make much more progress now.

Dr. Levitt: I'd like to go back to Dr. Spiegel's remarks and some of the comments earlier concerning definition. In my opinion there has been a lot of wasted hot air about the question of a finer concept of mental health. The difficulty is that we are working with very gross, vague extractions where it is impossible for us to use concise definitions which will lead to some sort of practical end. The abstractions, in my opinion, are fruitless.

Now to go back to the illustration about the electricity and the practical studies of electricity which were done without a formal definition of electricity. Actually there was a definition, but it was not in terms of abstractions. If you press the button over here, you have certain wires and machines hooked up and the light flashes—that's electricity. Who

cares what's going on in terms of sign waves and flexes and bonds and things like that!

The same thing can apply to mental health. The approach would require those of us who consider ourselves experts to agree that here are people who have mental health. We don't care what it is actually that they may have so long as we agree that it's mental health. The next step, presumably, would be to determine the characteristic common denominator among other people. This would be the beginning of a science of prevention.

Dr. Lyle: I think we have witnessed some men who at least the general public would consider qualified to provide a definition of mental health. But one wonders whether they would be willing to defend it. It seems to me that in the absence of anyone willing to do this, we might ask a question as to what kinds of people fall into the categories that we consider to be mentally unhealthy. Within even the last ten years hasn't this become increasingly vague? People who ten years ago were not considered ill, in the sense of being mentally ill, now are considered to be so.

I'm wondering whether the term "mental health" has not been a barrier more than an assistant in dealing with problems in this area. I don't find it particularly surprising that there is a breaking point for a person in terms of his psychological mechanisms or his personality any more than I would if someone informed me that if you put a man's leg across a chair and pry on it and apply enough pressure on either end of it, the bone in the leg will break. To draw a parallel, we aren't really asking if we can build bones that are so masterly strong that they will resist these kinds of pressures. I should like to offer for a substitute—which might be open for discussion as to whether or not the term is useful or whether another might not be more appropriate—the phrase "optimal personality development."

Dr. Barnes: I think this has been an interesting and valuable discussion on the part of all. One thing which I want to speak about briefly here is that there seems to be—perhaps that's just one of our problems—confusion of mental health with some sort of ideal. When you begin to talk about an ideal state of affairs or an optimal state of affairs, no matter what it may be, you're in the realm of philosophy and not in the realm of science. I happened to be reading Plutarch's *Lives* when I came across the story of Lycurgus, who was a legendary law-giver of Sparta nearly three thousand years ago. He made a very scientific study, going around the various Ionian Islands, Crete, and parts of Greece, seeing how people lived and studying their customs. Then he came back to Sparta and inaugurated a mental health program in which he completely

revised the attitudes of the Spartans in a very amazing way. It's interesting to read of the sexual practices and other behavioral ways in which he altered the national character and behavior of the Spartans. The Spartan states lasted for several hundreds of years under this system of mental health—presumable mental health—which Lycurgus devised. This was the achievement of an ideal, although we would question whether it was healthy. It was the achievement of the philosophical sort of ideal.

Dr. Iscoe: I can agree that we can't get a definition suitable to everyone as far as mental health is concerned. Dr. Levitt has pointed out that perhaps we don't need it, while Dr. Ojemann's article told us that we don't need a definition as much as we need a demonstration. I'd like to make simply a plea for miniature systems. [The tape of the recording broke at this point and the rest of Dr. Iscoe's comment was lost. However, he made the point that we need a demonstration in miniature systems. He said that syndromes must be isolated and should then be linked up and measured.—EDITOR.]

Dr. Senn: We must admit that today we are much more humble than we were some years ago when we began certain ventures which we thought would be preventive in mental illness. For instance, it was thought by many people that natural childbirth would be preventive in terms of mental illness but it did not turn out to be. On the other hand, as we have become humble, we have also realized that although our initial goal was not so tame as providing better mental hospitals, these changes in medical care and patient care have been fruitful and helpful. They have brought happiness and joy to many people and have influenced for good the general patient care in hospitals. It is my hope that while we are humble today in looking at our preventive procedures, we may also be courageous. Dr. Spiegel talks about the need for looking at the social and cultural influences to which we must adapt. May we have the courage to look at the social-cultural influences, the theological and religious influences and look at them carefully and see whether they, too, are not needful of change. It is not simply the human who needs to adapt to something. Are we considering invariable, fixed, constant, and unchanging the role of the social scientist as one who merely looks at our civilization without seeing if it is unchanging or in need of change? Will we have the courage to attempt to change that?

Dr. McGuire: I think that Dr. Gottlieb pointed to a consideration that will permit some of us who are working in different areas than the field of mental health to cooperate in the problem. He pointed back to the control of the stimulus producing whatever the situation is that you are working with. Cannot the work being done by a number of different people in psychiatry, psychology, education, and in the everyday

world help to reduce borderline or ineffective functioning? As Dr. Gottlieb said, there are those among us who are concerned with—and here is the key word—"reduction" of the stimuli of these situations which produce the borderline or ineffective functioning. And there are others of us who are working with the notion of strengthening the defenses, the capacity to cope with stress, the ability to work with conflict and to live in a world which produces anxiety without slipping over the border. I think we have to do this because we cannot define optimums. You've worked with various kinds of people in our society and the various kinds of expectations that are set up for them. You cannot define, except for a very restricted group of people with whom you're working, what is optimum. So it doesn't make sense to force on us the idea that there is such a notion of optimum.

Here is where I depart from Dr. Gottlieb. I know he is concerned with how the human being learns. But I know that as long as we use the language of potentialities to be uncovered, we're tricking ourselves. We have shown that you can do a tremendous amount with even the relatively unchangeable abilities. Therefore, there are going to be those of us who are going to work on the preventive side. There are going to be others who are trying to work with individuals, with families, with particular cases or groups of cases. And they are going to try to set up situations where the learning—or for those who prefer it, the "unfolding"—will come out in what they believe to be an optimum way. I'm willing to go along with them. But there are also those of us who are working at the same time with a notion that there are a variety of products, a variety of human beings coming out, to live in many different situations and that we can do something about their learning experiences. I even had to learn to live with Dr. Roger Williams and his notion that you can produce mass changes in an organism and put things in an organism to prevent the possibility of something happening. But I also say you can provide learning experience with older people in a family situation, and you can, in many cases, sit up and do something about it. You certainly can provide other kinds of learning situations. Consequently, I think we have to keep the broadest gates we can. That's why I enjoyed the term "mental health"—that's why I will try and listen to the other people's ways of looking at it. I have a particular view as a consequence of working with a wide range of research situations. What we're dealing with is something much broader than mental functioning or emotional after-functioning. We're dealing with something which is solved in many different ways. We see cancer patients who have had histories exactly like many so-called mental patients. And yet, somewhere, there has been a build-up and a changeover into a deeply

organic solution—the not being able to cope any longer with that which faces them. It may be that we're tied very closely to the psychosomatic, the area of psychosomatic behavior as well as effective behavior.

Dr. Huston: The time has gone on to 10:30 and I think I shall ask Dr. Gottlieb to close the discussion.

Dr. Gottlieb: I'm very grateful to all those who participated in the discussion and I am very happy to see that what I have had to say provoked so much spontaneous comment. I was a little bit surprised that nobody attacked the theory of adaptation. I have great questions as to whether this is an adequate theory on which to build our structure. I'm not sure that people who are productive and who can utilize their capacities to the fullest are necessarily adapted personally and within good residence with their culture. Sometimes adaptation and good residence with one's culture ruins productivity. We have a study currently in operation, and there are studies similar to ours in other parts of the country, in which we have built a social milieu for a group of chronic schizophrenic patients. What we have seen is a beautiful boomer for social adaptation. There is no question about this—these patients were all chronic, backward, dilapidated, deteriorated, regressed individuals. Many of them are now reaching a point of social adaptation. They are *still* schizophrenic! Thank you.

INTRODUCTION TO CHAPTER III

Dr. Top: It is a privilege to introduce the next speaker, Dr. August B. Hollingshead. I should like to say a few things about him, although most of you already know him by reputation and by name. He had his early training in California, getting his B.A. in 1931 and his Master's degree in 1933. His Ph.D. was from the University of Nebraska in 1935. He's taught in several schools and Iowa happens to have been his first. He was in the Department of Sociology here for one year during the depression, trying to keep warm and at the same time get enough money to eat. I guess he was one of many in that situation, not only here but elsewhere. He also taught at Alabama and Indiana and has been at Yale since 1947. He is a professor there in the Department of Sociology and is director of the program of medical sociology, which is a joint undertaking of the Graduate School and the Medical College at Yale. Among other things he has been a consultant for the Ohio Valley Division Committee on Natural Resources and also for the National Institute of Mental Health. He is the author of such works as *Principles of Human Ecology* and *Elmtown's Youth* and co-author of *Outlines of the Principles of Sociology*.

CHAPTER III

*Factors Associated with Prevalence of Mental Illness**

AUGUST B. HOLLINGSHEAD, PH.D.

Mental illness in recent years has been recognized as the most serious unsolved health problem facing our society. A few figures will indicate the number of persons known and believed to be suffering from psychiatric disorders. First, there are 750,000 persons currently hospitalized in mental institutions in the United States; these patients occupy some 55 per cent of the hospital beds in the nation. Second, hundreds of

* The research reported here is supported by a grant from the National Institute of Mental Health of the United States Public Health Service (MH-263), to Yale University under the direction of F. C. Redlich, M.D., Chairman, Department of Psychiatry, and Professor August B. Hollingshead, Ph.D., Department of Sociology.

thousands of ambulatory patients are treated in psychiatric clinics and in private practice. Third, some 16,000 to 17,000 persons commit suicide each year, and there are about 3,800,000 alcoholics in the adult population.[1] In addition, estimates indicate there are from seven to eight million other Americans who could benefit from psychiatric care if it were available.

The problem of mental disturbances is not new. Historical evidence indicates that mental illnesses have been known in all civilized societies. Mental aberrations of kings, generals, priests, and other personages have been recorded since ancient times. Persons who were not important enough to have their mental difficulties written into the human record also must have been afflicted. Even though the problem of mental illness is an old one, the American public is more aware of it today than in earlier years. Moreover, responsible leaders have begun to see possibilities of alleviating it through the application of scientific knowledge.

Three methods have been developed by the medical sciences and arts to understand and treat diseases. These may be categorized briefly as (1) the *clinical* method of direct observations of patients with symptoms of disease; (2) the *experimental* method of examining data associated with a disease in the laboratory; and (3) the *epidemiological* method of observing a disease, and phenomena associated with it, in a defined population. Each of these ways of learning about disease has contributed to man's knowledge. The central concept in this lecture, namely prevalence, was contributed by the epidemiological method.

Since this is an interdisciplinary institute, all participants may not be familiar with the terms and concepts developed by epidemiologists. Therefore, I shall give you a few elementary definitions before I begin the presentation of substantive materials.

Prevalence is defined as the number of cases of a specified disease present in a population aggregate during a stated interval of time. The criteria that define a "case" need to be stated fully. The size and characteristics of the population aggregate have to be specified. Finally, the time interval has to be stipulated. The interval of prevalence may be one day, one week, one month, one year, or some other determined period.

A second elementary epidemiological concept pertinent to this discussion is *incidence*. Incidence is the number of *new cases* of a disease developing in a defined population within a specified interval of time. A new case needs to be defined carefully in any disease, but especially so in mental illness for reasons I will discuss in a moment.

Prevalence differs from incidence in that *all* cases of a given disease "active" in a population are counted. A prevalence figure includes "old" as well as "new" cases.

The ratio of cases of a disease "active" in a population during a specified interval of time in relation to the size of the population at risk is stated usually as a *rate*. A rate is computed by dividing the number of cases of the disease by the population aggregate. The procedure of stating findings in terms of rates has a number of advantages. First, it enables an investigator to see how many cases would be contributed by the population, or a subgroup in the population, if the proportion of patients in the population or subgroups were applied to a population of some standard size. The standard sized population used as the basis for expressing a rate may be 1,000, 10,000, 100,000, or a million. By stating findings in terms of a rate per "standard unit" the researcher is able to make comparisons of the number of "cases" in population subgroups of different sizes. I will use 100,000 as the "standard unit."

Other advantages of stating findings in terms of rates are that rates may be *adjusted* by holding one or more factors constant statistically. In this way factors that may be confounding a calculated rate are controlled. In addition, rates may be specific for sex, age, race, socioeconomic status, religion, or any other defined criterion. Finally, rates may be tested for significance by standard statistical procedures.

Determination of the number of mentally ill persons in a defined population is the first requisite to intelligent planning of how to cope with the problem before us. This has been recognized by epidemiologists and public health officers for a long time. Although national censuses in a number of different countries have attempted to gather statistical data on inhabitants who are mentally ill, none of these efforts has been successful. The studies that have been made of incidence and prevalence of mental illness in particular populations have not been made carefully; they deal only with clinical cases, or they are not comparable.[2] The net result is this: satisfactory data do not exist on the endemic distribution of mental disorders in the population of any society. All we have are very rough estimates.

The computation of incidence and prevalence rates in mental illness is extremely difficult for a number of reasons—among them being, on the one hand, the fear of mental illness in the population; and on the other, the lack of clarity among psychiatrists as to who is a "case." In spite of various attempts by psychiatrists to draw a clearly demarcated line between who is "well" and who is "sick," the boundary between mental health and mental illness remains indeterminate.[3] Unfortunately, psychiatry lacks a standard measure of what is "normal" and what is "abnormal" in emotional and psychological functioning. A standard measure of "normality" and "abnormality" would enable researchers to determine the presence or absence of mental illness in a population. It

also might enable them to estimate the proneness of some persons to mental illness. In sum, the lack of criteria for dividing the "sick" from the "well" presents great obstacles to investigators who desire to make studies of incidence and prevalence of mental illness in a population.

The determination of true or endemic prevalence and incidence of mental disorders in a defined population is dependent upon the development of standardized criteria for measuring "normality" and "abnormality" of psychological and emotional functioning. If this could be done, then a researcher might be able to examine either a total population or an adequate probability sample. The next problem would be to obtain enough competently trained psychiatrists or clinical psychologists to make the examinations. The population to be studied would have to be cooperative and subject itself to the necessary examinations and tests. These conditions have not been met by any research team. It is probable that some time will elapse before they are realized by any research group.

With these preliminary remarks as a frame of reference, I shall turn to the task at hand, namely, the presentation of empirical data to demonstrate how selected social, biological, and disease phenomena are interrelated in the prevalence of *treated* psychiatric disorders in a defined population. The data presented here are from the ongoing, extensive study of mental disorders[4] in the population of the New Haven, Connecticut, community.[5] The data were assembled by a team of three psychiatrists,[6] two sociologists,[7] and a clinical psychologist,[8] to test postulated assumptions of interdependence between social class and the prevalence of treated psychiatric disorders.

RESEARCH DESIGN

Three technical operations had to be completed before hypothesized relationships[9] between class status and mental illnesses could be tested empirically. These were: (1) the determination of who is a psychiatric "case," (2) selection of a cross-sectional sample of the community's population as a control, and (3) the stratification of both the control and the psychiatric populations.

1. *The Psychiatric Census.*

We determined who was a psychiatric "case" by taking a census of psychiatric patients. The "Psychiatric Census" was limited to residents of the community who were patients of a psychiatrist, a psychiatric clinic, or were in a psychiatric institution between June, 1, 1950, and December 1, 1950. To make reasonably certain that all patients were included in the enumeration, the research team gathered data from all public and private psychiatric institutions and clinics in Connecticut and nearby states, and private practitioners in Connecticut and the metropolitan New York

area. It received the cooperation of all clinics and institutions, and of all practitioners except a small number in New York City. Checks indicate we have data on at least 98 per cent of all residents in the community who were receiving psychiatric care on the date of the census.

Forty-four items of information were gathered about each patient and placed on a schedule. The psychiatrists selected material regarding referrals, symptomatology and diagnosis, onset of illness, the nature, intensity, and duration of treatment. The sociologists obtained information on age, sex, occupation, education, religion, race and ethnicity, family history, marital experiences, and so on.

2. *The Control Population.*

The second research operation was the selection of a 5 per cent systematic sample from the population of the community. Data on age, sex, occupation, education, religion, and income of family members, as well as other items necessary for our purposes, were placed on the interview schedule. This sample is our "Control Population." It provided a standard of comparison for the psychiatric patient population.

3. *Stratification of the Population.*

The control population and the psychiatric patients were stratified by the use of "Hollingshead's Index of Social Position." This index utilizes three factors to determine an individual's class: ecological area of residence, occupation, and education.[10] The principal classes[11] may be characterized as follows:

Class 1. This stratum is composed of well-to-do families whose wealth is often inherited and whose heads are leaders in the community's business and professional pursuits. Its members live in residential areas generally regarded as "the best"; the adults are college graduates, usually from famous private institutions. Their social life revolves around private clubs, cliques of families, and exclusive organizations of one kind or another. Almost all gentile families are listed in the local *Social Directory*, but few Jewish families are so listed. Three per cent of the population is in Class 1.

Class 11. Adults in this stratum are almost all college graduates; the males occupy high managerial positions and many are engaged in the lesser ranking professions. These families live well, but there is no substantial inherited or acquired wealth. Its members live in the "better" residential areas. The social life of this stratum tends to revolve around the family, church organizations, community associations, and business or professional organizations. Less than 5 per cent of the families in this class are listed in the *Social Directory*. Nine per cent of the community's population is in Class 11.

Class III. This stratum includes the vast majority of small proprietors, white collar office and sales workers, and a considerable number of skilled manual workers. Adults are predominately high school graduates, but a considerable percentage have attended business schools and small colleges for a year or two. They live in "good" residential areas. Their social life tends to be concentrated in the family, the less prestigeful churches, and in lodges. Twenty per cent of the population is in Class III.

Class IV. This stratum consists predominantly of semi-skilled factory workers. Its members have finished the elementary grades but the older people have not completed high school. However, adults under thirty-five have generally graduated from high school. Their residence is scattered over wide areas. Social life is centered in the family, the neighborhood, the labor union, and public places. Its members comprise 50 per cent of the community.

Class V. Occupationally, Class V adults are overwhelmingly semi-skilled factory hands and unskilled laborers; most have not completed the elementary grades. The families are concentrated in the "tenement" and "cold-water flat" areas of New Haven city. Only a small minority belong to organized community institutions. Their social life takes place in the family flat, on the street, or in neighborhood social agencies. Eighteen per cent of the population is in this stratum.

Now that we have outlined the research design and sketched the major features of the community's class structure, we will turn to the presentation of data on the prevalence of *treated* mental illnesses. We will confine the discussion to five factors: sex, age, diagnosis, length of time in treatment, and class status to see how these attributes and variables are related to prevalence.

PREVALENCE RATES AND SELECTED FACTORS

Age, Sex and Diagnosis.

We began the analysis of the data from the Psychiatric Census and the "5 Per Cent Sample" with the assumption that the prevalence of treated mental illnesses would be related to the sex and age structure of the population and the diagnoses of the patients' disorders. Therefore, the data were divided into sex, age, and diagnostic groups. The division by males and females needs no explanation. The age and diagnostic groups we used require a word of clarification.

AGE GROUPS:

The patients and the population were divided into six age groups: under 15 years of age; 15 through 24; 25 through 34; 35 through 44; 45

through 54; and 55 years of age and over. We think these categories reflect changes in social and psychiatric variables associated with age in our society. Individuals under 15 years of age are generally viewed as children. Adolescence and young adulthood is encompassed mainly in the period from 15 through 24 years of age. This is the period when young people complete school and college. Moreover, most males and females marry in this phase of the life cycle and establish adult behavior patterns. Social and physiological changes are not so marked in the decades from 25 through 44, but they cover years of social and emotional growth, as well as stress for the average person; youth has passed and middle age has begun. The involutional period generally occurs in the decade from 45 through 54; marked endocrinological changes occur in men and women in these years. Socially the family of procreation is dissolved by the maturation of the children of men and women in this age range. Economically, the family reaches the peak of its earning power. From a psychiatric viewpoint, the middle fifties are the years when disorders of the senium make their appearance clinically. Individuals at 55 years of age are on the edge of the decline that eventually leads to the physical impairments of old age and the withdrawal of the individual from active participation in society.

DIAGNOSIS:

The psychiatrists on the team adopted the diagnostic scheme developed by the Veterans Administration[12] during World War II as the best one available at the time the data were gathered.[13] The Veterans Administration schema enabled the psychiatrists to integrate the diagnostic categories of the several institutions from which we collected data with the diagnostic categories used by analytically oriented clinics and private practitioners. Each patient was diagnosed by the three psychiatrists and the clinical psychologist on the team in terms of his predominant symptomatological syndrome. Diagnoses were made on the basis of the notes in the case record, and the symptomatology given by the patient's psychiatrist. The specific diagnoses made by the psychiatrists were punched along with the rest of the information on Hollerith cards. After the cards were punched and the data tabulated, the decision was reached to condense the many diagnoses into seven categories of neurotic reactions and five types of psychotic disorders. By combining specific diagnoses into larger categories with a similar symptomatic base the data could be handled statistically.

For purposes of this paper the data will be condensed into the two major diagnostic categories: neurotic reactions and psychotic disorders. We will not have time to discuss the various subdivisions of the neuroses and the psychoses.

Prevalence of Neuroses by Sex and Age.[14]

The prevalence of neurotic patients in the population of the New Haven community by sex and age shown in Figure 1 indicates that both sex and age are related to prevalence. The curve for males parallels the one for females at all age levels, but there is a sharper differential in prevalence between males and females at approximately 30 years of age than at any other period in the life cycle. Between 25 and 34 years of age males have a rate per 100,000 approximately 150 higher than females. The next largest differential by age is among boys and girls under 14 years of age. The rate for boys is approximately three times higher than for girls. At all other ages there is little difference between the prevalence of neurotic disorders for males and females, but males have higher rates until the involutional period is reached. From 45 through 54 years of age there is no difference between the two sexes; after 55 years of age females have rates that are slightly higher than males.

The salient point about the rate curves in Figure 1 is the sharp peak in the 25 through 34 age period for both males and females. During adolescence and early adult life the rate rises sharply. After 35 years of age it drops just as sharply through the years of late maturity to old age. Clearly there is a close relationship between age and the prevalence of treated neurotic reactions.

Age and Class.

When we view the prevalence of treated neurotic disorders from the perspectives of age and social class, with the data adjusted for sex, the peaking phenomenon illustrated in Figure 1 is repeated. This may be seen by a glance at Figure 2. However, there are real differences in prevalence in the different classes by age.

In Classes I and II the rate is low in the childhood years. It rises almost perpendicularly through the adolescent and early adult years to a climax in the 25 through 34 year period. The rate drops sharply in the 35 through 44 age bracket, and continues to fall without any change in slope through the involutional years. There is a slight shift, but still downward, from 55 years of age to the end of life.

The curve for Class III parallels that of Classes I and II until age 25. Then it drops, almost in a straight line, in each of the successive age periods. The shape of the curve for Class IV is similar to that for Classes I and II, but the rate per 100,000 is much lower during every age period.

The prevalence curve for Class V follows a unique pattern. In the years 14 and under, the rate is 160 per 100,000. It drops to 120 in the adolescent and early adult years and rises to 320 in the years from 25 through 34. Then it drops irregularly in each of the successive age periods. The

FIGURE 1

PREVALENCE OF NEUROTIC PATIENTS IN THE NEW HAVEN COMMUNITY BY SEX AND AGE

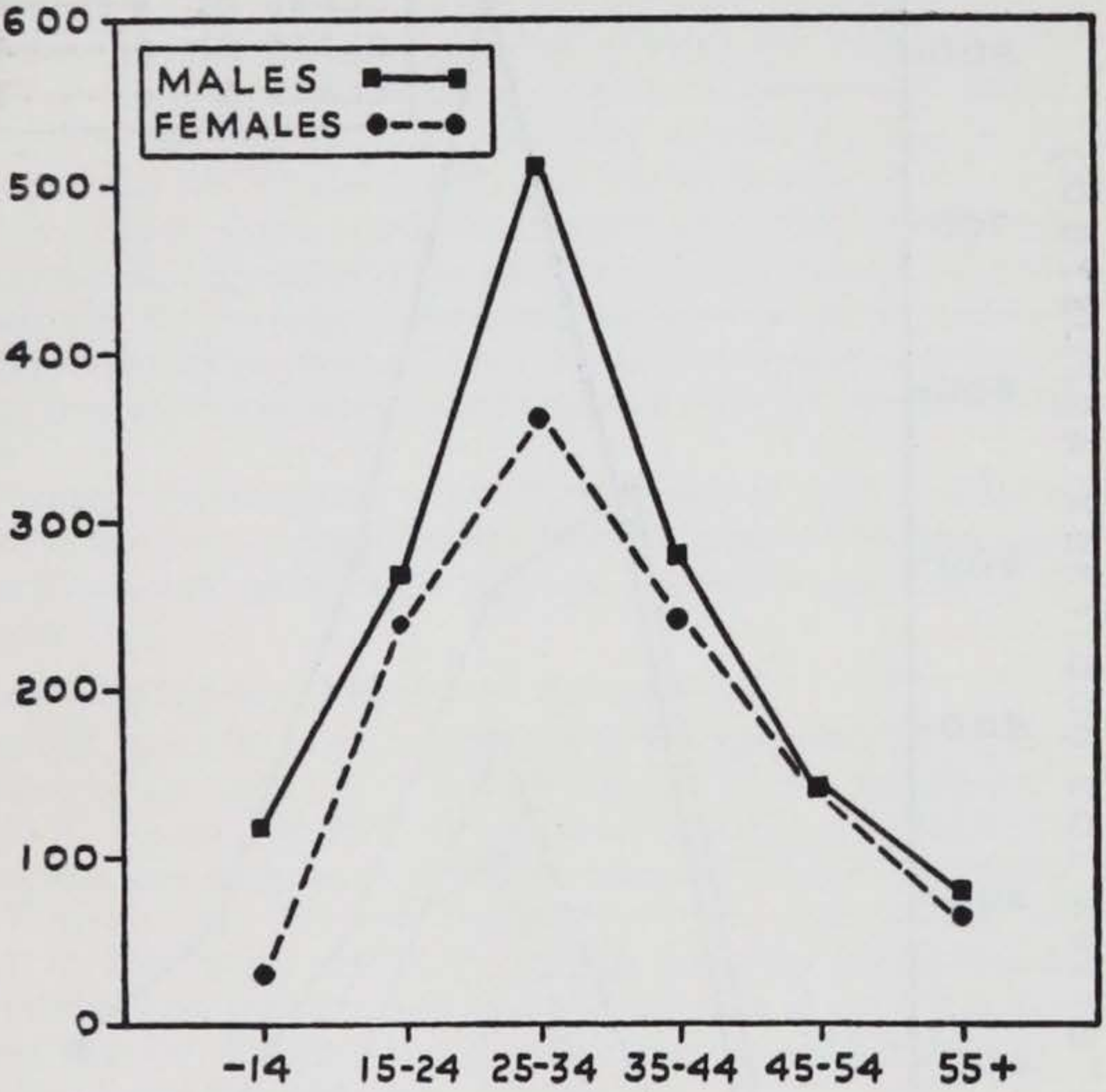
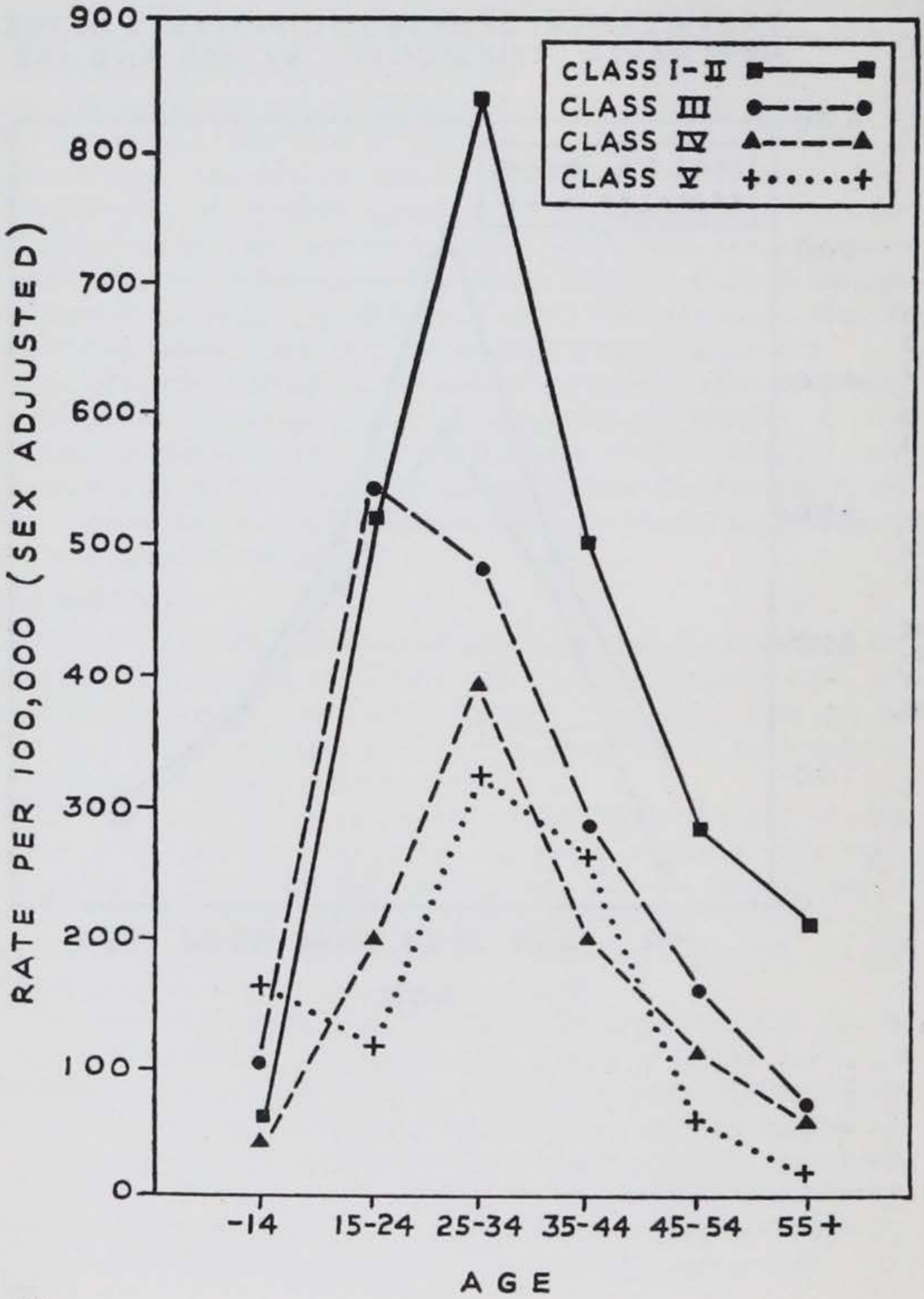


FIGURE 2

PREVALENCE OF NEUROTIC PATIENTS IN THE NEW HAVEN COMMUNITY BY AGE AND CLASS



rates of treated neurotic disorders differ significantly from class to class. The level of significance from one class to another is beyond .001.

Prevalence of Psychoses by Sex and Age.

The prevalence of treated psychotic disorders in the community's population by age and sex is depicted in Figure 3. The curve for females increases from almost zero at 14 years of age to 1,600 per 100,000 at 55 years of age and above. Males trace the same pattern, except that between 25 and 44 years of age the increase in rate is slight. After 44 years of age the rate increases rapidly and parallels that for females.

The age distribution of psychotic disorders is dramatically different from that of neurotic reactions. Among the neurotic patients the rate for both males and females is highest in the 25 through 34 age group; the rate is 510 for males, and 260 for females per 100,000 in these years. During the same age interval, the rate for psychotic disorders is 500 for females and 700 for males. Moreover, the rates for the prevalence of psychoses are only one-third as high as they are for females after 55 years of age; they are less than one-half the rate for males 55 years of age and older.

In passing, it is interesting to note that the rates for both neurotic and psychotic females are slightly below those for males at all ages except above 55 years of age when the psychotic rate for females exceeds that of males.

Prevalence of Psychotic Disorders by Age and Class.

We will turn now to an examination of the prevalence of psychotic disorders by age and class. The rates, adjusted for sex, are depicted in Figure 4. A perusal of this chart will show there are marked differences among the several classes from one age group to another. In Classes I and II the rate rises constantly from adolescence through the early adult years. It falls sharply from 25 through 44; it remains constant through the years of later maturity, and rises after 55 years of age. It is worth noting, however, that the rate is only 50 higher per 100,000 after age 55 than it is during the years 25 through 34.

The rate for Class III parallels that for Classes I and II until 34 years of age, but unlike Classes I and II, the rate in Class III gradually increases each decade. The rate for Class IV parallels the three higher classes until age 34, but it is higher in each age range. It levels off between 35 and 44 years of age, just as Class III does. It increases somewhat to age 54, then expands sharply throughout the remainder of the life span.

Class V exhibits a distinctly different curve in comparison with the

other classes. The rate is higher than in any other class at all age levels. The rate is low in the childhood years but in the 15 through 24 age range it rises to 303, and to 1,540 in the years between 25 and 34. Above 35 years of age the increase is in a straight line year after year as the population ages. However, we should note that the largest increase comes in the years between 15 and 34.

The widest differences in the prevalence of psychotic disorders from one class to another occur in the adult years. These differences increase with age, especially in Classes IV and V. After 55 years of age the prevalence of psychotic disorders by class is as follows:

Class	*Rate per 100,000*
I-II	434
III	638
IV	1,353
V	3,161

The great differences in prevalence of psychotic disorders above 55 years of age revealed by these rates call for further analysis. There is a definite inverse relationship between class status and prevalence in the New Haven community. But why?

COMPONENTS IN PREVALENCE

For our purposes, prevalence, to repeat, is the ratio of persons in the population who are under psychiatric care to the total population of the community during the months of June through November, 1950. In the preceding discussion no differentiation is made between patients who have been in treatment for a week and those who have been under psychiatric care for a number of years. When the data are viewed from the perspective of the differences in the length of time the patients have been in treatment, we find some patients entered treatment recently, others had been in treatment at one time, were discharged and re-entered treatment, and others who have been in continuous treatment for a varying number of years. The question we are now asking is this: Is the time when the patients enumerated in the Psychiatric Census entered treatment related to class status?

The schedule used in the Psychiatric Census was designed to give us an answer to such a question. We collected detailed information on each patient's illness; the date when he entered treatment for the first time, the duration of his first treatment, the date of his discharge, the date or dates of his re-entry into treatment, discharge a second or third time and so on. The information yielded by these questions enabled us to analyze the data by the length of time the patients had been in treatment.

FIGURE 3

PREVALENCE OF PSYCHOTIC DISORDERS PER 100,000 BY SEX AND AGE

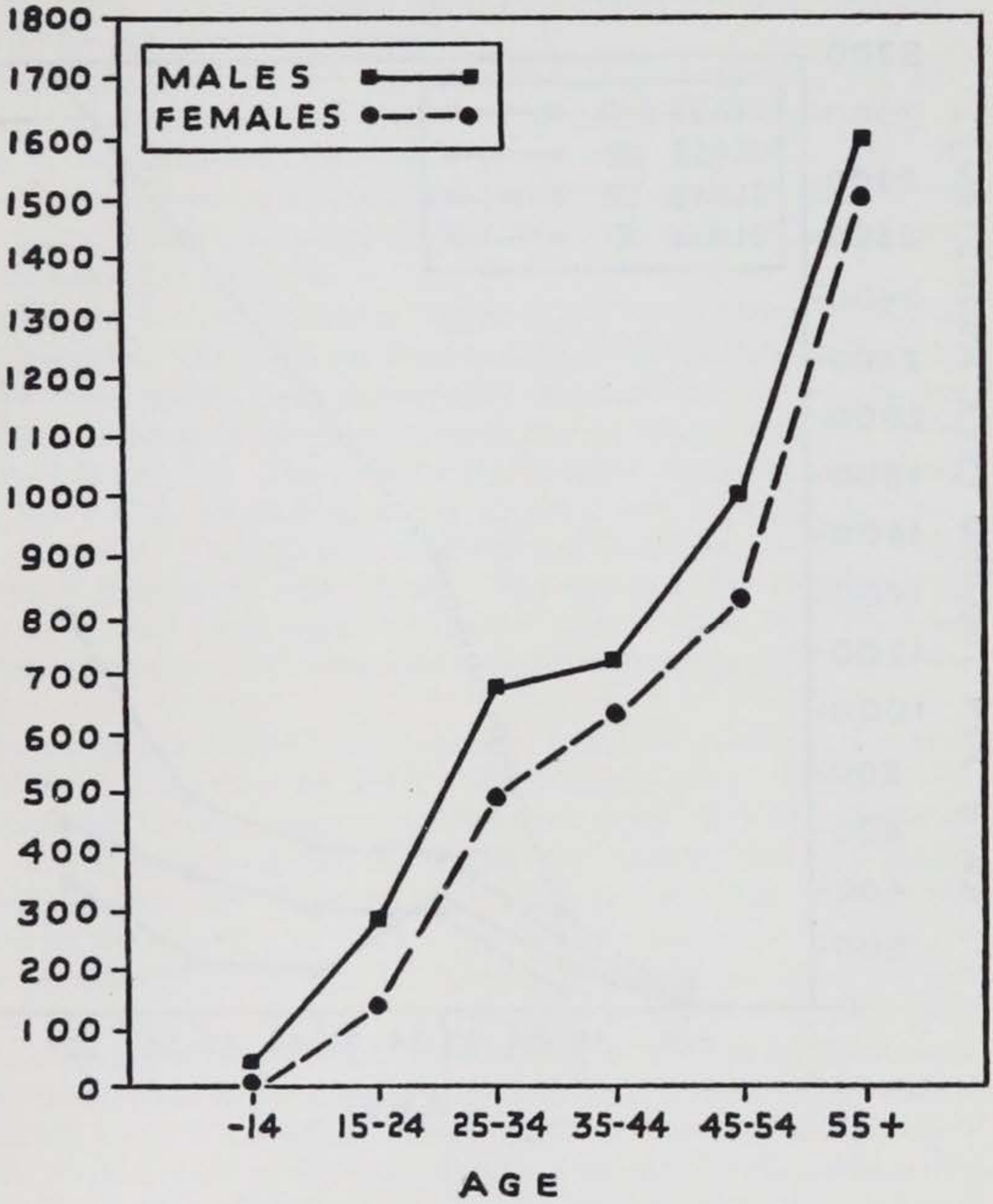
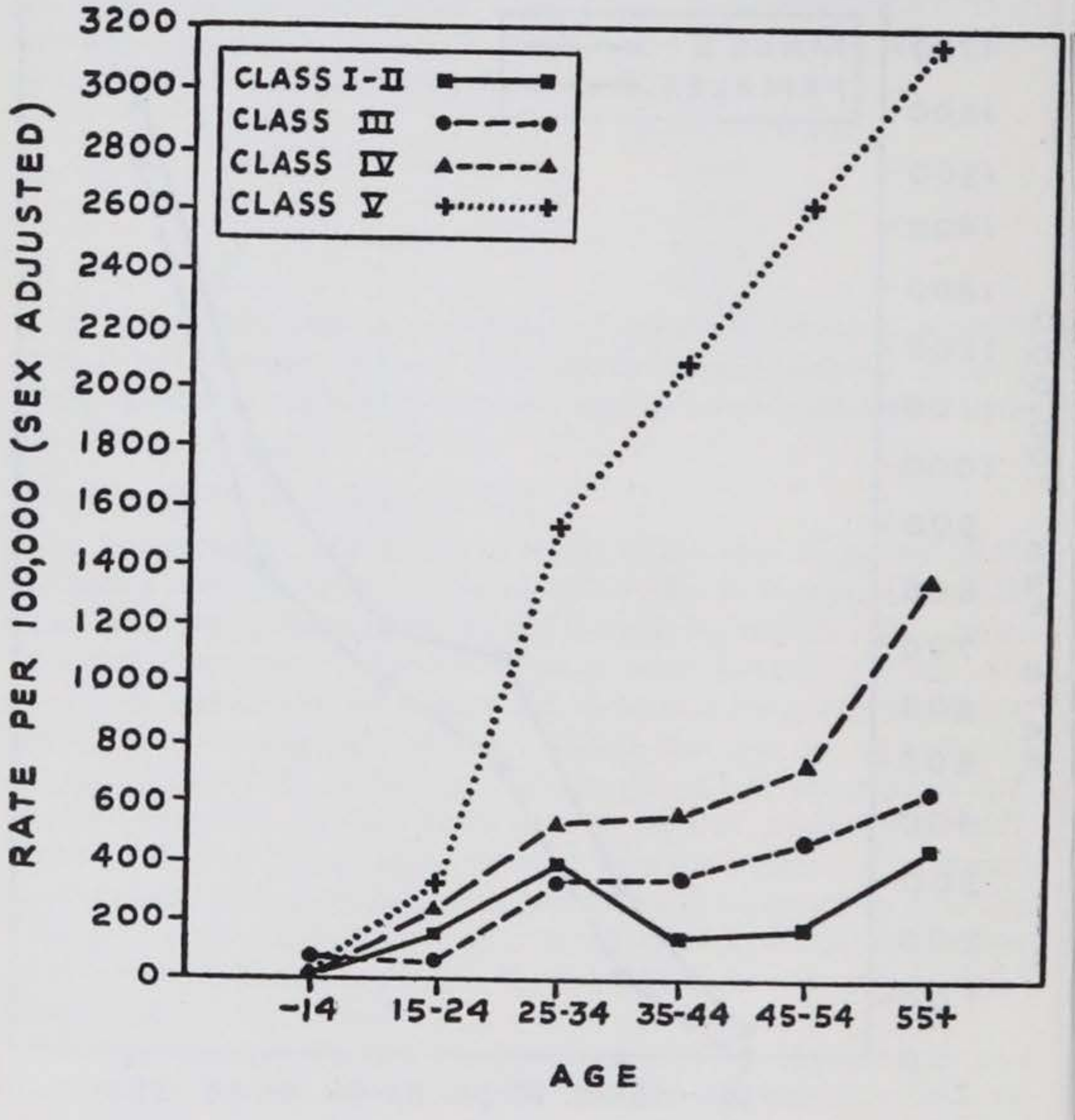


FIGURE 4

PREVALENCE OF PSYCHOTIC DISORDERS
BY AGE AND CLASS



Patients who were in treatment on May 31, 1950, and continued in reatment until December 1, 1950, are categorized as *Continuous* cases. ndividuals who entered treatment for the first time between May 31, 950, and December 1, 1950, are categorized as *New* cases. Individuals vho had been psychiatric patients at some previous time and re-entered reatment between May 31, 1950, and December 1, 1950, are counted as *Re-entry* cases. The Continuous, New, and Re-entry into treatment cases nake up the aggregate of *Total* cases.

The patients were divided into these three time categories; then sex- and age-adjusted rates were computed to determine if class status is inked to the duration of treatment. For present purposes no differen- iation is made between neurotic and psychotic diagnoses. In sum, we were dealing with all treated mental illnesses. The results of these analyses are summarized in Figure 5.

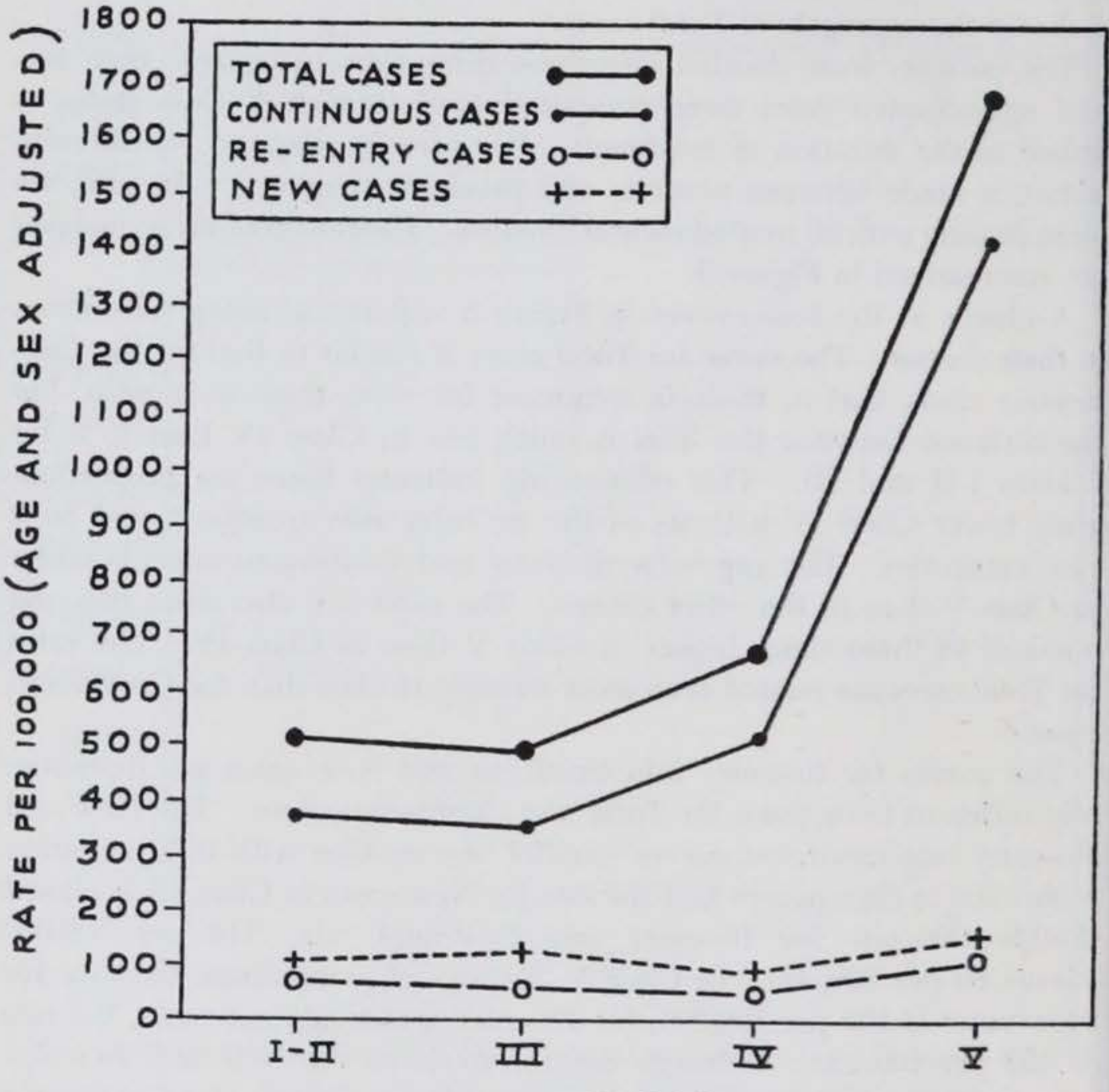
A glance at the four curves in Figure 5 will reveal sharp differences in their shapes. The curve for Total cases is similar to that for the Con- tinuous cases, that is, those in treatment for more than six months, but the distance between the lines is much less in Class IV than it is for Classes I-II and III. This relationship indicates there are proportion- ately fewer Class IV patients in the Re-entry into treatment and New case categories. The gap between Total and Continuous cases is wider in Class V than in the other classes. The rates are also some two and one-half to three times higher in Class V than in Class IV. The rates for Total cases are related even more strongly to class than the Continuous cases.

The curves for Re-entry into treatment and New cases are dramatic- ally different from those for Total and Continuous cases. The New and Re-entry into treatment curves parallel one another with little variation from class to class except that the rate for New cases in Class III is almost double the rate for Re-entry into treatment, viz., 114 per 100,000 versus 69 per 100,000. In Class V, by way of comparison, the rate for New cases is 139 per 100,000; for Re-entry into treatment cases, the rate is 123 per 100,000. Although the variations in the New and Re-entry rates from class to class are not as spectacular as the rates for Continuous cases, the differences are significant beyond the .001 level of probability.

Classes I and II contribute almost exactly the number of New cases as we would expect on the basis of their proportion of the community's population. Class IV has a lower number than could be expected pro- portionately, whereas Class V has an excess of 36 per cent. Class V is contributing disproportionately to the number of new patients entering treatment for the first time, as well as to the accumulation of Continuous

FIGURE 5

AGE AND SEX ADJUSTED RATES BY CLASS AND COMPONENTS IN PREVALENCE



cases. Moreover, Class V patients re-enter treatment in excessively large numbers. On a proportionate basis there is an excess of 61 per cent of Class V patients who re-entered treatment in the six months prior to the Psychiatric Census. Class IV, by way of contrast, has a deficiency of 15 per cent; Classes I and II have the expected number of Re-entry cases.

When the three components in the curve for Total cases are viewed both in relation to one another and to class status the differential in rates between Class I-II and Class V changes markedly. The rate for Continuous cases in Class V is 3.8 times higher than in Classes I and II, viz., 1,406 per 100,000 against 369 per 100,000. The differential for New cases from Classes I and II to Class V is less—97 cases per 100,000 in Classes I and II compared to 139 cases per 100,000 in Class V—or an increase of 43 per cent. The difference in rates for Re-entry into treatment between Classes I and II and Class V is likewise small—88 per 100,000 compared with 123 per 100,000; this is an increase of 39 per cent.

The varying magnitudes of these differences produce the gap between the Continuous and the Re-entry into treatment curves. The gap is slightly less for Class III than for Classes I-II, but the rate of Continuous cases increases sharply from Class III to Class IV and dramatically so from Class IV to Class V. The widening gap between the rates for Continuous and Re-entry into treatment cases indicates that *something is happening in the treatment of patients in the two lower classes which produces this difference.*

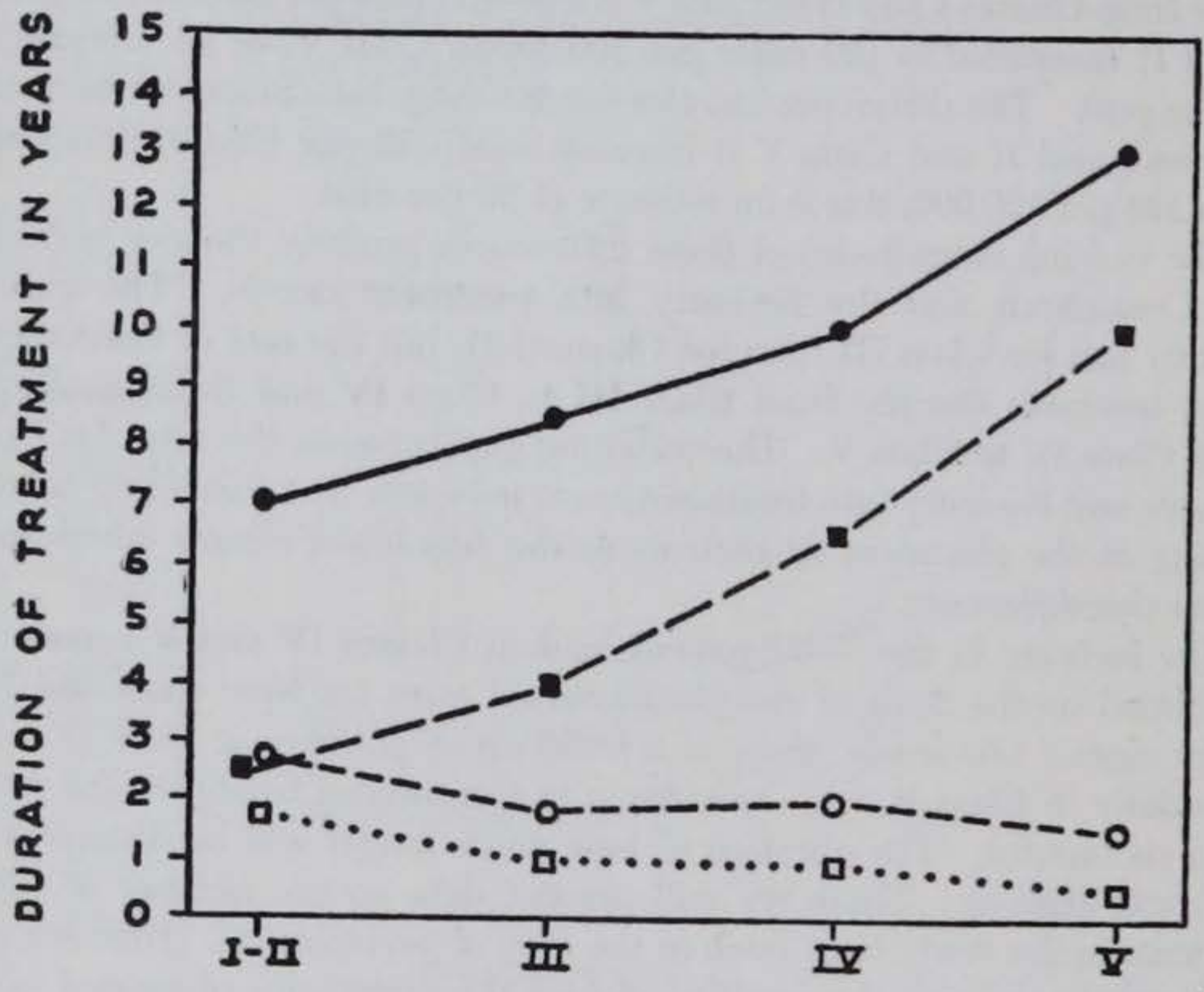
The increase in the Total patient load in Classes IV and V cannot be explained on the basis of sharply increased rates for New cases and Re-entry cases. Obviously, there is a build-up of patients in Class IV, and especially in Class V, who have been in Continuous treatment for more than six months. The question of how much longer will be discussed in the next analysis. There we will present data on the number of years patients in the study have been in the care of psychiatrists. Here we are concerned only with the question of how the prevalence of treated cases is related to class from the perspective of its principal components; cases in Continuous treatment for more than six months, cases that entered treatment within the six months immediately preceding the Psychiatric Census and those that re-entered treatment in this interval.

CLASS AND YEARS IN TREATMENT.

The mean and median number of years neurotic and psychotic patients have been in their present course of treatment are shown in Figure 6. The data summarized here reveal that the number of years patients have been in treatment is linked to class status. Among neurotic reactions, the higher the class the longer the patients have been in treat-

FIGURE 6

THE MEAN AND MEDIAN NUMBER OF YEARS PSYCHOTIC AND NEUROTIC PATIENTS HAVE BEEN IN THEIR PRESENT COURSE OF TREATMENT



ment. Among psychotic reactions, the lower the class the longer the patients have been in treatment. A glance at the curves symbolizing the mean and median number of years the neurotic and psychotic patients have been in continuous psychiatric care will indicate that the effects of class position are starkly real whether the mean or median is used. However, these measures tell us different things.

The median for the psychotics discloses that in Classes I and II, 50 per cent of the patients have been in treatment less than three years, whereas in Class III the comparable figure is four years; in Class IV, 6.5 years; and in Class V, 10 years. In sum, one-half of the Class V patients have been in treatment almost four times as long as the comparable 50 per cent of the Class I-II patients. The other 50 per cent have been in continuous psychiatric care longer than the medians given in Figure 6. This is evident from the larger means for each class. For example, the Class I-II psychotics have a mean of seven years. This is more than twice as long as the median for this group. This difference indicates that a certain proportion of the Class I-II psychotic patients have been in treatment for many years, but the remainder have been under psychiatric care for a relatively short time. The differential slopes of the mean and median below the Class I-II level reveals that in the lower classes, particularly in Class V, once a patient is diagnosed as psychotic and is committed to a state hospital he tends to remain there. This lamentable fact produces the high mean and median for Classes IV and V.

Some readers may jump to the easy conclusion that our figures on duration of treatment are no longer valid because the Psychiatric Census was taken before the era of tranquilizing drugs. Shortly after the Psychiatric Census was completed we selected a random sample of 100 state hospital patients in treatment for the first time for follow-up purposes. These patients were followed until March 1, 1956, or for five years and four months after the census was taken. Fifty-four of the 100 were still in hospital on March 1, 1956; 30 had died in the hospital; and 16 had been discharged between the date of the "Psychiatric Census" and the end of the field work. The 54 patients who were still in hospital five years and four months after the census date had been there a long time. When the follow-up stopped, the mean number of years these patients had been in continuous psychiatric care was: Class I through III, 14; Class IV, 18; Class V, 22.5. The experiences of these patients during the present years of the "tranquilizing era" do not lead to the conclusion either that the new drugs are "emptying" the state hospitals or that the class differences in duration of treatment are growing shorter with the passage of the years.

The duration of treatment curves for the neurotic patients are not as dramatic as those for the psychotic patients. Nevertheless, the differences in the length of time the neurotics have been under psychiatric care are highly significant. The mean in Classes I-II is 33 months. It declines rather consistently to 18 months in Class V. The median shows greater variation from one class to another; it is 23 months in Classes I-II, but only 6 months in Class V. The differences between the mean and the median measure the tendency of patients in the several classes to remain in treatment or to drop out of it. In Classes I-II, the neurotic patients remain in treatment much longer than in Classes III and IV. The vast majority of the Class I-II neurotic patients are in ambulatory treatment with private psychiatrists. On the other hand, the Class V neurotics are either clinic patients or they have been committed to the state hospital. Those treated in the clinics tend to drop out of treatment shortly after they begin, but the neurotics in the state hospital tend to be retained in hospital indefinitely. These counter trends produce the relatively large amount of disparity between the mean and the median figures for neurotic reactions below the Class II level.

REFERENCES

[1] Kenneth Appel, "Present Challenge of Psychiatry," *American Journal of Psychiatry,* Vol. 111 (1954), pp. 1-12.

[2] Paul Lemkau, Christopher Tietze, and Marcia Cooper, "A Summary of Statistical Studies on the Prevalence and Incidence of Mental Disorder in Sample Populations," *Public Health Reports,* Vol. 5 (No. 53, December 31, 1943).

[3] For a review of the literature pertinent to this point see "The Concept of Health in Psychiatry" by Fredrick C. Redlich in *Explorations in Social Psychiatry,* Alexander Leighton, John Clausen, and Robert Wilson, editors. To be published by Basic Books, Inc.

[4] For published reports on the several aspects of this problem see the following: Frank Auld, Jr. and Jerome K. Myers, "Contributions to a Theory for Selecting Psychotherapy Patients," *Journal of Clinical Psychology*, Vol .X, January, 1954, pp. 56-60; August B. Hollingshead and Fredrick C. Redlich, "Social Stratification and Psychiatric Disorders," *American Sociological Review,* Vol. 18, April, 1953, pp. 163-169; A. B. Hollingshead and F. C. Redlich, "Schizophrenia and Social Structure," *American Journal of Psychiatry,* Vol. 110, No. 9, March, 1954, pp. 695-701; August B. Hollingshead and Fredrick C. Redlich, "Social Class and Psychiatric Disorders," *Interrelations between the Social Environment and Psychiatric Disorders,* Milbank Memorial Fund, New York, 1953, pp. 195-208; August B. Hollingshead and Fredrick C.

Redlich, "Social Stratification and Schizophrenia," *American Sociological Review*, Vol. 19. No. 3, June, 1954, pp. 302-306; Jerome K. Myers, "Note on the Homogeneity of Census Tracts: A Methodological Problem in Urban Ecological Research," *Social Forces*, Vol. 32, May, 1954, pp. 364-366; Jerome K. Myers and Bertram H. Roberts, "A Sociological–Psychiatric Case Study of Schizophrenia," *Sociology and Social Research*, Vol. 39, September-October, 1954, pp. 11-17; Jerome K. Myers and Leslie Schaffer, "Social Stratification and Psychiatric Practice: A Study of an Out-Patient Clinic," *American Sociological Review*, Vol. 19, June, 1954, pp. 317-320; F. C. Redlich, "The Concept of Normality," *American Journal of Psychotherapy*, Vol. 6, 1952, pp. 551-576; F. C. Redlich, "The Influence of Environment on Mental Health," *Bulletin of the New York Academy of Medicine*, Vol. 30, No. 8, August, 1954, pp. 608-621; F. C. Redlich, A. B. Hollingshead, B. H. Roberts, H. A. Robinson, L. Z. Freedman, and J. K. Myers, "Social Structure and Psychiatric Disorders," *American Journal of Psychiatry*, Vol. 109, April, 1953, pp. 729-734; Bertram H. Roberts and Jerome K. Myers, "Religion, National Origin, Immigration, and Mental Illness," *American Journal of Psychiatry*, Vol. 110, April, 1954, pp. 759-764; H. A. Robinson, F. C. Redlich, A. B. Hollingshead, "An Investigation of Social Structure and Psychiatric Disorder," *The American Psychologist*, Vol. 7, 1952 p. 348 (Abstract); H. A. Robinson, F. C. Redlich, and J. K. Myers, "Social Structure and Psychiatric Treatment," *American Journal of Orthopsychiatry*, Vol. 24, April, 1954, pp. 307-316; Leslie Schaffer and Jerome K. Myers, "Psychotherapy and Social Stratification: An Empirical Study of Practice in a Psychiatric Out-Patient Clinic," *Psychiatry*, Vol. 17, February, 1954, pp. 83-93; F. C. Redlich, A. B. Hollingshead, and E. C. Bellis, "Attitudes Toward Mental Illness and Psychiatric Treatment," *American Journal of Orthopsychiatry*, Vol. 25, No. 1, January, 1955, pp. 60-70; A. B. Hollingshead and F. C. Redlich, "Social Mobility and Mental Illness," *American Journal of Psychiatry*, Vol. 112, No. 3, September, 1955, pp. 179-185; B. H. Roberts and J. K. Myers, "Schizophrenia in the Youngest Male Child of the Lower Middle Class," *American Journal of Psychiatry*, Vol. 112, No. 2, August, 1955, pp. 129-134; A. B. Hollingshead, R. Ellis, and E. B. Kirby, "Social Mobility and Mental Illness," *American Sociological Review*, Vol. 19, No. 5, October, 1954, pp. 577-584; J. K. Myers and Frank Auld, Jr., "Some Variables Related to Outcome of Psychotherapy," *Journal of Clinical Psychology*, Vol. 11, No. 1, January, 1955, pp. 51-54.

[5] The New Haven community includes the city of New Haven, and the towns of West Haven, East Haven, North Haven, and Hamden. The population of the community was some 240,000 in 1950.

[6] F. C. Redlich, B. H. Roberts, and L. Z. Freedman.

[7] August B. Hollingshead and Jerome K. Myers.

[8] Harvey A. Robinson.

[9] See A. B. Hollingshead and F. C. Redlich, "Social Stratification and Psychiatric Disorders," *American Sociological Review*, Vol. 18, No. 2, April,

1953, pp. 163-169, for a statement of the hypotheses tested in this research.

[10] Ecological area of residence is measured by a six-point scale; occupation and education are each measured by a seven-point scale. To obtain a score on an individual we must know his address, his occupation, and the number of years of school he has completed. Each of these factors is given a scale score, and the scale score is multiplied by a factor weight determined by a standard regression equation. The three factor scores are summed, and the resultant score is taken as an index of this individual's position in the community's social class system. The development of this scale will be described in detail in a forthcoming book by A. B. Hollingshead and F. C. Redlich on *Psychiatry and Social Class*.

[11] Research by sociologists has demonstrated that the population of the New Haven community is stratified into distinct classes. See Maurice R. Davie, "The Patterns of Urban Growth," *Studies in the Science of Society*, G. P. Murdock, ed., New Haven, 1937, pp. 133-161; John W. McConnell, "The Influence of Occupation Upon Social Stratification," (unpublished Ph.D. thesis, Sterling Memorial Library, Yale University, 1937); A. B. Hollingshead, "Trends in Social Stratification: A Case Study," *American Sociological Review*, Vol. 17, No. 6, December, 1952, pp. 679-686; Ruby J. R. Kennedy, "Single or Triple Melting-Pot? Intermarriage Trends in New Haven, 1870-1940,"*American Journal of Sociology*, Vol. 39, January, 1944, pp. 331-339; Jerome K. Myers, Jr., "The Differential Time Factor in Assimilation: A Study of Aspect and Processes of Assimilation Among the Italians of New Haven," (unpublished Ph.D. thesis, Sterling Memorial Library, Yale University, 1949); Mhyra S. Minnis, "The Relationship of Women's Organizations to the Social Structure of a City," (Ph.D. dissertation, Yale University, Sterling Memorial Library, 1951).

[12] *Psychiatric Disorders and Reactions*, Washington: Veterans Administration, Technical Bulletin 10A-78, October, 1947.

[13] After our data were collected, the Veterans Administration's nomenclature and category system was superseded by an outline developed by the American Psychiatric Association. However, there is strong similarity between the Veterans Administration classification and the outline used currently by the American Psychiatric Association.

[14] Two different ages are used in these analyses. The present age is used for the control population. The age when the patient entered treatment is used for the patient population.

DISCUSSION

Dr. Top: Thank you, Dr. Hollingshead, for a very fine paper and presentation. From an epidemiological standpoint, I think everything possible has been done, in accordance with our present knowledge, to cover contingencies which might lead to false estimates and results in terms of the way we interpret them.

I should like to ask Dr. Berlien to open the discussion.

Dr. Berlien: I think we all have been tremendously impressed—I know I have—by this really fine and scholarly presentation. This is a piece of work for which we're going to be grateful to Dr. Hollingshead and his colleagues for a long time to come; and the publication of these findings is going to bring about, I hope, a greater alertness to those very meaningful statistics that he found.

I should like to say that I wonder if it would be of importance epidemiologically to study the incidence or prevalence in connection with the number of available psychiatrists for private treatment of patients and the number of psychiatric beds or other facilities available for public cases. The reason I bring this out is that at a conference down in the deep South on mental health and education a year ago last February we learned, as the conference went on, that there were in some states no child psychiatrists whatever. There was no point in trying to take children for care because care simply wasn't available. I should suspect, therefore, that a census would be markedly lower in that setting simply because psychiatrists and psychiatric facilities aren't available; and I am told, moreover, that this situation also prevails for psychotic cases in hospitals—they're short of beds.

Secondly, I would wonder about the change in the way we handle our aged. In years gone by the aged, even though they got to be quite queer—psychotic, by our standards—were kept at home; now they go to the hospital.

Thirdly, relative to the question of how we diagnose, I want to tell you of a fantastic experience I had as a consultant to the headquarters of the Western Pacific Command during World War II. During the Leyte campaign I visited every division we had in combat, and I came upon one amazing situation, which, incidentally, I reported on in a paper I read in Los Angeles two years ago. Because of the marked variance in philosophy in two divisions located on either side of a mountain, it so happened that if a combat soldier wandered down one side of the mountain—the side where the division was oriented to recognize mental illness—and got into a first aid station and later into a field hospital or something, he was classified, if his condition warranted it, as a mentally ill soldier and he went back through the line of evacuation tagged as mentally ill. On the other side of the mountain, the philosophy of the other division was "Soldier, you're gonna fight. You're not gonna get out uh fightin' because you've got one of these fancy psychiatric diagnoses tagged on you!" Any man that got evacuated on that side of the mountain—he was evacuated just the same—got a different diagnosis. He had an ulcer of the stomach or asthma or something else, but *not* a psychia-

tric condition. Obviously, a census taken there would have shown radical and startling differences!

Fourthly, I'd like to say that the census, in my opinion, particularly as regards that group which were found to be patients in state hospitals, should make a distinction among those who were reported in continuous treatment. Undoubtedly many should be recorded as being in continuous *non*-treatment.

We will now open this up for general discussion.

Dr. Levitt: Dr. Hollingshead, I want to ask you a question. It seems to me that you have a built-in bias in results that you expect to gain from the study, and that the work you have done shows not prevalence of the disease but rather prevalence of treating patients. This bias shows up most strikingly in the contrast of the relationship of incidence to social and economic status; that is, you have a peak number of prevalence of neurotics in the higher social and economic groups, among those best able to afford private practitioners, and you have the peak prevalence of psychotic illnesses among the lower socio-economic groups, principally those in state hospitals, those least able to afford anything but non-paying, community-residence type of treatment. I apologize for the word "treatment" since it usually turns out to be residential care.

Dr. Hollingshead: We have published about twenty-five professional papers on this project. In every one we have underlined "treated," and we have emphasized that we are dealing here only with *treated* cases. But almost always this question comes up and we are accused of writing bias into our project. I think, if you will look at the paper, I have emphasized "treated."

Now to refer to Dr. Berlien's comment about cases not in treatment. We have run into that before, too. But if you say that cases in state hospitals are not in treatment, you are accusing the state hospital people and the state hospital system of biases and loaded behavior. We once made that mistake in the early days of presentation and we were right soundly sat upon, not only by state hospital people but by social workers and so on. So we now refer to them as custodial care cases. A lot of people don't like that either, but it's the best we can do.

So far as going back to your first point is concerned, we do have in the New Haven community two child psychiatric clinics; but we still don't get too many children in treatment.

I think I will comment further on Dr. Levitt's point, as I believe that I am as aware as anyone in this room of the need for a real epidemiological study, or what I called in my introductory remarks "true prevalence" or "endemic prevalence." There I tried to sketch in one paragraph some of the difficulties you encounter when you try to design a

study like this, let alone attempt to carry it out. At the present time I am actively engaged—and have been for the past year—in designing and planning a study which I hope will be endemic and show true prevalence in a population. It will be a probability sample where we will study the individual and his family sociologically and psychologically. We will also take an individual out of this family and bring him into our research center where he will receive a battery of projective tests. We will have psychiatric social workers who will gather data on him—life history data—and then the psychiatric examination. I think we can do this, but we are going to have many heartaches and headaches before we finish. This, in my judgment, is one of the next steps in research, and I hope that Dr. Yarrow tonight mentions this need.

One thing to remember is that we started this study ten years ago, and I might say that we had built into it some of the things Dr. Spiegel and his group are doing today in Harvard's Department of Social Relations, such as their study of families and so on. We were told then that this was nonsense, that we wouldn't receive support, and that no institution was going to have anything to do with a lot of nonsensical, sociological research. "Cut it out of your design," they said, "if you expect to get financing."

Dr. Spiegel: I know there are many subtle aspects of Dr. Hollingshead's study which are impossible to put in, in the length of time he had in which to present his material; but there is one on which there has been some published papers which I'd like to hear more about. This refers to the fact that the difference between the length of time of treatment and the various classes is not only a function of the state hospital situation and the matter of who can afford care but is also a matter of the difference in values and capacity of people in various class levels to engage in psychotherapy and the type of interchange that is necessary to psychotherapy. I wonder if Professor Hollingshead could say a word about that.

Dr. Berlien: I think because the time is growing short, we will let some of these questions accumulate and then let Dr. Hollingshead comment on them at the close of the discussion. I should like to recognize a very distinguished journalist in our midst who wants to say something.

Mr. Deutsch: I should just like to say that I think Dr. Hollingshead's group study has been extremely valuable, and that I have little patience with people who expect perfectionism in the first pioneer attempts at the epidemiology of mental health. I should like to make a plea to Dr. Hollingshead to emphasize more the availability of resources as affecting these epidemiological studies. Dr. Berlien brought up this question of resources in the South which reminds me of a very interesting period

in American history when the first census of the insane in the United States in 1840 became an important propaganda agent for the pro-slavery advocates of the period. This census indicated that there was ten times as much prevalence of mental disease at that time among the Negroes of the North as among the Negroes of the South. The slavery advocates took this as a demonstration that the Negro was unfit for the burdens of civilized freedom and that slavery was their natural state. Of course it turned out that one of the major reasons for this phenomenon was that there were no institutional provisions for Negro insane in the South. Most of the institutions they had at the time were for whites only; and, in any case, slave masters were not going to put a slave mental patient in a hospital while he could do any kind of work at all on the farm.

Another kind of problem in our time is how a mental patient is processed to a psychiatric facility. It is my guess that there is quite a skewing because a lot of kids who are picked up as juvenile delinquents come to the attention of social agencies and a large proportion are shunted to some kind of mental clinic (where such clinics are available), while the middle-class delinquents who are the neurotic kids do not get discovered in this way.

I would say, too, as a lay student of the mental hospital problem in this country, that I would also underscore the point—in spite of the fact, Dr. Hollingshead, that you might irritate a number of state hospital heads—that has been made by yourself and others that the talk of treatment in most of our state hospitals today in terms of many of the patients is misleading to the public as far as the differentiation between cold storage for human beings and treatment for mental patients is concerned.

Dr. Vaughan: I think Dr. Hollingshead has done us a great favor today to present his material in such productivity and give it to us to chew on. Some of us who have been chewing on this material now for a number of years and have had the privilege of watching this development have been struck more and more by the basic importance of this study, which throws more light on the old problem of how to identify and deal with what we think of as psychiatric disorders in people who are actively engaged with professionals under the banner of psychiatry—be it on the couch or in cold storage. So I think the point of prevalence is really not a function of the natural history of the disease but a manner of coping with the disease, which should be stressed here a little bit more than it has been in the past. This is contrasted, perhaps, to incidence, the study of which will give more information as to the secondary nature of the disease itself.

For instance, to make a comparison with the slide showing age-sex distribution of treated cases in New Haven: When we did a study in

Massachusetts on the use of psychiatric facilities in the community, approaching it from the point of view of use, our age-sex curve looked just the same except that we had a bimodal characteristic lagging, mainly in the child guidance clinic. Interestingly enough, the high proportion of men in the 25 to 35 age group which showed up here also showed up in our studies. This is directly related to the fact that the Veterans Administration has developed facilities and is catering to people in those age groups.

What is our whole concept of medical care on a psychiatric basis? An interesting thing would be to consider—if you wish to call them psychiatric cases—the cases which are being dealt with by nonmedical facilities and, of course, the curves would be quite different. In our study we were struck by a tremendous lack of cases in the teenage group, which simply meant not that there were fewer teenagers with trouble but only that very few teenagers made use of psychiatric clinic facilities in Massachusetts. However, if we had included in our study certain private programs currently being developed in various universities in Massachusetts and had included only those cases of Massachusetts residence who were being cared for in those institutions, there would be hundreds more cases and naturally the curve would look quite different. I'm sure that this point is important.

Dr. Berlien: I'm going to have to comply with the law now and ask Dr. Hollingshead to close this meeting.

Dr. Hollingshead: I wish Dr. Berlien had complied with the law before he got this last request!

Now there are a large number of points which were made. I don't think, Dr. Spiegel, I'll have time to answer your question in detail. We have discussed this before and will probably discuss it again. All I can say is that we have at least two chapters in the book where we go into this whole question.

I'd like to close with some remarks about the problem of "availability," which has been raised several times. The question is—"available to whom?" We can demonstrate pretty definitely that the position of the potential patient in the social system is going to determine what kind of psychiatric facilities are available to him. You can have exactly the same kind of behavior in different segments of the social structure of the New Haven community and in Class I it will be perceived in one way and in Class IV it will be perceived another way and in Class V still a different way. We have a chapter we call "Paths to the Psychiatrist," and we show how any individual who eventually reaches the psychiatrist goes through four milestones as he follows his paths down to the psychiatrist. First, there has to be the abnormal behavior, and then there

has to be the perception of this behavior as being abnormal. Then there must be an evaluation of this perceived behavior as either delinquent or disturbed. And here you get into some wonderful arguments and different evaluations by professional people. The lawyer and the minister perceive abnormal behavior differently from the psychiatrist. The psychiatrist may say the subject's behavior is "disturbed" and that he should be treated by a psychiatrist for his emotional involvement rather than receive punishment; but whether he is punished or whether he is treated, will depend, in large part, upon where he is in the social structure.

You have, then, this question of appraisal; and then the decision has to be made on whether the individual should be treated and then you have the problem of implementation; that is, who is "available" to whom and what kind of care is "available" under the circumstances. Before an individual who has performed the disturbed behavior may be perceived as such, a decision may be made; but unless the individual had reached a psychiatrist, he was not included in our study. We are concerned here with only that narrow group of clinical cases; and, as Dr. Top can tell you, epidemiology, experimental medicine, or clinical medicine—each starts with a clinical case. Then you work outward, as your knowledge progresses, from the clinical case to inferences about the clinical case.

Here may be where the laboratory man can come in and it may be that psychochemistry will give us the real break-through—or it may be the type of work that Dr. Spiegel, Dr. Kluckhohn, and the group at Harvard are doing, or it may be some of the meager things we're trying to do down our way which will lead to new insights on the social side.

In closing, I should like to emphasize the point that although ours is a *treated* prevalence—and we have been reminded that we have this built-in idea of prevalence—nevertheless, if we can demonstrate in our study in a cross-section of patients in a given period of time what the social system is doing to those patients, irrespective of their disorder, I think we have made a contribution that's worth the effort we have put into this. For instance, to take one specific syndrome, the organics, we have an age- and sex-corrected rate of the organic psychosis which is 28 times as high in Class V as it is in Classes I and II combined. This is putting a new dimension into our understanding of psychiatric disorders. Thank you very much.

[The following additional comments on Dr. Hollingshead's speech were received by letter. EDITOR.]

Dr. Blatz: It was such a relief to find presented to us so clearly the data collected over the five-year period of investigation on "prevalence and incidence" of mental illness. There has been so much loose statistical

nonsense thrown around that I was fascinated at the ingenuity shown by the speaker and his tables. However differently the data may be interpreted by various experts in many fields, at least there is available some concrete data, carefully collected and collated and upon which we may depend. I am looking forward to the publication of this material. There seems to be a suggestion that some light may be thrown on the etiology of mental illness. There was too little time to go into these intricacies but enough to indicate the richness of the material.

Dr. Luckey: (1) In Dr. Hollingshead's division into classes, he has left the largest bulk of society in Classes IV and V and I wonder what significance that has in our study of social factors. There may be other breakdowns that might give a clearer picture of the situation.

(2) There is a problem entering into the consideration of social factors that needs to be reckoned with. Representing as we do some of the problems to be found in a metropolitan area, one of the most serious situations that we meet is the mobility of population. Since World War II we have had a tremendous increase in the mobility of population. Each war or fluctuation in economics has led to a shift in our population; but there has been a very much accelerated shift in these last few years, not only into the metropolitan areas but shifts from one neighborhood to another. In certain of our schools more children are moved in and out during the year than the total enrollment of the school. This means that the teacher is confronted with a constantly changing group and does not have a long time to get acquainted and build up healthy emotional reactions. When one considers the very important role that schools play in preventive psychiatry, one does have to face this problem realistically.

(3) In the overcrowding in certain sections of the city another problem is arising. We have an increase of submarginal housing, even while slums are being cleared away. There is a break-up of the neighborhood influences. The effect of this rapid change from neighborhood to neighborhood has never been measured that I know of. To be sure, there is a greater mobility of all population groups. Along with this very mobile society, we also have an increase in the form of both parents working. We have few of the previous controls or influences. What has this done to the problem of a stable home or the development of the feeling of security in children?

(4) The shift in neighborhoods is so rapid that it must play a very important role in the child's development of peer relationship. These peer groups show rapid fluctuation in membership. It is possible that in order to establish themselves in the new neighborhood, the children must try to be more spectacular in their behavior in order to achieve status in the

new group. As a result, children may seek attention by methods that previously would not be used or that would be frowned on.

Do some of our problems of juvenile delinquency rise from this attempt by the children to gain status?

It is also possible that the new child is exploited by the more recent members of the group. A study of "pecking systems" in this peer relationship, especially in transient urban sections, might be of importance.

What is the value of neighborhood councils and social agencies working in close proximity to the home neighborhood?

Does the very mobility of the pupil prevent the building up of useful contacts and the strengthening of the social environment for these children?

INTRODUCTION TO CHAPTER IV

Dean Peterson: It's a pleasure to have the privilege of substituting for Dr. Whitehead although we are all disappointed that she was unable to be here with us.

Last evening I picked up the April issue of *Harper's* and I read these words in an article by Dr. Jerome D. Frank:

> Never before in human history has there been so much agonized concern over the raising of children—or such a pervasive feeling of parental inadequacy and guilt—as there is in America today. Once all a parent had to do—when he was faced with a disturbing bit of child behavior—was to recall how he himself was handled at the same age and repeat the treatment. Today there is far more apt to be a harried search through the pages of one or more of the countless books and articles on child guidance that have flooded the popular press, a desperate attempt to understand why the child has done whatever he has done—a nagging sense that, whatever the reason, the parent is somehow to blame and may unwittingly make the whole situation worse if he fails to react properly.*

All of us have seen the precepts that are looked on with horror today returned to favor tomorrow. In one brief span of time, the doctrine of rigid child training, and what has been termed antiseptic neglect, went down before the onslaught of unconditional love, only to re-emerge as the necessary setting of limits.

In this dilemma, we turn this afternoon to recent research for some suggestions, some clues and, conceivably, some answers. Our speaker, Dr. Brim, is unadulterated blue—B.A., Yale; M.A., Yale; Ph.D., Yale. He was also a bit of blue in the service when in the Air Force. He has held distinguished research appointments with the Social Science Research Council and with the Committee on Child Health of the American Public Health Association. He has had an academic career at the University of Wisconsin as an assistant professor of sociology. He has served as a staff member on the research project, Cultural Factors in Talent Development, at Yale University. He was a technical research analyst with the New York City Youth Board and currently is Project Director of Social Science and Parent Education with the Russell Sage Foundation. It is a pleasure to have you here at this conference, Dr. Brim.

* Frank, Jerome D., "Are You a Guilty Parent?" *Harper's*, April, 1957, Vol. 214, No. 1283, p. 56. Quoted by permission of Harper & Brothers, New York.

CHAPTER IV

*Recent Research on Effects of Education In Human Development**

ORVILLE G. BRIM, JR., PH.D.

Our purpose this afternoon is to review studies of the relation between educational programs transmitting human development materials and the increase in mental health of children.

By educational programs I mean those whose influence is achieved through, and which are directed to, the motivational and belief systems of the individual which are under his conscious control. To my mind this contrasts with therapeutic programs where the primary emphasis is on working with those motives and beliefs of the individual of which he is unconscious.

I include in the review only those research studies of reasonably sound experimental design which employ statistical or other acceptable procedures in the analysis of results. This excludes, therefore, purely descriptive evaluations of programs, testimonials, single case histories, and so on.

The review is limited to studies of three varieties of educational programs: First, there are those which seek to educate parents with the aim of promoting mental health of their children. While such programs may have subsidiary aims for the child, such as his physical health, and while they may have other aims which are instrumentally achieved on the way, so to speak, to reaching the child, such as promoting mental health in the parent also, the dominant goal of such programs is the child's mental health. The second type of program is similar to that for parents, but differs in that the educational effort is directed to the teachers of children. The third type of program seeks to improve the child's mental health through educating *him* directly in human development materials, rather than working through the child's parents or teachers.

Before turning to the research data itself, let me present a brief overview of the scope of educational programs of this kind and of the evaluative research which is available.

* This study is part of the project, "*Social Science and Parent Education,*" sponsored by the Russell Sage Foundation and the Child Study Association of America.

Considering parents first, a conservative estimate of the number of parents reached regularly, month by month, by individual counseling, study groups, and mass media is over ten million persons. Parental education programs are found in federal, state, and local governments, in the work of more than thirty major national organizations including school, church, medical, home economic, and other groups, and in an unnumbered host of local organizations.

In regard to teachers, all know that during the course of regular training for their profession most teachers with college degrees have received systematic education in human development materials. In addition there are many extensive and well known special programs, usually at a postgraduate level, specifically designed to train teachers in human development materials for the primary purpose of improving the emotional well-being of the children in their classrooms. Perhaps the major program of this special type, for example, is that formulated by the Institute of Child Study at the University of Maryland. This educational program, consisting of a three-year sequence of special individual and group instruction, is operative in some fifteen states with over 4,000 teachers as participants.[4]

The special education of children in human development materials is not new, nor are such programs as unusual as they were a decade ago. At the high school level, for example, a national survey[13] shows that courses with human development materials in them are taught in at least forty states and probably in all of them, and about three per cent of all high school students enroll in such courses. For younger children it is difficult to estimate the scope of such programs because there are undoubtedly many which are not publicly reported. There are well known programs such as the Delaware human relations program[7]. This program consists of organized classes for children with systematic lessons on human relations. In 1952 it was estimated that more than 200,000 students were enrolled in more than 7,000 such classes given in every state in the country.

All major educational techniques are used in all three varieties of programs. Parents are regularly educated by mass media, by group procedures, and by individual educational counseling.

For teachers, also, special pamphlets and books, as well as study groups, are used. Individual counseling of teachers also occurs, as in the New York City Three Schools Project, where a clinic team is available in the school and provides educational counseling for teachers.

So also is the case for children: Specific mass media materials such as the pamphlets of Science Research Associates, and study groups such as in the Delaware program, are regularly used.

For this vast endeavor in education for mental health there exist only a handful of evaluative research studies. For parents[5] and for teachers the number of such studies is about 20 each. Of the 20 pertaining to teachers, most are evaluations of a single program, that of the University of Maryland[4]. For programs directed to children, the number of studies is perhaps less than ten.

Of this total of about 50 research studies not all are of equal merit. Only about a dozen are of excellent design and analysis. Unhappily, the majority of them have various degrees of deficiency which run the usual gamut; that is, no controls, failure to handle loss of subjects, use of inappropriate tests of significance, procedures not specified clearly, etc. While all are mentioned in this review, designation will be made of those of outstanding quality.

In the near future these studies will be augmented by reports of numerous research projects currently underway including several very substantial ones. For example, the St. Louis County Health Department[44] is in the third year of a five-year study evaluating parent discussion groups. In Canada, the Forest Hills Project[33] will soon publish reports evaluating its major program of teacher education, and separately, its program for children in human development, although the latter is more therapeutic than educational. Hence, even a few years from now our knowledge of the effects of educational programs may be substantially increased through research in progress.

Turning now to the research data itself, I have organized my discussion around what strike me as some of the critical assumptions of the educational programs for parents, teachers, or children.

The foundation of all three types of program is, of course, the assumption that there is a causal relation between presentation of human development materials and improvement in the child's mental health. It appears to me that this assumed causal relation has probably four elements in it, which together form the causal sequence through which the effects of the program must flow. I see these four elemental assumptions as follows:

1) Mental health in the child, and his mental health as he matures, may be increased by certain kinds of interpersonal relations which he has with his parents, his teachers, and with others;

2) Interpersonal relations productive of mental health flow in part from a host of effective and evaluative factors which determine the way in which one perceives others and himself; for examples, as to the causes of behavior, the intent of other persons lying behind their actions, and so on.

Throughout the paper we plan to refer to these complex motivational and evaluative factors as "attitudes," simply for shorthand purposes. This group of motivational and attitudinal factors can be viewed as intervening variables which mediate the relation between knowledge on the one hand and interpersonal relations on the other.

3) Those attitudes, which are conducive to interpersonal relations of the kind which in turn promote mental health, are determined in part by knowledge of human development. Here I use human development in its broadest sense and include not just "ages and stages" in its simplest form but knowledge of the critical demands and common responses of different phases of growth, plus knowledge of the dynamics and motivations of behavior.

4) Knowledge of human development can be transmitted through educational programs to parents, teachers, and children.

The four concepts of human development—knowledge, attitudes, interpersonal relations, and mental health—provide the points of focus for our discussion of research. One can ask of the available research two quite different questions. First, we might ask whether research shows education to *change* any of the four elements without questioning their causal relation to each other. Thus, "Do educational programs really change the factual knowledge of those participating in them? Or change their attitudes? Or change their relations with other persons? Or, finally, change their mental health?" One may ask each of these four questions separately, since each of these elements may be taken as a separate dependent variable in evaluative research. For example, a study may seek to discover the relation between educational programs and change in a teacher's classroom behavior. The validity of the causal sequence is not questioned here. Rather, one assumes it to be true and studies instead the effects of programs upon some single element within the chain. Secondly, we might ask whether the causal sequence really operates as assumed. For example, given a demonstrated change in parental attitudes as a result of an educational program, what is the evidence that such change in attitudes is related to subsequent change in the mental health of the child? More generally we ask, "Does the change from educational programs, in whichever element in the sequence one might name, really flow on through to influence the mental health of the child?"

It is my belief that we profit by asking both of these rather different questions, and I intend to consider them in order. First, then, what does the research tell us about the effects of educational programs upon the four elements of information, attitudes, interpersonal relations, and mental health?

In regard to information, one would certainly anticipate that educa-

tion in human development should result in a gain in factual knowledge of human development; the studies which specifically evaluate this question indeed show increases in information. For studies of parents several[1, 18, 29, 32] show a gain. For teachers, five studies are available, all showing gains in information (Avery, Duff, Hohn, Mershon, Perkins in Reference 4). The various criteria of information increase are a greater use of human development principles, increase in use of evidence, increase in analyzing child's behavior, increase in basic use of child development concepts, increase in use of objective data, and increase in ability to advance both specific and multiple sound reasons for specific behaviors. In regard to children, we have found two studies we would classify as gains in information by children from programs directed to them.[27, 37] Both studies are from the excellent series evaluating the Iowa preventive psychiatry program, and both show an increase in the child's awareness of the underlying causes of human behavior.

In sum, there are eleven studies dealing with informational changes resulting from educational programs, and all show increases. We have found no study reporting negative results.

The next element to consider is the change in attitudes of persons exposed to programs. A variety of attitude measures have been employed, reflecting the variety of conceptions of what the important characteristics are.

In regard to parents, an excellent study by Shapiro[34], using five attitude scales derived from Shoben[35] and Harris, Gough, and Martin[16], finds a significant decrease in parents' authoritarianism and possessive attitudes and an increase in attitudes indicative of good judgment. Another study[18] shows a favorable change in parents' attitudes towards the development of self-reliance in children. Another[8] shows a change in the direction of developmental, in contrast to traditional, child-rearing attitudes. Still another[31] shows a change in attitude on the Fels rating scales toward what is good and bad handling of children on the control-freedom scale, and on the free-growth scale. A fifth study[12] of more than 1,000 parents, shows that parents' attitudes towards the seriousness of fifty traits pertaining to children changed in the direction of that of experts of child development.

Finally, an excellent study by Balser and his colleagues[2] using the MMPI as an evaluation instrument finds that parents improve on the family relations scale of the MMPI.

In contrast to these six findings, there are negative results reported. In the Balser study just mentioned, the Shoben attitude scale was also used in the evaluation. While the experimental group of parents im-

proved in attitude as measured by the scale, one of the two control groups not involved in the educational program showed an even larger improvement. Another study by Collins[9] evaluating a two-week program found no significant changes on this same Shoben scale. Finally, two studies[29, 39] both report no changes in mothers' attitudes in the area of sex instruction of their children after an educational program.

Considering teachers now, four research reports are available (Avery, Hohl, Perkins, and Wood in Reference 4). One finds that teachers completing a third year in a child-study program were significantly more accepting of themselves and of others than were those in the earlier years of the program. Another reports that teachers' attitudes towards the seriousness of behavior traits in children changes in the direction of child development experts, thus paralleling the finding for parents mentioned above. A third reports that the teachers' conception of the ideal teacher changes in the direction of that of experts, and the fourth reports teachers indicate significantly warmer and more accepting attitudes towards children in their classes.

In addition to these four studies, there are two excellent studies which utilize the MMPI as the attitude instrument. These were both done by the same research group. Teachers and school administrators underwent a 15-week hour-and-a-half seminar series. In the first study[3] the experimental group made significant improvements on the MMPI, but the control groups also made these changes. In the second study,[2] using the same educational procedure, the data show the same improvement on the MMPI for the experimental group, but this time it was not paralleled by change in the control group. Moreover, in the second study the results also show significant improvement in teachers' attitudes towards child care, as assessed by the Shoben scale.

Regarding children, we have been unable to find any study of attitudinal change, although the two Iowa studies mentioned before in connection with changes in causal understanding of children may also belong in part here, since it is difficult to distinguish the informational from the attitudinal aspects.

If you are keeping score on the attitude research, it sums to the following: for parents, six positive changes, three non-changes, and one experimental-control group tie; for teachers, five positive changes, and one tie; for children, no results in this category. In sum, the *weight* of the evidence supports the assumption that educational programs result in attitude changes believed to be desirable.

The third element to be considered is that of the actual behavior of the parent, the teacher, or the child. Because of the usual difficulties in direct observation of behavior, especially in studies of parents, the infor-

mation pertaining to behavior frequently involves paper and pencil or other reports rather than direct observation. A possible hiatus between such reports and the actual behavior should be kept in mind in considering the results of the studies.

First, in regard to parents there are some eight studies assessing changes in parental behavior following educational programs in human development. Three of these are evaluative studies of the same pamphlet series, the "Pierre the Pelican" series of the Louisiana Society for Mental Health. The first[31] contrasts experimental and control groups on fifty-four behavioral items, and reports eighteen significant improvements in parent-child relations by the experimental group. A second[43] using the same design but comparing the two groups on some forty-three items, finds ten significant improvements, eight of which favor the experimental group and two which favor the control group. Please note this, however. On the items where these two studies are comparable, the results contradict each other on one-half of the items. To put it another way, the agreement is no more than one would expect on a chance basis. The third study[15] uses the same procedure but contrasts the two groups on only five items, all concerned with feeding practices. This study finds no significant changes on any of the five items.

The remaining studies evaluate study groups or individual counseling procedures. One[19] reports significant improvement in mothers' child-rearing practices in the direction of agreement with expert judgment. Another[17] reports an increase in use of casual incidental instruction of children in sexual matters, as recommended by the program. A third[29] reports an increase in flexibility and permissiveness in child care. Another[6] reports changes towards permissiveness in feeding practices on the part of eight of some fifty-seven mothers involved, but no change in the rest. Finally, one[39] reports *no* changes in behavior in the area of sex instruction, thus contrasting with the study mentioned above.

In regard to teachers, we have found but two studies which we would put in this category. One of these (Greene, in Reference 4) reports an increase after a child-study program in teachers' use of positive versus negative ways of handling children, and in a democratic organization of the classroom. The second, (Haddock in Reference 4) based on a direct observation of teachers' performance in the classroom, finds that teachers involved in the child-study program changed toward a greater use of human development principles, including looking for underlying causes, using multiplicity of causes, reserving judgment, and several others.

Considering children now, three reports are available. One,[30] evaluating a five-week educational program for fifth graders, shows changes in the experimental group in that children originally disliked improved

their status in class and that there was a general increase in sociometric liking for each other in the class. Another,[25] evaluating a program for adolescents, finds a significant decrease in conflict of the adolescents with their parents. The third (Greene, in Reference 4) reports an increase in positive responses by pupils to teachers where the teachers have been exposed to an educational program, *and* have shown a prior increase in their positive ways of handling the children.

Let's return now to our scorekeeping task for this area of behavior change. For parents, there appear to be three clear positive changes, one no-change, and one falling somewhere between with 8/57 of the parents changing. The three mass media studies leave the issue confused, there being two positive changes, one no-change, and the two positive changes not supporting each other. For teachers, there are two positive changes; for children, three such changes. In sum then, the weight of, but not weighty, evidence supports the assumption that behavior changes deemed desirable follow from educational programs in human development.

Let us look now at the last element in the causal chain, improvement in the mental health of children. It may appear that before we can deal with this issue we need a formal definition of mental health, but, fortunately for me, I do not think this is the case. Instead I will report to you on those few studies which have evaluated the effects of educational programs upon various aspects of the child's personality, and leave to you the decision as to whether the changes are or are not relevant to your concept of mental health.

Two studies employ as their measure of the child's mental health the children's form of the California Test of Personality. This test, as you doubtless know, measures "good adjustment" by a total score and two subscale scores for self and social adjustment, as well as scores for the more specific subscales. Both studies report on fifth-grade children who participated directly in educational programs with human development content. Both studies used experimental and control groups. The first of these indicates improvement in the experimental group in the self-adjustment portion of the California test, with no change in the control group;[30] however, no tests of significance were made. The second study[36] shows a significant over-all increase on the California test for the experimental group and especially large increases on several of the subscales, such as sense of personal worth. In contrast, the control group tended to deteriorate on the test and did so significantly on several of the subscales.

A third study[10] evaluates the results of an educational counseling program of parents in Baltimore. Changes in the behavior records of 100

children of parents who were counseled over more than a year were compared with the records of children of parents not so counseled. Matched pairs of children were used. Comparisons of the records of the two groups made by trained clinical personnel show that 96 of the 100 experimental children improved in ratings during the course of the counseling and that ratings for the experimental group were generally better than those for the controls.

Three more excellent studies evaluate the effects of the Iowa program on the child. In the earliest of these by Ojemann and Wilkenson[28] children whose teachers had been given personality data on them and had had the data interpreted to them achieved higher marks, showed a decline in personality conflict, and improved social attitudes. A more recent study[27] evaluates changes in children studying a specially prepared curriculum with emphasis on looking for the causal factors in behavior, and whose teachers had been trained to give this special program. The results show that on a problem-situations test, which measures the degree to which one is punitive to others in an interpersonal situation, the experimental children showed a decline in punitiveness to others. This same test has been shown in other studies[23] to be related also to authoritarianism. Last, in a separate study by Levitt[22] using the same experimental groups as were just described, the children in the groups showed a significant decrease in anti-democratic tendencies and an increase in responsibility. The measure of anti-democratic tendency employed has been shown in separate studies to be related to anti-Negro feeling[14] and to the measure of punitiveness already described.

In sum, these six studies show that children involved directly in this type of educational program, or whose parents and teachers were involved in such programs, make significant increases on the California test designed to measure good adjustment, that they decline in the tendency to be punitive to others, that they decline in anti-democratic attitudes, that they increase in responsibility; and, in view of the fact that some of these are significantly correlated with other measures, one may also infer that such children decline in authoritarianism, and in prejudice towards minority groups.

Given that such changes are in a direction which we consider to be mentally healthy, the evidence supports the assumption that educational programs in human development promote mental health in children.

With this, we have completed our review as it pertains to our first question. This question was, you recall, whether or not changes occurred from educational programs in the four aspects of information, attitudes, interpersonal relations, and mental health of the child.

Consider now the second question we were to ask of the research data,

namely, "Does the change from an educational program, in some element in the assumed causal sequence, have an end effect on the child's mental health?" The importance of this question is all too clear. The dozen studies indicating an increase in information, the numerous studies attesting to an improvement in attitude and in behavior, are simply irrelevant to the issue of the child's mental health unless it is demonstrated that these changes are instrumental in promoting mental health. Indeed, a contrary assumption to that of a causal relation between, say, information and mental health, is simply that there is no such relation, and that we might improve the information of the general public on human development a hundred-fold and not influence in the slightest their mental health. It is imperative, therefore, that we consider the validity of this assumed causal sequence.

Looking now at the changes in interpersonal relations which have been shown to result from educational programs, let us ask whether there is evidence that such interpersonal relations are related to mental health. Obviously we do not question the general principle that one's relations with others influence his mental health. The great bulk of clinical literature, stemming from and including the work of Freud, supports this principle. But consider now what the several studies on changes in interpersonal relations have shown: an increase in permissive feeding, in casual instruction of children in sexual matters, in permissiveness in child care, in positive ways of handling children, in tendencies to act on the basis of ideas of multiple causation, and an improvement in sociometric status, to name most of them. Consider the results of the "Pierre the Pelican" evaluation studies indicating that parents improve in asking the child's permission to use his things for a new baby, in providing the baby with a separate room, in the frequency with which the father changes the diapers, etc.

Now, do we know from these same or other research studies that the characteristics of interaction I have just mentioned promote or inhibit the mental health of the child? Perhaps owing mainly to my ignorance, and I would be most happy to learn this was the case, I nevertheless do not know of any first-rate study which has shown these particular interaction characteristics to influence mental health. While it may make sense that casual instruction in sex matters helps one to avoid sexual disabilities, or that improvement in sociometric status results in an improvement in feelings of self-worth and desirability, these are still hypothetical relations. And so for the others, discrete and elusive, such as positive handling, having father change diapers, and the like; the causal relation between these and the child's mental health remains an open question.

As we move on to consider changes in attitude, we reach a more pleasant

terrain. Some of the studies of the effects of educational programs on attitude which I have reviewed use as an attitude instrument, Shoben's scale of parental attitudes. Moreover, some of these studies have shown improvement in such attitudes. It is, therefore, noteworthy that Shoben originally validated the scale by the method of known groups, and included in the scale those eighty-five items which discriminated between mothers of problem and non-problem children. Thus, these studies in effect show educational programs to result in changes in attitudes, which in turn have been shown to be related to the adjustment of the child.

One of the subdivisions of Shoben's scale pertains to authoritarianism. Authoritarianism in child-rearing has been shown in several studies to correlate very closely with general authoritarian attitudes of the parents in other roles.[21, 38, 41] Thus, it is relevant to note studies such as that by Maynard[24] which shows that there is a positive correlation between the amount of authoritarianism in a school principal's behavior and undesirable social attitudes on the part of the students in his school.

Several of the studies we have reviewed report favorable changes on the MMPI after educational programs. We could question whether parents and teachers with MMPI profiles indicating poor adjustment also produced undesirable emotional characteristics in children. However, an excellent study by Crawford[11] points toward the answer. Crawford compares pupils of three poorly adjusted teachers with those of three well adjusted teachers, as determined by the MMPI. A test-retest of the children with Roger's test of personality adjustment finds that over the year the pupils change in the direction of the teachers' adjustment. Those pupils with poorly adjusted teachers *significantly decline in adjustment;* the reverse is true for pupils of the well adjusted teachers. This indicates, then, that the favorable changes on the MMPI shown to follow from education of parents and teachers may in fact contribute to improvement in the child's mental health.

In regard to the other attitude changes which have been demonstrated in the studies we have reviewed; e.g., in improved attitudes towards self-reliance in children, change toward developmental attitudes, a change in attitudes towards seriousness of traits in the direction of experts, changes in the conception of the ideal teacher towards the conception of experts, more accepting attitudes towards children, more acceptance of oneself, etc.—for all of these we can only say that while there is a certain validity one might assume between such changes and the mental health of the child, unfortunately it has yet to be clearly demonstrated.

Lastly, considering the relation between changes in factual knowledge and the child's mental health, we have been unable to find any study which relates, for example, the amount of knowledge that a parent or teacher

possesses and the mental health of the child. To be sure, the six studies which were focused on changes in the child's mental health *imply* a previous increase in information; however, this prior information increase was not itself measured; hence, speaking rigorously these researchers do *not* show the relation between information gain and mental health. The one exception might be the study by Dr. Ojemann and his colleagues which shows both an increase in causal approach to behavior (which is in part a measure of information) and a decline in punitiveness to result from their program.

To me, the relation between increased information and improved mental health has less face validity than do the other assumed relations and demands some research attention. For one thing, certain kinds of factual knowledge may incapacitate the parent or teacher; for another, many people have pointed out (e.g., Reference 26) that much factual knowledge is given in a way that makes it difficult to translate into attitudinal or behaviorial changes, so that it may remain, in this sense, useless information.

This, then, completes our review of research as related to our second question.

In considering my summary remarks for this overview, it seemed that I could follow either of two paths. On the one hand, the available research is predominately encouraging, and we might conclude that, even though more research is necessary, the educational programs of the type considered here have demonstrated their value. On the other hand, one could view the available research as helpful but inadequate, and conclude that such educational programs, like Willie Lohman, are out there riding on a shoestring and a smile. Being by temperament more critical than supportive I have chosen the latter path.

To me this brief overview does not show us to be in a strong position in respect to the scientific bases of our educational efforts. Entirely apart from any expansion of our current efforts, even to justify the scope of the present effort, involving many millions of dollars and certainly more than a million man-hours per year of highly trained scientific personnel, demands more knowledge about the effects of what we are doing than we have at the present time.

Moreover, we are not sure that the worst thing we might find if this research were to be carried out is simply that no beneficial effects occur. We must consider the possibility that such programs are in fact detrimental to the mental health of the child. While we have not, of course, been concerned with the role of therapeutic programs in this review, they must be mentioned now. We can view therapy, within our current framework, as a procedure whereby the defenses of the individual are dis-

sipated through a certain kind of interaction with another person. The purpose of this therapeutic program, in essence, is to render the individual *educable,* so that he may benefit from exposure to factual knowledge concerning himself, other people, and his relations to them. This suggests that many of the persons exposed to educational programs may not be educable because of certain defenses which they have.[42] In some programs for parents,[40] more than in those for teachers, this problem has been recognized, and the good parent education programs are extremely sensitive to avoiding attempts to educate where it is evident that the parent cannot profit and indeed where harm might be done. This also suggests the importance of some professional training in recognizing such areas on the part of those responsible for educational programs.

To pour information into individuals indiscriminately may indeed result in emergence of trouble for those persons where they are not able to accept such information or where the information may be "accepted" in unexpected ways, feeding already established disturbance. Some of the programs directed to children suggest that the education of children proceeds before resistances to insight, that is defenses, are developed. We must recognize in disagreement with this that certainly children have defenses, however weak, and that these should be considered in such programs. I believe it is most instructive in this connection to consider some further results of two studies which have been mentioned before. In Rosenthal's study[30] of fifth-graders to whom human relations were taught, a lesson was included adapted from the large Delaware program. This lesson consisted of having other students in the class rate an individual on ten traits, and then the individual was asked to look at the ratings on himself done by others. Such ratings were anonymous. This frank appraisal of one's behavior and personal characteristics by others, designed to develop insight into oneself, might well be traumatic to some of the students. It is most important, then to hear Rosenthal's result; namely, that in his experimental group those children initially poorly-adjusted declined further in adjustment as a result of the program, whereas those children initially well-adjusted, and therefore presumably better able to handle the information about themselves, and/or receiving less punishing information, were the ones who improved. This is an isolated finding, but Shapiro's study of parents[34] indicates that those who held initially desirable attitudes showed the greatest improvement after a course of education through study groups, while those with initially unfavorable attitudes failed to show this substantial improvement.

This argument and these two studies lead to a tentative conclusion about educational programs; namely, that the educator in human relations, just like the therapist, cannot unwittingly or irresponsibly tamper

with those humans he is educating, and yet sometimes may do just this by indiscriminately asking the members of his program to look at themselves and at others without regard to their individual abilities to healthfully assimilate this information. This simply underscores the need for better knowledge of the effects that we produce, and I hope keeps us from being complacent by saying that at the very least we are doing nobody any harm.

If we are serious about our efforts to educate to the end of promoting mental health, and clearly we are serious about this, and indeed must be in terms of the scope of the mental health problem, then better research is needed. As scientists we should be too proud to just happily assume that we can put in facts at one end and have healthy children come out at the other. It seems to me that the better research of the future will have not necessarily better research execution, but better conceptualization.

The crucial problem in conceptualization deals with the characteristics of the child which pertain to his mental health. Attempts at global descriptions of mental health, or definitions in terms of some solitary global trait, such as adjustment, make mental health difficult, if not impossible, to measure and hence place it beyond the realm of research operations. It might be well to substitute a conception of mental health for research purposes which views mental health as composed of a variety of skills. This, then, permits us to isolate those characteristics worthy of study, whether they be the child's causal approach to life, social desirability to others, the relation between his real and his ideal self-image, his feelings of self worth, or whatever. Given such a formulation, one is enabled to work backwards through the causal chain of theory, asking himself what changes in the parent or the teacher or the child are necessary to generate these mental health characteristics in the child. *Then* one can specify the type and the content of the educational programs designed to produce these intervening changes. But without this theory, which links increased information to the mental health of the child, we cannot design the educational program adequately. While all the studies reviewed here this afternoon have had at least some causal theory, however rudimentary it may have been, we are pleading here that such theory be made explicit.

Given the formal explication of such a theory one is then ready to test it; i.e., "evaluate the program." Let us consider for a moment what we have learned about evaluation from our review. It is clear that we can no longer afford to test only the variables in the intervening phase, e.g., changes in the teacher, as was done in the Maryland evaluation studies, and then just assume that this produces mental health in the child. Only if there is substantial additional evidence which in turn relates such in-

tervening variables to the child's mental health is it legitimate for one to do this.

In the absence of such evidence one can do either of two things. One might test the effects on the mental health of the child alone, by-passing the hypothesized intervening changes in the parents or teachers and dealing directly with the effects upon the youngsters. Given the predicted changes in the children, this in turn tends to validate the hypothesis about the intervening changes in the parents or teachers.

Or, one can assess changes in both the intervening variables and also in the child's mental health. This procedure is used by Ojemann and his colleagues, and also appears in the St. Louis study referred to earlier. In the latter, the theory hypothesizes that the important intervening variable is the mother's conception of the causal basis of a child's behavior. An assessment is made of the mother's causal conceptions, *and also* of changes in the mental health of the child through extensive clinical testing and rating procedures.

In closing, we see on the scene a rich variety of competing theories about what, if any, are the educable characteristics of parents, of teachers, or of children which promote mental health in the child. These include specific child-rearing practices such as weaning procedures, specific attitudes such as warmth, authority, or rejection, various cognitive syndromes such as the causal approach to human relations. Let us include also those theories which hold that there are no such characteristics amenable to change by education.

In spite of any critical remarks, I yield to no one in my concern for education for mental health. Yet I, like you, look forward to the time in the future when these various conceptions do not compete against each other as at present but rather are entered into a race organized according to the canons of science, so that we will in fact be able to determine who the winner may be.

REFERENCES

[1] Andrew, Gwen, "A Study of the Effectiveness of a Workshop Method for Mental Health Education," *Mental Hygiene*, 1954, 38, 267-278.

[2] Balser, B. H., F. Brown, M. L. Brown, L. Laski, and D. K. Phillips, "Further Report on Experimental Evaluation of Mental Hygiene Techniques in School and Community," *American Journal of Psychiatry*. In press.

[3] Balser, B. H., F. Brown, M. L. Brown, E. D. Joseph, and D. K. Phillips, "Preliminary Report of a Controlled Mental Health Workshop in a Public School System," *American Journal of Psychiatry*, 1955, 112, 199-205.

[4] Brandt, R. M., and H. V. Perkins, "Research Evaluating a Child Study Pro-

gram," *Monographs for the Society for Research in Child Development,* Inc., XXI, 1, 1956.

5 Brim, O. G., Jr., "Evaluating the Effects of Parent Education," *Marriage and Family Living,* 1957, 19, 54-60.

6 Brim, O. G., Jr., "The Acceptance of New Behavior in Child Rearing," *Human Relations,* 1954, 473-491.

7 Bullis, H. E., "An Educational Program for Development of the 'Normal' Personality," *American Journal of Psychiatry,* 1952, 109, 375-376.

8 Chandler, B. A., "An Exploratory Study of the Professed Parent-Role Concepts and Standards of Child Behavior of Mothers in a Parent Education Project," *Dissertation Abstracts,* 1955, 15, 219-220.

9 Collins, M. G., "A Study of Parent Attitudes on Child Management Before and After Training, Utilizing the Critical Incident Technique," *Dissertation Abstracts,* 1954, 14, 872-873.

10 Cooper, M. M., "Evaluation of the Mother's Advisory Service," *Monographs of the Society for Research in Child Development,* 1947, 12, 1.

11 Crawford, R. E., "Teacher-Pupil Personality Relationships," *Dissertation Abstracts,* 1953, 13, 589.

12 Davis, E. A., and E. McGinnis, *Parent Education: A Survey of the Minnesota Program,* Minneapolis: University of Minnesota Press, 1939.

13 Engle, T. L., and M. E. Bunch, "The Teaching of Psychology in High School," *American Psychologist,* 1956, 11, 188-193.

14 Gough, H. G., D. B. Harris, W. E. Martin, and M. Edwards, "Children's Ethnic Attitudes: I. Relationship to Certain Personality Factors," *Child Development,* 1950, 21, 83-91.

15 Greenberg, B. G., M. E. Harris, C. F. MacKinnon, and S. S. Chipman, "A Method for Evaluating the Effectiveness of Health Education Literature," *American Journal of Public Health,* 1953, 43, 1147-1155.

16 Harris, D. B., H. G. Gough, and W. E. Martin, "Children's Ethnic Attitudes: II. Relationship to Parental Beliefs Concerning Child Training," *Child Development,* 1950, 21, 169-181.

17 Hattendorf, K. W., "A Home Program for Mothers in Sex Education," *University of Iowa Studies in Child Welfare,* 1932, 6, 11-92.

18 Hedrick, B. E., "The Effectiveness of a Program of Learning Designed to Change Parental Attitudes toward Self-Reliance," *University of Iowa Studies in Child Welfare,* 1934, 10, 249-268.

19 Jack, L. M., "A Device for the Measurement of Parent Attitudes and Practices," *University of Iowa Studies in Child Welfare,* 1932, 6, 137-149.

20 Klatskin, E. H., "Shifts in Child Care Practices in Three Social Classes under an Infant Care Program of Flexible Methodology," *American Journal of Orthopsychiatry,* 1952, 22, 52-61.

21 Levinson, D.J., and P. E. Huffman, "Traditional Family Ideology and Its Relation to Personality," *Journal of Personality,* 1955, 23, 251-273.

22 Levitt, E.E., "Effect of a 'Causal' Teacher Training Program on Authoritarianism and Responsibility in Grade School Children," *Psychological Reports,* 1955, 1, 449-458.

[23] Lyle, W.H., and E.E. Levitt, "Punitiveness, Authoritarianism, and Parental Discipline of Grade School Children," *Journal of Abnormal and Social Psychology*, 1955, 51, 42-46.

[24] Maynard, Honor E., "A Study of Pupil Human Relations Within the School as Influenced by the Principal's Pattern of Behavior," *Dissertation Abstracts*, 1955, 15, 755.

[25] Morgan, Mildred I, and R.H. Ojemann, "The Effect of a Learning Program Designed to Assist Youth in an Understanding of Behavior and Its Development," *Child Development*, 1942, 13, 181-194.

[26] Ojemann, R.H., "Changing Attitudes in the Classroom," *Children*, 1956, 3, 130-134.

[27] Ojemann, R.H., E.E. Levitt, W.H. Lyle, Jr., and M. F. Whiteside, "The Effects of a 'Causal' Teacher-Training Program and Certain Curricular Changes on Grade School Children," *Journal of Experimental Education*, 1955, 24, 95-114.

[28] Ojemann, R.H. and F. R. Wilkinson, "The Effect on Pupil Growth of an Increase in Teacher's Understanding of Pupil Behavior," *Journal of Experimental Education*, 1939.

[29] Owings, C., *A Research in Parental Sex Education* and *The Effectiveness of a Particular Program in Parental Sex Education*, Minneapolis. University of Minnesota Press, 1932.

[30] Rosenthal, S., "A Fifth Grade Classroom Experiment in Fostering Mental Health," *Journal of Child Psychiatry*, 1952, 3, 302-329.

[31] Rowland, L.W., *A First Evaluation of the Pierre the Pelican Health Pamphlets, Louisiana Mental Health Studies, No. 1*, New Orleans: Louisiana Society for Mental Health, 1948.

[32] Schaus, H.W., "An Experimental Investigation of Methods in Parent Education," *University of Iowa Studies in Child Welfare*, 1932, 6, 117-134.

[33] Seeley, J.R., "The Forest Hill Village Project," *Understanding the Child*, 1954, 23, 104-110.

[34] Shapiro, I.S., "Is Group Parent Education Worthwhile? A Research Report," *Marriage and Family Living*, 1956, 18, 154-161.

[35] Shoben, E.J., Jr., "The Assessment of Parental Attitudes in Relation to Child Adjustment," *Genetic Psychological Monographs*, 1949, 39, 101-148.

[36] Slobetz, F., and Alice Lund, "Some Effects of a Personal Development Program at the Fifth-Grade Level," *Journal of Educational Research*, 1956.

[37] Stiles, Francis S., "Developing an Understanding of Human Behavior at the Elementary School Level," *Journal of Educational Research*, 1950, 516-524.

[38] Willis, R.H., "Political and Child-Rearing Attitudes in Sweden," *Journal of Abnormal and Social Psychology*, 1956, 53, 74-77.

[39] Witmer, H.L., *"The Attitudes of Mothers Toward Sex Education,"* Minneapolis: University of Minnesota Press, 1929.

[40] Child Study Association of America, *Parent Group Education and Leadership Training: Three Reports*, New York: 1953, (Rev. Ed.)

[41] Child Study Association of America, "Evaluation of the Leadership Training Program," New York: 1955. Mimeographed.

[42] Committee on Preventive Psychiatry of the Group for the Advancement of Psychiatry, "Promotion of Mental Health in the Primary and Secondary Schools: An Evaluation of Four Projects," Report No. 18, Topeka, Kansas: 1951.

[43] Michigan State Department of Mental Health, "A Report of Some Aspects of the Effectiveness of the Pierre the Pelican Mental Health Pamphlets," Lansing, Michigan: 1952. Mimeographed.

[44] St. Louis County Health Department, Clayton, Missouri. "Mental Health Research Program," Herbert R. Domke, Principal Investigator. This study is supported by a special grant from the National Institute of Mental Health, U.S.P.H.S.

DISCUSSION

Dean Peterson: We're indebted to you, Dr. Brim. It strikes me you've put a lot of human development wash through a critical wringer. And I think that is a better way to treat the wash than to throw it into the dryer which just fluffs it up and makes it shed a little lint!

Dr. Milford E. Barnes is all Old Gold—B.A., Iowa; M.D., Iowa, and a residency in our Psychopathic Hospital. Dr. Barnes is returning the first of the coming month to this University to join Dr. Kugel in the Department of Pediatrics and Mr. Roll in the College of Education in what we hope will develop into an exciting interdisciplinary center for mentally retarded and emotionally disturbed children. So it is with particular delight that I take this opportunity to welcome Dr. Barnes back to his university. Dr. Barnes has been involved in child guidance centers in Madison, Wisconsin, and more recently in Wilkes-Barre, Pennsylvania. We are looking forward with great anticipation to having a lot of fun while working together in this new center which is in the process of being organized in this University. Dr. Barnes will open the discussion.

Dr. Barnes: I'm sure we all appreciate the devastating clarity with which Dr. Brim evaluated the present-day research. He posed two questions for us. One is—what are the changes that may be produced by an educational program? Can changes be produced in information and attitudes in a personal relationship to behavior? He answered that question in the affirmative; that is, from the research results, changes apparently can be produced. Certainly my own inclination would be that remarkable changes can be produced by the process of education, almost frighteningly powerful changes at times. The second question he asked is—what relationships exist between the changes produced and mental health? You can produce the changes, so what? This is

a problem that we have been batting around all day here and not getting too far with.

This question is not one we can answer readily, but I should like to pick up his last suggestion that we begin to consider this problem less in global terms. I think Dr. Ojemann suggested this in the outline that he sent to us at the beginning. Dr. Gottlieb reiterated that we need specific elements in our research concepts. For purposes of argument I thought of something that might be interesting to you. We have had held out to us the example of the eradication of certain physical diseases, such as smallpox, diphtheria, etc. While thinking about this, I began to wonder whether these diseases really were eradicated. Have they really been prevented? The answer is, surprisingly, no. These diseases have not been prevented. It's perfectly true that nobody gets smallpox, but everybody is injected with cowpox before he goes to school. The law requires it of us. Most of us are infected with diphtheria and with other different diseases. The mode of prevention is to infect us with a weakened or killed strain of a disease organism from which we build up immunity, an active immunity so-called. Out of our own bodies we build up antibodies and defense mechanisms and have some way of coping with infection. Later on if we should be exposed to a large dose of the virulent bacteria, we would have active defenses ready to cope with it. This is fighting fire with fire; that is, disease with disease. In a sense, we are giving the human body the experience of going through an attack of a weak disease in order that it will build up defenses against a more severe disease.

As I thought more of this, it seemed to me that this is true throughout medicine. As a matter of fact, most of medicine seems to consist of fighting one disease with another. In order to fight pneumonia, we infect a person with a mold, such as penicillin or aureomycin. We poison the body with sulfa drugs which are a worse poison to the bacteria than to the person. Sometimes it takes a thief to catch a thief. Perhaps this is playing with words, but my purpose is to draw some pertinent points relative to the prevention of mental disease.

We find that in medicine in general there are the following modes of combating disease in a person. The first is to give the patient passive immunity, which means to give him some immune serum borrowed from another person or another animal. This is a temporary measure. You put antibodies which somebody else built up into the person and he lives on these, so to speak. The second mode is to give the person a mild attack of the disease in order that he can go through an experience of the disease and thus can build up his own defenses to it. A third means is to give the person another disease which will counteract the first one.

And the fourth thing we do in physical medicine is to give the person adequate nutrition in the form of vitamins, fluids, food and so forth, so that he can restore his own homeostasis. Now, do we do this sort of thing in the emotional life? I think in a way we do. I think we can compare active immunization with a sort of emotional immunization. Possibly we have neglected research along this line.

One of the things I noticed in Dr. Brim's review of the research was that none of the programs seemed to emphasize going through an experience. The concepts had to do with giving people information but not with the results of enduring an experience. As a clinician, I cannot conceive how a person's emotions can be altered, how he can learn to cope with feelings without enduring them. I've tried to advise children for years and it's never done any good. I've had to help them live through an emotional experience and then they derive some benefit.

Now we did do this sort of thing during the war—and I use the example of war because this is the only place in our modern human society that I know of where we deliberately set out to face an emotionally terrifying experience. We stumble into other experiences—we get married and think it's going to be easy. But war is the only place where we deliberately set out to prepare ourselves for severe emotional crises. We do this by the following means: We give soldiers indoctrination courses; we discipline them; we emphasize habit training; we teach them to rely on their officers and to rely on one another; we teach them about their weapons and then we set them through a combat reaction course. Thus in giving them some little experience of combat, we are trying to immunize these soldiers both *passively*—which would be by the support of the discipline, the organization, the friendships, leaning on other people and so forth—and *actively* by experiencing directly a small amount of combat in order that they won't panic when they get into the real thing. Perhaps fraternity hazing—which we've pretty much had to rule out—served some of this purpose. You haze a boy a bit and he develops a way of coping with it so that he can get along well in everyday living in the fraternity.

Now to continue with our analogy, passive immunity can be compared to the borrowed strength a person has in a situation in which he is dependent upon a parent, a spouse, or some love object. Sound nutrition might be represented by a sort of morale factor. A person with good morale gains his morale from the people about him and from his awareness of his role; i.e., his respect for his role in an organization or a group. It is exceedingly difficult for a man alone and isolated to maintain good morale. Whether we deliberately give a person one disease to combat another, I don't know—unless we do it with psychological therapy.

Having drawn this set of analogies, I would like to relate this to Dr. Brim's suggestion that we can make specific attempts to treat people through experience and information. I personally would tend to emphasize information and specific skills to meet specific stresses that occur in modern life. One can't seriously suggest that you try and give a person a light dose of schizophrenia, but we can aid people who get into stress situations, we can help them to cope with it, let them lean on a therapist, a parent—somebody—while they're in stress and thereby they may develop an active immunity or ability to resist which would carry them through. I think we do do this in child guidance centers and I think perhaps this is part of what happens in psychotherapy.

Now, is a state of immunity health? No, I don't think the state of being immune to smallpox is healthy, but it certainly is not healthy not to have this immunity if you're going to be exposed to smallpox. One way of looking at health is to consider the degree of general good health, including nutrition and so on. Then there are a lot of specific immunities that our body has developed in resisting stresses and disease processes. Perhaps this has some relationship to mental health, I don't know. However, it would seem to me that in this matter of experience there ought to be many possibilities for research programs in which we deliberately attempt to aid people to meet certain kinds of crises through training and experience and information. We could later measure the results by seeing how the subjects coped with the stresses for which the programs were designed. I think it might be difficult to persuade people that it is worthwhile to subject themselves to some stress in order to meet another. But I'm not sure, philosophically, if it's any different from subjecting yourself to cowpox in order to avoid smallpox. At any rate the possibility for research along these lines does exist.

I'm very grateful for this opportunity to have been here, and I do hope that we can pick up the very stimulating elements in discussion that Dr. Brim has presented to us.

Dr. Caldwell: I should like to ask Dr. Brim if any of the studies have in any way looked for negative changes, or has there been sort of a deliberate looking the other way?

Dr. Barnes: Would you like to get these remarks together, Dr. Brim, and then answer them? [Dr. Brim replied affirmatively.]

Dr. Spiegel: I should like to say that I feel very grateful, Dr. Brim, for this presentation which seems to me like a much needed breath of fresh air in an atmosphere which has been very confused and cloudy. I myself don't think the presentation destructive at all, but rather one which clears away the underbrush so that we can begin to take a good clear look at what is intended to be accomplished. In the education of children I

think—to grind the ax—that it is impossible to define what we want to do in terms of mental health without seeing that we are attempting to adjust the individual to a particular type of culture. I suspect that behind many of the criticisms Dr. Brim has raised, one would find criteria for the identification of the traits that are presumed to be associated with mental health but which really have nothing to do with mental health at all, but rather represent the notions of norms and the appropriate role relations which are held by people who are members of the middle-class especially urban middle-class families.

There is no evidence at all that I know of, and Dr. Brim has certainly made this clear, that authoritarian attitudes within a family have to be associated with mental ill health; nor is there any evidence that permissive attitudes are associated with mental health. So one could run down this list of presumed traits—the warm and affectionate, or positive (whatever that means), or the communication of sexual information, whatever that means to the child, or even the communication of developmental processes. All these things seem to me to be the notions we would tend to hold as members of the subcultural group, but not necessarily notions that have anything to do with this mutual process of adjustment.

I think I will disagree with Dr. Barnes in specifying desensitization or immunization as the thing we have to look for at this time, principally because we don't know what the pathogenic element is. Until we can determine what is pathogenic in the mutual adjustment between the individual and his environment, how would we know in what way to immunize him? It's quite true that some of the efforts which we've made can be shown to be destructive to the individual. I am thinking in particular of one of the children in an Italian family who was very well trained in middle-class role relations because she had to spend a year and a half as a patient in the hospital. There the nurses, the doctors, the aides and what not very carefully, but unconsciously, trained her to develop autonomy, initiative, self-respect and a whole lot of things that would have worked well for a middle-class family; but it so happened that she came from a working-class Italian family which thought all these things were very bad. In the family's eyes this girl was spoiled. She was much too demanding and thought too much of herself and she wasn't nearly dependent enough upon her parents.

It seems to me that if we are to make progress in the identification of what mental health really is, we will have to look at the problems which these factors raise. We must recognize that subcultural groups have varied standards of behavior. Ours is a middle-class culture and this seems to be the determining factor and the magnetic force which determines what the nation as a whole conceives to be the proper way of behavior

and relating. You would have to determine what problems this middle-class culture gives to particular subcultural groups and how we can, as educators, help them to resolve those problems. If we can do this, it seems to me that we can get to the pathogenic element.

Dr. Dreikurs: I am not quite clear what conclusion you drew from this truly magnificent paper. Did you mean to conclude from your analysis that these particular forms of educational influences on parents and teachers are adequate to change their mental health, or are you talking about educational influences in general? From my own experience clinically, with both parents and teachers, I found out that teachers and parents have to learn and *can* learn how to train children differently because our time requires a different training and this difference in training can then be used toward a better mental health.

Dr. Brim: I'm sure I need not indicate that my motives were constructive and I'd hoped that the review was constructive. This is not the first time I've had the occasion to present this kind of review and some people always see it as constructive and others always perceive it as destructive. My own position and that of Dr. Spiegel is that Dr. Ojemann and his colleagues have demonstrated—and there is no gainsaying what they have demonstrated—that this kind of educational program results in important changes in the children. Dr. Slobetz's study also shows, without equivocation, that youngsters improve on the California tests of personality adjustment and there are several others. Dr. Balser's studies show that parents and teachers improve on the clinical instrument, the MMPI. To me the weight of the research is all positive. I think the most striking fact about these some fifty studies is that the one negative—the one detrimental result—occurred in some youngsters who probably already were ill, clinically speaking, and that the no-effect studies simply show no change while the majority of studies show increase.

Now you asked, do people look for negative changes? I can't, of course, impute motives. I would say this, though, not wholly in jest, that there are people of clinical persuasion who don't believe you can educate people and they do evaluation research to show that you can't and they strongly look for negative results. Researchers who believe you can change people look for positive results. Let's simply assume that they're all scientists; and that if negative results came up, they'd find them. And that would be my answer.

Dr. Caldwell: But measurements of negative effects can't be made after the study is over. The instruments for measuring negative results must be built into the research, just as the instrument for measuring positive changes.

Dr. Brim: Well, if you try an attitude instrument on somebody after an educational program, they can increase, they can stay the same, or they can decrease.

Dr. Caldwell: I would like to ask if any of these studies have tried a follow-up after a suggested interval following the program to see what happens to the children a year after, or don't we have the facilities?

Dr. Brim: I appreciate that question. Except for the Iowa program, none of these studies that I've mentioned today—to my knowledge—take the measure a year to five years later. There are some such studies underway, such as the long-range study of the effects of the counseling program at the Institute of Child Welfare at Berkeley. All we have now is a quick before and after measure. Dr. Ojemann has some evidence on the time it takes these changes to occur which I'm not competent to review here. Perhaps he could comment on this. Do you recall the effects on attendance, for example?

Dr. Ojemann: Yes, that was the one that occurred to me as you were speaking. In the first semester there were no differences between the attendance records of the experimental pupils and those of the control pupils; but after the second semester there tended to be a difference in the amount of absences between the two groups. The hypothesis on which we were operating was that if by learning to deal with his social environment, the child would feel more at home in it, then he would be less motivated to find excuses for staying away from school.

Dr. Slobetz: I hope we are taking the comprehensive point of view—that education will show effect. May I suggest that we could find good ground to dig in in the area of theories of learning and the psychology of learning as I can't help but feel that we're missing something. Basically education is trying to share the risk in this game of life which generations pass on to succeeding generations. The question is how can we best pass the ball, so to speak, to our children. The problem relates not only to mental hygiene but also to arithmetic and to spelling. Therefore, I think we should take a good look at the theories of learning and the implications that they have for effecting changes in people, or encouraging the changes that people make in themselves.

Dr. Iscoe: I'm going to bring up something on learning theory. These studies are encouraging in one respect—that *some* children appear to profit, although others don't. Those children who are at the point, who've received, perhaps, prior reinforcements, reflections from others that they're well liked, that they're getting along, those are the ones who can profit from some of the information that Dr. Brim said was given. Others seem to recede and their adjustment becomes worse.

We have a point here—we agree that human beings can learn. Factual

learning is easy, relatively easy, that is; emotional learning, Dr. Barnes and Dr. Brim have said, is a little more difficult. If you have a group of children to whom you present certain situations, some apparently show an increased sensitivity, in a follow-up after a year or two, in their ability to deal with problems of adjustment. Broadly speaking, one group of children can generalize from what they've learned. The other group that hasn't appeared to profit is the one we should investigate. We need to find out why this group can't follow through, and then perhaps we should try different procedures. This is a group that the school has to investigate—not so much because of their mental health alone but also for the benefit of their learning capacity in general. No doubt these are children who are going to have difficulties in curriculum. They may be children who are of average or above average intelligence who aren't learning according to expectancy. This is the group which perhaps furnishes a potential reservoir of maladjustment later on. Right here you have a good screening procedure— at least a potentially good screening procedure— which I think might contribute much to our still nebulous idea of what we call mental health.

Mr. Deutsch: This question is directed to Dr. Barnes rather than to Dr. Brim. I was interested in your—what seemed to me—incomplete discussion of the proper type of approaches to physical diseases and the almost exclusive emphasis on immunization of the individual. I bring up the point because I think, as a layman, that this reveals what seems to me to be a psychiatric bias toward research in general. There are many other public health approaches to physical diseases, such as the clearing of the swamps against malaria and yellow fever, which has practically wiped them out in this country. This would indicate to me the desirability of concentration or stress on the socio-environmental problems in what we call mental health and mental illness. For that reason, I, for one, certainly welcome what some psychiatrists consider the intrusion into the area by sociologists, anthropologists, and statisticians. In the same way one would have welcomed the intrusion of the sanitary engineer into the field of medicine and public health in dealing with the problem of yellow fever and malaria. However, I wonder if this emphasis on working with the individual is not a psychiatric bias against sociological or socio-environmental research and overemphasizes the problem of the adaptation of the individual to society instead of the possibility of changing society; that is, making a mentally healthy society for the individual.

Dr. Hollingshead: I should like to comment on Mr. Deutsch's query posed to Dr. Barnes. Now I've been working with psychiatrists very closely for ten years, medical doctors of other varieties for about twenty-five years. I'd like to point out that psychiatrists are medical doctors in

the first instance and medical specialists in the second instance. Medical doctors are trained primarily in their first two years (and in their pre-medical years) in the basic biological sciences. In past years, certainly in the generation of the older doctors in this room—and I mean "M.D.'s" rather than the "Piled-Higher-and-Deeper" boys—the emphasis was upon biological sciences and there was this heavy organic training. Secondly, when you look at this field of interactional factors operating in a disease syndrome, the general idea of epidemiology, the emphasis in the past from John Snow to the present has been upon the overwhelming etiological agent that infects the human organism or the animal or plant body—so that is the type of epidemiology that has developed.

In more recent years (as we had called to our attention in the comments at lunch) in the first decade of this century, the field of pediatrics had not as yet benefited from the discovery of vitamins. As a result of our learning more about nutrition, a second type of epidemiology developed. In pellagra and in scurvy—if I understand them—there is no invasion of the organism by another organism that is an overwhelming etiological agent. These conditions arise from a subtle, deprivation type of nutritional deficiency. So then we had with the studies of Goldberger on pellagra a second type of epidemiology developing. I believe that we are today on the brink of recognizing the need for developing possibly a third type of epidemiology that is concerned with a syndrome of behavior, which up to this time has defied identification under the electron microscope—and I'll use here, for illustrative purposes, a failure to find the "schizococcus!" Up to the present time we have failed to find the specific nutritional deficiency which gives rise to the effective disorders; or we have failed to find the chemical functioning that may give rise to schizophrenia or to the manic-depressive disorders. Dr. Gottlieb was telling me yesterday that they have some very interesting research going on at his place that may give us some leads—and I hope that you do have some leads there in your enzyme research. But, so far, we don't know.

My point here is this—in defense of Dr. Barnes—remember that he was trained in a generation when there was this heavy emphasis upon the organic. Now there has come the era of nutritional deficiency. I don't think that, as yet, we have the group of medical doctors trained who recognize the possible legitimacy of a maladjustment or disease entity which may arise from failure in communication, failure in the learning process, or this vague thing that we call stress. A new conception, a new way of approaching the problem may be necessary. I think that Dr. Spiegel can speak more intelligently on this from a medical point of view because I feel that your group is in the forefront of developing this type of understanding and conceptualization. Now that, Mr. Deutsch, is a long speech,

I know, in answer to your needling of Dr. Barnes and now I'd better turn this back to Dr. Barnes.

Dr. Barnes: You're supposed to be defending me!

Dr. Helfant: I'd like to make a comment on the plea that our original speaker made for evaluating the learning of specific techniques in relation to mental health. I'd like to move in the opposite direction and say that rather than getting more specific, I think we should get more general. We live in a society, it seems to me, where we specialize in human relations techniques. I have had referred to me, as a school psychologist in the public schools, children who have very good human relations techniques but they are a grotesque caricature of the mental health of the person involved. By this I mean that one can have good human relations techniques and have lousy human relations! I don't think that learning theory, as it has been conceptualized today, really explains the fact that somebody can learn something but yet not use it—not have it, so to speak. And I don't think that we can wait for learning theories that tell us this. I think the epidemiological research that we've been talking about on and off for all day can help us find some answers to these without our necessarily finding the causal agents. I would suggest that our present studies really should be regarded as pilot studies and that we have pointed out some directions as a kind of criterion which I should like to see used. An example would be—is there a program that will decrease the number of admissions to mental hospitals? This kind of broad criterion would convince me much more than, for instance, an educational program that would result in, say, fathers taking more care of babies!

Dr. Rankin: I'd like to ask a question of Dr. Brim. I may be playing devil's advocate because I personally have a great deal of faith in the potentiality of parent education and the education of children about human development; but I wonder just how great is the improvement shown by those studies you have reviewed that tended to indicate positive and significant improvement as the result of teaching. There's a great deal of research done to test theories in which significant findings are obtained, but the actual amount of difference is very slight. This is a practical issue; and although this is perhaps hard to answer, can you give us a better idea as to whether in these studies there tended to be rather large changes? You wouldn't put a lot of effort into giving children special or extra training in arithmetic to raise their scores on arithmetic tests from, say, 50 per cent to 52 per cent and I'm curious to know whether this analogy doesn't apply here.

Dr. Brim: Yes, I'll speak to that point. It seems to me that not for primarily conceptual but for research purposes we need something of the other end of these programs that we can lay the measure on. When I

was pleading for something specific, it was only because in many ways that's more easily measured. Now it might be possible to maintain a global conception of mental health and still measure it, but what would it look like?

Dr. Harris in Minnesota and Dr. Anderson have developed a satisfactions scale in eight different areas of life. It makes sense to me that no matter what your definition of mental health is, if it doesn't somehow also correlate with a person's feeling satisfied, then it's probably not very good. So I would be quite content to use Dale Harris's "satisfactions scale" as a global measure of the effect of a program. If people improve in satisfactions, that's all the evidence I need. Now there are other global measures that have no theory at all, such as rates of admission to hospitals and so on, but are nonetheless good. And really, the proof of the pudding in the end is something like the number of people diagnosed as mentally ill by psychiatrists, or rates of admission, or suicides—as has been pointed out. This is, in the last analysis, the criterion of mental health in our society. But rather than use it for trying to formulate some theoretical conception that intervenes between these, we must break it down in such a way that we can measure—otherwise we get hung up talking about adjustment, or confidence, or whatever.

As an example, I mention the study by Dr. Slobetz and Dr. Lund which shows that youngsters improve in certain segments of California tests of personality. So what? If you score higher on the test, you get along better; but there still is the question of how much of an improvement you need before you say the youngster has had a significant improvement in mental health. Again, you can't answer that question except in terms of these objective criteria, such as Dr. Ojemann's noting of the number of times pupils are absent from school. This is something you can count and that people understand. If we could correlate a little improvement on the California test, a little shift in the MMPI with a 50 per cent reduction in the suicide rate, then we could answer a question; but the way it is now, it's unanswerable.

Dean Peterson: I'm informed that our time is up and that we need to stop. I certainly would like to express all of our appreciation once again to Dr. Brim. I think the various remarks we made about the discouraging aspects of your report were meant out of recognition of the clarity with which you presented this and the truth seems self-evident. I'm sure we can all benefit from such a clear paper. Thank you.

[The following comment on Dr. Brim's paper was received by letter. EDITOR.]

Dr. Blatz: Even in the early days of parent education, the leaders were concerned with the efficacy of their methods. They were con-

sistently looking for a yardstick to measure the results of their work. This educational program was unique in two ways. (1) The student, namely the parent, went directly home to try out the "new" techniques. Usually the student waits for some time, often years, to apply his knowledge (*cf.* the medical student). (2) The leaders were concerned not only with a list of *knowledge,* that is book-knowledge, but whether the student's attitude towards the problem of child training was changed and in which direction.

The invention of the attitude test was the forerunner of attempts to measure parent education efficacy. Brim, in his paper, brought together the results of some of these tests. On a statistical basis these tests did not appear too optimistic, but from the point of view of a continued attempt to submit parent education to this kind of analysis, the paper was very gratifying. After all, the child may learn his arithmetic perfectly in school, but does that mean he will handle his budget adequately when he grows up? Does the medical student with the highest standing necessarily become the doctor of the highest integrity? Does the law student at the head of his class contribute most to the community welfare? These are questions that still have to be answered by some technique other than an examination of overt skills.

Parent education started out trying to answer such questions. There is still much to be done.

Dean Loehwing: It is gratifying to the university to see this splendid turnout. I hope that the sponsors at Bethesda may feel as satisfied as the University does with a conference of this sort. I'd like to hope, too, that our modest but genial host, Professor Ojemann, feels that his labors have been worthwhile. I think we can compliment him on very successful planning, not only Mr. Ojemann but his entire staff.

It is a great pleasure for me to be asked to preside here tonight. Just why Mr. Ojemann asked me I'm not sure. I'm not very conversant with your field but am very much interested in it and shall endeavor to learn all I can. As you well know, the state of Iowa has in recent months undertaken an active campaign in the development of all aspects of preventive medicine. I, for one, am especially delighted to see mental health included in this extensive program. That is why this conference on this campus is timely for us in the state of Iowa and in the University in particular.

To me it's a great pleasure to be able to introduce the speaker and I've noted with interest that she has come back to our campus again. It's a pleasure to have her, as one of our own former staff, come back to see us. I think some of you may know that Mrs. Yarrow began her career in Wisconsin and Minnesota and then came down to Iowa and was on our Child Welfare Research Station staff between 1943 and 1945. I am intrigued by one item in the biographical notes which states that she was an instructor at MIT. It is an interesting thing for a lady to be a staff member of that particular institution. I also noticed that she was at Queens College in Denver as a member of the psychology staff. In her present position as chief of the section on Social Developmental and Family Studies of the National Institute of Mental Health, we will be hearing her speak in her professional specialty. It has been my pleasure to look at her book and I think the title is very intriguing. She is co-author of the book I think many of you must know, perhaps far better than I, *They Learn What They Live,* published by Harper & Brothers in 1952. Believe it or not, graduate deans do prowl the library occasionally, and I've had a chance to look through some of her other publications and they're very interesting. It is our pleasure, then, to present to you Dr. Marian Radke Yarrow.

CHAPTER V

Next Steps in Research

MARIAN RADKE YARROW, PH.D.

Dean Loehwing, ladies and gentlemen. Certainly it's very much an honor in the First Institute on Preventive Psychiatry to be asked to talk to you on "Next Steps in Research." I should tell you, however, that I didn't choose this very grand title and I feel a little presumptuous in talking under this title. I have sought a more modest one but haven't really come up with a good one. But I think that's perhaps excuse enough. There's something unique about the position at the end of a conference. You come to it with a prepared paper as you were instructed to come. Then you listen all day to various conclusions that are drawn and various emphatic statements that are made. You review your paper and you remember how you said something quite different and then you also think—now that person is really going to be mad when he hears this. You're tempted to start revising, but, let me tell you, I have been brave enough not to make any such revisions. But, as I said, I have been tempted.

None of us, I suppose, is content with our present fund of scientific understanding concerning the development of mental health and mental illness, and all of us share motivations to contribute to increased understanding through concentrated and improved research efforts. Whether we are considering "secondary" or "primary prevention" (as the concepts have been used in this conference), we have progressed just beyond a good beginning in research. Hence, it is very reasonable that we should give some thought to future possibilities and to assessments of the present status and directions of research.

I should like to examine with you some aspects of research in primary prevention. A delimitation of research problems is in large measure a function of personal orientation and experience. My choices will reflect this, but I hope they will touch on your interests as well. I shall be concerned mainly with *developmental problems in preventive psychiatry.*

I think, sometimes, that in the many opportunities which we make for ourselves, in our professional meetings and publications, to point out the needs in future research and the imperfections of past and current work that we may be demonstrating too well and too clearly the principle of substitute activity and substitute value. I should like to think that in the discussions of today and this evening, we are not primarily engaged in

ıbstitute behavior, but that research will be generated which at a future ıeeting might be discussed for its discoveries and failings.

In our concern for the specific problems and techniques of research we :nd often to overlook characteristics of the "institution" or "culture" of :search in which we are functioning and the possible effects it may have n the choice and conduct of investigations. At a time such as this when 're are examining trends and needs in research it might be enlightening) take brief notice of the *institution of research* as well.

Sociologists have done much to point up the importance of institution-lisms in the conduct of individual behavior; how, for example, the social ructures (formal and informal) within the mental hospital determine ıany aspects of patient behavior—the roles available to him, the positively r negatively sanctioned values and behavior, the modes of defensive nd offensive maneuvers he learns to employ, etc.

In the mental health field, we may ask if the nature of research as an ıstitution has influenced the directions of inquiry; how, if at all, the re-earch culture has accounted for major advances over time, or has had a earing on various stabilities and shifts within the field.

Contradictory as it may seem, our research culture is highly conven-onal and rigid, and established conventions would appear to have done ıuch to account for some of the unchanging aspects of research activity. Thus, the need to select problems clearly accepted as good and prestigeful probably not too entangled with social values), the need to adhere to rescribed techniques, the need to produce a certain number of publica-ons with other than negative findings have motivated researchers. In ıis process, both problems and techniques tend to acquire a status, and status quo. As we well know, some have high prestige, others are of ›wer caste. Some problems acquire a mold and continue with little hange in conceptualization or techniques over a number of years and in ıany, many, many studies. May I pin down a variety of instances in which ıis kind of rigidity or custom has had an effect?

Research on the long-standing question of where we can find the origins f adult personality provides one illustration. It is conventional to formu-ıte the problems, whether in prediction or postdiction, in terms of early xperience (usually in relation to the mother) and an end point much later ı adolescence or adulthood (such as relationships between weaning and ›ilet training or more recently between independency training and out-omes in personality). Time after time a gross phenotypic relationship ; investigated, the how and why of the relationship remaining in con-entional obscurity.

A similarly uniform and equally ubiquitous design is known as studies f parent-child relationships. Here it is the custom to obtain verbal re-

ports from parents on their child-rearing ideals, attitudes or practices, either current or of years ago before the child became delinquent, schizophrenic, or famous, and to relate these variables to variables in the child. Studies of social influences upon personality development give another example. In study upon study, families or individuals are sorted on the basis of a social variable (such as ethnicity or class). Mean group differences are reported in monotonous procession for one variable or another. "Explanation" usually ceases with the report of significant differences. In studies of learning, there are similar research conventionalities: experiments on reinforcement have uniformly used food or shock punishment and dealt with response in an all-or-none fashion. The illustrations could be increased. In each of these examples, alternative designs come to mind. Why are they not tried?

But, you may say, we have been working with the problems mentioned for many years and have not reached certainty. Why be concerned that we are continuing to work on these unknowns? I do not wish to be misunderstood. There is a legitimacy in each of the problems as formulated. I am questioning, however, how much traditions and sanctions and needs for safety in research culture keep us in too narrowly bounded paths. It is as if, presented with the complex ink blot of possible relationships to study in individual development, we are overtrained to make the "popular" response in devising an investigation. There would be less cause for distress were it possible always to see more clearly a *progression* over time within a given formulation and approach. Such progress might be through careful genuine replications of given studies; (How often do conclusions and theoretical structures rest upon ten clinical cases or forty preschool children?) or through successively truly more incisive and analytical studies built on preceding ones. The present tendency (granting, of course, some significant exceptions) is not so much this kind of progress as it seems to be relatively haphazard choice of variations on the given themes.

We come then to a question of how significant advances in research come about? I am not prepared to answer this question or to suggest that it is determined entirely by research culture. However, if you will call to mind studies which have had the effect of re-focusing research, and of leaving long lines of investigations in their wake, (Ex. Lewin and Lippitt, *Autocracy and Democracy*[1]) you will often find them to be studies which have not adhered to the expected or the entirely sanctioned. Often these studies are later demolished shred by shred (and legitimately so) for every conceivable kind of omission. (Ex. Spitz, study of hospitalism[2]) Yet their contributions remain significant by what they have effected in later investigators.

It is difficult to draw a conclusion from this. Mainly I shall avoid one, except to suggest that we need to cultivate the kind of research culture that makes it easier for informed, creative exploration, harder for naive "pilot" studies (which is one way of saying, "This is a sloppy study, but, remember, I said I was only exploring"), easier and more valued to verify or replicate the apparently significant studies, more difficult to be uncreative.

One further comment on research culture: We have (if I may sound political) our party affiliations in research. Parties have their traditional problems which tend to confine their developments to given areas. Thus, to my knowledge the experimentalist party (right wing) has rarely brought its skills to bear on questions such as social influences on personality. Only recently has it begun to study some of its traditional rat problems using children. The clinical party, on the other hand, seldom has students of learning or perception. This kind of party concentration, while it has much to recommend it, tends to intensify the special shortcomings and blind spots that accompany any single approach. It does not always work best toward the development of a theory of behavior and development.

Fortunately, there are evidences of change, in this regard, in various areas of mental health research. To cite but a few examples, research on the therapeutic milieu (the treatment setting of the mentally disturbed child or adult) has brought clinicians, sociologists, and psychologists to common problems. Infant studies have begun to attract experimentalists. Psychologists and psychiatrists are showing increasing analytical interest in the family, an area of research traditionally the concern of sociology alone.

Collaborations such as those cited have been fruitful. They have advanced understanding usually by casting an old problem in a partly new light, and giving methods untried in a given area a try-out. This is not a plea for interdisciplinary collaboration for its own sake. I would offer the hypothesis, however, that we may often look to the informed newcomer to a problem area (newcomer by virtue of discipline, theory, past concentrations) for advances, which then require more careful follow-up by others.

I should like now to turn my primary consideration, *to discuss research on children's learning environments, natural and experimental, and the processes or mechanisms by which these environments affect individual personality and behavior.*

The assumption that personality or quality of mental health is significantly shaped by the social interactions in which the individual participates from infancy on through life is readily accepted. From accum-

ulated research we have abundant evidence of this general relationship. We look for refinements in these relationships, however. The reasons need be reviewed only briefly: As the etiological significance of specific environmental or situational factors in individual development has been investigated, we find that we have established only tenuous relationships and frequently our efforts result in contradictory findings. We have become increasingly aware that even conditions in childhood in which extreme deviations exist (such as maternal deprivations and severe traumata) do not always result in personality outcomes in line with theoretical predictions. The cases of war-induced conditions of privation and psychological assault are other examples in which "recoveries" have occurred where they were not expected. On the other side of the coin, and defying explanation, are the children who despite "good" upbringing (in the popular vernacular) turn out badly. As we look at present knowledge concerning the nonpathological we are well aware there also that we must arrive at many conclusions by way of inference from the pathological. When we are called upon to prescribe corrective or preventive measures, it is impossible to come forth with adequate programs. These are some of the unknowns we want research to solve. What are some of the possibilities which might be explored?

A fundamental need is to give up some of the simplicities in research. We have oversimplified children, parents and families, environmental variables and personality outcomes, too, in an understandable eagerness to formulate sufficiently precise and limited designs. Our need now is for studies in which more detailed data are sought and more complex variables and designs are worked out. By considering each of these oversimplifications, in turn, we may arrive at a number of research prospects for the future.

First let us consider how the child has been oversimplified in research. In studies of antecedent environmental variables and personality outcomes, children have been regarded as essentially alike. It has been assumed that knowing the environmental variations (the socialization practice, the teaching given, the parental affect, etc.) to which the child was exposed is sufficient to predict systematic differences in response, immediately or distantly in children's lives. The failure to attend more closely to the child as a variable is conceivably the source of many of our troubles in prediction as well as treatment. There are several directions to look to in rectifying our current neglects of the child qua child.

(1) We might concern ourselves more seriously with the genetic, constitutional and maturational factors in the child in interaction with social experiences, i.e., not in isolation as in much earlier research, but as a modifying set of variables to be taken into account along with sociali-

zation variables. Historically these biological factors were credited with considerable significance in development. But as hopes for clear-cut relationships between constitutional types and personality dimmed and as the effects of situational and environmental conditions were documented and varied experimentally, the genetic and maturational variables receded in research, particularly in the mental health field.

It is not surprising that in the recent resurgence of interest in infant studies a renewed interest in constitutional factors would appear there. Escalona[3] was among the first to bring this emphasis into systematic data collection. She pointed out the distinctly different behavioral repertoires and sensitivities among newborns. These findings and the immediate enthusiasms about them seemed to open new and promising possibilities. Thus far, few results have been reported. However, several infant projects now in progress have taken the constitutional variable into account. We will have to wait for their completion.

From a theoretical standpoint this kind of refined evaluation of the infant should help us to approximate more closely the infant's *experienced* mothering, and thus provide us with a better basis for prediction. This kind of close and step by step investigation not only in the initial months but continuously in the ensuing years could give us important clues as to the significance of the constitutional variable in studies of personality.

Other biological considerations have begun to move into investigations of personality development. Influenced by animal experimentation, the concept of critical phase, (the organism's vulnerabilities to given kinds of influences at specific stages in development) is being applied in studies of infancy. Also, biochemical effects, etiologically and therapeutically, have assumed a greater role in research on personality pathology.

I am sorry that my illustrations deal mainly with infancy, where the genetic and physiological seem most evident. I am really suggesting that this class of variables might be more seriously considered throughout development.

(2) From another and quite different point of view, I should like to suggest greater recognition of variabilities within the child. This is a consideration of the subjective or psychological structure of the situation for the child. It calls for investigation of the child's learning environment (in the family or in society) at the level of the child's perceptual or cognitive understanding of it. Seldom have such data been obtained in seeking understanding of the steps in the relationships between rearing or training environments and behavioral and personality outcomes. Instead we act on the assumption that adult behavior and intentions in handling the child are perceived or experienced by the child in the form they exist for the adult. For example, are the love-motivated and avoid-

ance-orientated disciplinary techniques employed by the adult (to use a very primitive illustration) so experienced by the child of any age? It is hard to assume that social learning as it occurs in the "undifferentiated" infant is the same process as at a later stage of development.

Are there developmental maturational stages in learning behavior? In perception? How do the discriminations of social interactions of the three-year old compare with those of the ten-year old? I venture that we cannot give unequivocal answers to these questions. Are there not needs and possibilities for seeking out this kind of basic developmental data in mental health research? To confine ourselves to only one example of data which certainly have value in application, we have the familiar work of Piaget and his associates,[4] in which they have demonstrated various characteristics of children's perceptions of causation. They have shown changes or stages in children's causal thinking with maturational level. More work is needed before the data are entirely clear. However, the knowledge that we have should make us pause and ask what may be the implications with regard to the child's socialization or training. The child's sensitivity to the adult's motives, the child's modes of thinking about causal interpersonal relations will surely play a role in the learning that takes place at different developmental levels. The study and application of this basic side of child development deserves exploration within the research on personality formation. It would be very interesting, for example, to attempt to predict from knowledge of developmental data on children's causal thinking and interpersonal perceptions the kinds of insight achieved by children of various ages in the studies of Dr. Ojemann.[5]

Having dwelled on the maturational aspects of the child's cognitive field, I would not want to leave the idea that variations in children's perceptions of interpersonal relations are entirely maturational differences. In some of our own current work with children, just pre-adolescent, the varied nature of their thinking about other persons and their varied sensitivities to others' motives for behaving are wide indeed within any age. This leads to the fascinating question concerning not only how maturational levels may affect the child's "intake" from the environment, but at the same time how different kinds of learning environments for children may materially influence the development of differential sensitivities to interpersonal relations and intrapersonal dynamics. It is at this point that the introduction of experimental studies would be most important. We will come back to a consideration of experimental learning situations in later discussions.

My earlier proposition that research in preventive psychiatry needs "complicating" suggested this not only with regard to the child. It applies as well to a second set of variables in children's learning environments—

he family. Studies dealing with child training and family relationships ıave met with a tremendous barrier to adequate research. The family n our society, except in unique circumstances, is not believed to be open o direct scrutiny. Therefore, we have substituted for primary data in ›ur research the means most available: parents are relatively willing to alk about their children. This has been our avenue to the family.

Our difficulties lie not only in finding means of access to families but, s mentioned earlier, in having so greatly concentrated on mothers' re›orted behavior and feelings toward their children. Ackerman[6] has pointed o the error of these ways by emphasizing the larger reality that impinges ıpon the child (upon each member of the family), and which should be ntegrated into child-rearing research designs. He reminds us that the elevant family variables include maternal behavior, maternal personality, I would like to add paternal as well), marital and parental relationships, ınd the psychosocial structure of the family as a group. Also, I would ike to add the behavior and personality of the child. All of these con:titute the influences of the family; any one of which alone gives us a imited picture, indeed. Perhaps only the psychiatrist in the process of herapy with a particular disturbed family relationship is likely to be at:uned to these multiple influences. Our research designs have not been so attuned.

How can the necessities which Ackerman's analysis suggests be made workable in research? As a first step we might make a conscious shift in problem formulation from *parent* to *family*. We might reorient our thinking (at least partly) from family *effects upon* the child to the child's *interaction with and adaptation to* the family learning environment.

In considering the multiple family influences we could be totally overwhelmed in attempting to encompass them simultaneously in a single design. To save ourselves in part from such an experience, it will be necessary to make full use of current theory and empirical data to arrive at meaningful clusters of variables to be studied—within each dimension and cutting across dimensions. It will be necessary to design family studies for purposes of hypothesis testing, hypotheses which deal with complex relationships. Only in this way can we approach the family variables which the clinicians and our own experience outside research have made real to us.

As a concrete try-out of such research one might start with the presumed schizophrenogenic mother. This problem has stimulated the necessary varieties of research which we could draw on in designing our study. Our purpose would not be to investigate schizophrenia, per se, but to develop a study of family-child relations. Our hypothesis sources would be the questionnaire studies of child-rearing practices recalled by

mothers of schizophrenics, the psychiatrists' many accounts of family interaction and parent personalities in cases of schizophrenic offspring, the sociological studies of class status and mobility and social isolation among schizophrenics. Comparative groups, pathogenic and nonpathogenic families, should be studied with identical procedures.

In dreaming of such a study, or of others, in which we heed Ackerman's appeal for multiple variables, we are face to face again with the question of accessibility of data on the family beyond tests and interviews. Unless we add observational data, I am of the opinion that we can progress only a limited degree. I feel we can obtain such observational data on the family. We can do so without ethical compromises.

Let us review some of the instances in which entry into the family has been achieved for limited purposes. Some of Roger Barker's[7] observations of children have been done within the family itself. Current studies of infants are being carried on in family settings. In a number of studies of families in crises (ranging from mental illness, tuberculosis, birth of a premature) the investigators over months of interviewing achieve a kind of relationship within the family that opens to them a wide range of natural family interactions. Strodtbeck[8] has recently developed a technique whereby the investigator confronts the family with a problem for their joint solution. This process provides him with certain dimensions of family interaction. An ingenious idea being explored by another investigator is that of getting the entire family into sessions involving projective materials. In a number of therapy programs that involve child and parents in psychiatric treatment, coupled with home visits by social workers, and with various contrived situations for observing the family group together at the clinic, the family begins to be seen in the various frameworks which fill out a total assessment picture.

Finally, a number of professional roles carry with them unique kinds of accessibility and acceptability to the family, for example, the public health nurse, the pediatrician, the teacher. The opportunities offered through these roles deserve exploration.

It is tempting to elaborate further on the family as a learning environment, but I should like to give time to nonfamily and experimental learning environments as well. The two other problems I want to post in regard to the family apply equally to the other settings.

The first comes from an experience I have had recently in a research project in gerontology. I have been attempting to arrive at a framework for characterizing the daily life space of the aged person. I found myself considering his family relationships—the manner in which the younger generation regarded and responded to him. It soon became evident that I could not proceed on these family assessments without taking into ac-

count the characteristics of the aged person himself. Evident here, but is it evident in family assessment where a child rather than a very old man is concerned? The same point was brought home recently with children. In adoptive placement the child's first home, prior to his adoption, is with a foster mother. This "mother" cares for one or several infants at a time, but in the course of several years cares for many infants. There is a recognizable and stable style for each foster mother over the years. Beyond this there are also markedly different behaviors which appear in the same mother in caring for different infants. The point for family-child studies is apparent; the problem formulation is "interaction between" family and child rather than "behavior toward" the child.

My final point on the family: earlier in discussing conventionality in research designs, I mentioned the frequency with which an early childhood situation is paired with a later childhood prediction. It is an example of our repeated practice of measuring the family as if it were a static set of relationships. We know this not to be the case. The parent changes as does the child. Marital relationships may change. There may be vital social impacts upon the family. Reciprocal parent and child needs at one age may not hold at another. An illustration often used is the mother who finds the dependency of the infant rewarding but rebels at the independence of later childhood.

Changes within the family raise the question of studies of *continuity* in the child's learning environment. Perhaps at least some successive measures at intervening points between antecedent and consequent might be introduced. In lieu of extended longitudinal studies, at our present stage of groping for better data, it would seem wiser to look at continuity in family-child relations over a limited time span, sufficient to see the child over adjacent but significantly different developmental periods (such as from infancy through the early establishments of independence in and outside the home; or from pre-adolescence into adolescence).

No one as yet deserves an "Emmy Award" for the study of family-child relationships, although the enlivened air of critical enthusiasm and activity in this field recently shows promise.

Outside the home the child is in many learning environments. There is much to study here. Undoubtedly the process of integration into society (successive societies) throughout development has a great deal to do with individual mental health. Yet systematic data collection and theory are not advanced. We are quite poor, for example, in concepts for dealing with the effects of the child's renewed struggles for self identity when he is no longer entirely dependent on the home, or the effects of competing or contradictory adult models.

Our concepts in this area are mainly group-oriented (such as social

role, status, belongingness, etc.), not personality-oriented. Even in personality theories in which the general significance of extra-family influences are recognized, there is relatively primitive development of specific hypotheses.

Systematic exploration of extra-family socialization might, for example, be thought of in terms of parallels to formulations of socialization within the family. Investigation of the handling of children's dependency, responsibility and achievement needs, affection and aggression among peers and by nonfamily adults in various segments of our society would provide a systematic start in a big area in which present information is only patchy.

For the remaining time I should like to discuss experimental environments or situations through which basic analyses of cause and effect relations can be made.

It is customary to speak longingly about a genuine experimental attack upon developmental problems. Oddly enough, this ideal and the practical necessity for developing mental health programs have a meeting ground. As we well know, present corrective and preventive measures in the mental health field are inadequate to deal with present problems. Without attempting careful analytic survey of present measures, I shall risk opposition with two generalizations about measures now directed toward prevention. Adult education (which subsumes varieties of measures intended to influence human relations) has been notoriously weak in theoretical bases of operation and in research designs which encourage systematic procedures and sound evaluation of results. It has not produced a body of knowledge of positive effects to rely on. The second generalization concerns the intensive, theory-rich psychotherapy. Its application to large populations is obviously not a possibility. Where individual insight therapy has been used with out-patients drawn from all segments of the population we are again confronted with disappointment.

The experimental try-out of alternatives would seem to have exceedingly high priority in preventive endeavors. It is a brave spirit, indeed, that starts out on this venture. His first decision must be between an experimental program and an experimental variable: then he must make a choice of goals he wishes to achieve. Whichever his choice, he must, as a further requirement, ground the program or the variable and the goals on theoretical considerations.

If our investigator gives his social welfare aims the edge in his decisions, he is likely to choose a program over a variable, and to choose a community, a school, and the like as his laboratory. For our consideration I shall give the edge to the program, too. This is not because I am ready to make a case for the advantage of one over the other, but because the

unique and difficult research problems are more abundant in the experimental program. And because I feel, too, that programs will be and need to be tried in research.

The goals of such a program are likely to be a direct reflection of a social need, and hence are likely to be formulated in rather startling magnitude. Here is a potential array: to develop better citizens or parents, to develop attitudes and understandings which make for a better social order, to develop better interpersonal relationships, to increase self-understanding, to prevent psychiatric problems from arising, to decrease delinquency, etc.

To see these goals in proper perspective for research purposes, let us consider what our goals might have been had we preferred a variable to a program. We might have chosen to increase children's sympathetic responses or their assertiveness, or to decrease their autocratically dominative interactions. Each of these may represent a tiny segment of the program goals.

One of the problems in developing an experimental program soon becomes clear, that of translating a social goal into sufficiently specific workable research goals. The latter must be stated in concrete behavioral or attitudinal terms or in operationally defined concepts. This must be (1) if the antecedents (the program experiences) are to be arrived at systematically, and (2) if there is to be optimal evaluation of what is being accomplished by the program. How might one proceed then to close the gap between social program planning and scientific ideals of theory and hypothesis testing? It is possible to do so.

Unless one has the grand theory (and I think there is none), one will have to begin more humbly. Perhaps with more naiveté than quite necessary, a *first* step would be to translate the social goal into behavioral or attitudinal parts. Let us say our *program goal* is improving human relations or preventing delinquency. Our translation process would probably result in a long and heterogeneous enumeration of specific goals: developing increased sympathetic responses, decreasing autocratic behavior, reducing anxieties, developing alternative defense mechanisms, etc. *At this point* attempts to be strictly systematic should have no place. One should be allowed to dream creatively.

With an unreasonable list of specific behavioral and attitudinal change objectives, we would come to a second step in program plans. Here we would be required to organize and reorganize our specifics into some *relatedness*, an organization developing, loosely at least, a theoretical structure. It would be obvious at this point that many items on our first list have no place in the final scheme of objectives. This organization of specifics would have accomplished our first link with theoretical bases.

From this approximation we are ready for another step, another link with established scientific findings. Knowing that we are setting out to accomplish "X," "Y," and "Z" and that these variables bear some relation to one another, we can turn to data from the clinic and laboratory for guides in suggesting the kinds of experiences, procedures, or content needed to achieve these specific outcomes. We are now in a position to formulate specific hypotheses regarding program procedures required to attain these outcomes. Refinements of hypotheses may go further; for example, with what segments of our experimental population will given experiences have differential effects?

While the general structure of the experimental program, by means of this kind of planning, can be derived from basic research, there are many determinations with regard to specific procedures for which there are no guides. How much experimental treatment or intervention is required to achieve a given modification? Exactly what kinds of teaching materials or settings will be most effective? Answers to many of these questions are largely a matter of empirical testing.

Measurement of the effects of an experimental social or psychiatric program is now taken for granted as a necessary part of research. However, we still live with the tradition that in program evaluation we can or must settle for vague and impressionistic ratings of degrees of success of the program, degrees of improved general adjustment, final goodness or badness of home environment, and the like. Unfortunately, this procedure is not only part of the history of program evaluations; it is in many instances still the practice. For a program of several years duration, of carefully planned and complicated procedures of intervention, to end in a puny rating is a tremendous loss of research information.

It is probably true that the consternation which often comes at the end of an experimental program arises from the fact that the initial social goals were not, at the outset, translated into research variables (as outlined earlier); also from the fact that an appropriate assignment of professional skills was not made. The persons best able to carry out the therapy, casework, or teaching need not be called upon to conceptualize the problem and to develop systematic measurements of outcomes.

Assuming that the program has been developed in a way that specific variables are identified in the initial design, there are a number of possibilities and challenges for enriching evaluation procedures. The dicta of good experimental design (matched groups, before-and-after tests, etc.) need not be elaborated. I am again pleading for data of greater detail.

(1) In treatment or teaching going on over an extended period, and in which we are carefully controlling the input, we have an ideal oppor-

tunity for observing or deducing the steps along the *way of change*. A terminal evaluation coupled with the intervening processes provides considerably more powerful information on antecedent-consequent relationships.

(2) We can build into evaluation what is essentially a limited replication of the program by relying on several complementary sources of measurement. I am not suggesting the uninhibited use of batteries of tests that happen to exist. Sometimes, of course, a standardized test may deal with precisely the variables studied. This will have to be the guide to its use or rejection. The complementing of evaluation measures could, for instance, be based on different levels or aspects of response: behavior under various conditions, affects, goals and incentives, and so on. This practice has been followed, for example, in the Iowa experiments on causal teaching.

In the use of multiple evaluations, one caution might be mentioned in the use of self-assessments. The appropriateness and the meaning of conscious self-appraisals should be considered carefully. An example that is mentioned by Allport[9] in discussing the Somerville study of delinquents points to some of the possible difficulties. This study showed in numerous ways the prognostic significance of relationships within the homes of the delinquents. However, when the delinquents themselves were questioned about causes for their difficulties, none mentioned the role of the attitudes of his parents.

(3) Evaluation should be an individual as well as a group measure of change. Averages are not enough. Following the individual has the advantage of permitting us to trace the bases for modifications that take place as well as the reasons for no effect or negative changes. We may in this way discover segments or subgroups within the population (of differing maturity level, personality, cultural origin, family environments, etc.) for whom the experimental environment has quite different effects. From these more individually focused and continuous assessments we may hope to draw more specific hypotheses for testing.

I should like in closing to stress once again the great contribution which can be made by experiments in environments, or as Dr. Ojemann has very appropriately phrased it, experiments which go a significant step beyond cultural anthropology. We have braved the difficulties to cultural complexity with systematic investigations of existing culture patterns. We need equal efforts and imagination directed toward studies of experimentally created "culture patterns."

Research is a fascinating and serious business, controversial and ego-involving. Only a fraction of it from a much larger output brings yields upon which a field of knowledge moves ahead. We are all motivated

to increase the fruitful yield. In discussing together what we know and what we want to know in the field of preventive psychiatry, I hope I have been fortunate enough to suggest some ideas as well as to ignite some controversies or disagreements which will lead to research activity which will be fruitful.

REFERENCES

1 Lewin, K.; Lippitt, Ronald; White, R.; "Patterns of Aggressive Behavior in Experimentally Created 'Social Climates,'" *Journal of Social Psychology,* 1939, X, p. 271-99.

2 Spitz, Rene A., "Hospitalism. An Inquiry into the Genesis of Psychiatric Conditions in Early Childhood," in the *Psychoanalytic Study of the Child,* Vol. I, 1945, p. 53-74.

3 Escalona, Sibylle, "Emotional Development in the First Year of Life," in *Problems of Infancy and Childhood*, 11-92, Senn, M., Ed., Josiah Macy Jr. Foundation, 1952.

4 Piaget, J., *The Child's Conception of Physical Causality*, Harcourt, Brace and Co., 1930.

5 Ojemann, Ralph H.; Levitt, Eugene E.; Lyle, William H., Jr.; and Whiteside, Maxine F.; "The Effects of a 'Causal' Teacher-Training Program and Certain Curricular Changes on Grade School Children," *Journal of Experimental Education*, 1955, Vol. 24, p. 95-114.

6 Ackerman, Nathan W. and Behrens, Marjorie L., "A Study of Family Diagnosis," *American Journal of Orthopsychiatry*, Jan. 1956, Vol. 26, No. 1, pp. 66-78.

7 Barker, Roger G. and Wright, Herbert F., *Midwest and Its Children*, Rowe, Peterson, 1945.

8 Strodtbeck, F. L., "The Family as a Three-Person Group," *American Sociological Review*, No. 19, 1954, pp. 23-29.

9 Allport, Gordon, "Foreword" to *Experiment in the Prevention of Deliquency; The Cambridge-Somerville Youth Study*, by Powers, Edwin, and Witmer, H. L., Columbia University Press, 1951.

DISCUSSION

Dean Loehwing: Thank you, Dr. Yarrow. To me, as a nonprofessional in this audience, it's been very inspiring to hear this outline of novel and aggressive approaches to experimentation in this area.

I confess I feel as though I were myself the guinea pig here tonight in one experiment. I believe that this is Dr. Ojemann's way of educating a member of the administrative staff, and I'm gradually getting the hint. Perhaps when he and Dr. Huston and some of the members of the Child

Welfare and Psychiatry staffs visit the graduate office, this may pay off. It is gratifying to hear sketched what has already been done and the opportunities which experimentation in the field of psychiatry offers. I refer here to the fact that the times are fairly prosperous and, as far as I am able to see from the post I occupy, funds and facilities for the training of personnel are being made available to implement, on a scale never heretofore possible, some of the things which have been outlined and proposed this evening. I think that you must share with me the feeling that the public and our public officials are becoming more rapidly alert to these needs and to the necessity of action on their part; and those of us in educational institutions wish to do our part as well as those of you in the clinical, institutional, and governmental services in this area.

In introducing the gentleman who is to guide the discussion for the remainder of the evening, I was interested to know that he started on the West coast. I told him before dinner that I was intrigued by the fact that he began his educational career, at least in the college level, at Reed College at Portland, Oregon, an institution I believe all of us have heard a great deal about. He told me, however, that though Stanford is his actual Alma Mater, he has gradually gravitated eastward, and he's finally made the East coast. It was a long and interesting journey, with the war catching him about midway. As for his very interesting career in the military service, I think that he is to be commended. He served in the Adjutant General's Division of the U.S. Army and was also an Information and Education Officer for the War Department during the period of hostilities. He said he didn't have any wound stripes, but he must have done pretty well to be awarded the Bronze Star medal.

In his biography I also noticed something that aroused my sympathy, and I wish him well. He's recently taken on an editorship, that of the *Journal of Abnormal and Social Psychology*. I wish him good luck in this enterprise. I see, too, that he is an author of considerable note. He is currently professor in the Graduate Department of Psychology at New York University and is also vice-president of the Joint Commission on Mental Illness and Health. It is a great pleasure to present to you Dr. M. Brewster Smith, who will conduct the discussion.

Dr. Smith: Dean Loehwing, Mrs. Yarrow, ladies and gentlemen. After Mrs. Yarrow's very wise and very rich and, I think, extraordinarily constructive talk to us about the problems of the "Next Steps in Research" in this difficult and important field, I'm not filled with the spirit of controversy that she was inviting. I feel very much in the spirit of saying "amen" to a great many of the things she has said. In fact, I think the most constructive way in which I can launch our discussion would be by saying

"amen" to several of the things to which I listened with particular enthusiasm.

Mrs. Yarrow started out, you remember, by warning us against complacency and following accustomed folkways of the research culture and to that I would certainly say, amen. I can say this, I think, with more enthusiasm and more bitterness since this journal editorship which has been mentioned than I would have said before I read some 447 manuscripts last year!

Secondly, she urged us—and here I think a note of controversy does come into the picture, but not with me—that we ought to keep things complicated. Here, I think, is a point where perhaps there are diverse strategies which have come to light during the course of the day. I believe Dr. Gottlieb and Dr. Brim in various ways have suggested—at least I understood them to suggest—that we should find ways of making the problem simpler, whereas Mrs. Yarrow has urged us rather toward a strategy of complexity. Now I think here, perhaps, the issue may be overdrawn. I think we probably all agree that the facts that we have to cope with, the phenomena that we're concerned with, are utterly complicated. It doesn't necessarily follow, from this appraisal of the facts, that the most effective research strategy will be one which tries to approach in a complex way this complex area of facts. I think there is legitimate debate as to how we can best grapple with this immensely complex situation.

Thirdly, and I think most helpfully, Mrs. Yarrow has given us a number of positive suggestions and illustrations of her own sense of productive, useful strategy in this research area. Here she has talked about two quite different kinds of research interest. In the first of these, where she was talking about what we might call basic research on children's learning environments and on the ways in which these have impact on developing personality, she joins forces with a theme which ran through a number of comments earlier in the day. We heard Dr. Spiegel, Dr. Hollingshead, Mr. Deutsch, and others express the view that research in this field has paid insufficient attention to the social and environmental side of personality, particularly with the history of the psychiatric approach to the overlaying emphasis upon the individual and his pathology. The counterbalancing stress on this social and environmental side is a corrective which, I think, many have expressed rather strong agreement with in the course of the day. What I note here in Mrs. Yarrow's presentation is an aspect of considerable progress over the way these things were being discussed not too many years back. Although she is stressing social and environmental factors and although she represents a very productive and exciting laboratory on social and environmental factors in mental health, she is not talking like a social environmentalist. She is inviting us to study constitutional variables, to

study biochemical variables, in interaction with these socio-environmental factors which are her own major center of research interest. To me this is a major step in a cumulative maturing approach to the field that we're all concerned with.

The second of the kinds of strategic research problems that she talked about, that having to do with experimental programs and the evaluation of them, seems, at first glance, to fall outside of the basic research—that is, the effects of learning environments on personality development—and yet she gives this research problem some length and gives it very explicitly in her stress on the importance of translating or conceiving program goals in these experimental mental health programs in terms of psychological variables. In other words, the importance is stressed of using what theoretical resources we have for the conceptual guidance of what we're doing. Theory —and I agree that this is one of the major aims of her paper—theory gives us the handle, it gives us the tool for grappling with complexity, whether we're dealing with the apparently simpler, more tightly formulated studies of the specific effects of particular environmental patterns or whether we're concerned with evaluating more global programs. I think one might paraphrase this by saying that she is advocating a causal approach to mental health—in Dr. Ojemann's terms—rather than simply the study of blind correlations between variables more extrinsically conceived.

By way of introducing the discussion, I thought I would talk just a little longer and take this privilege of raising two or three random issues that came up during the day, doing, in effect, what Mrs. Yarrow gave up doing in not revising her paper after the day's discussion and thereby see if we can put before us a very few additional issues bearing upon the future of research in this field.

One problem that came up in the first session this morning—and I think it is latent and implicit in much that we talked about later during the day—is the problem I still think we can't quite dismiss of what gets put into this chapter headed "mental health." I think the solution Dr. Ojemann proposed that mental health is in the nature of a chapter heading rather than of a concept which we need clearly to define is a helpful one; and yet I think we cannot entirely evade some protracted concern with what belongs in this chapter. I think we can evade it, and quite properly so, with respect to the kind of studies that Mrs. Yarrow was first discussing where we're concerned with the effects of various contacts upon personality development. One must study personality development, but one can study various variables or facets of personality development without ever using the term "mental health," and one is not greatly hindered by avoiding the term. When, however, one comes to the second kind of studies that Mrs. Yarrow was discussing with us; namely, these experimental programs that

we find so interesting, which are aimed at achieving certain specificially global effects in the realm of mental health, then I think that we're faced quite frontally with what we think belongs in this chapter.

I'm reminded here of a very provocative point of view that the critic and humanist Lionel Trilling was expressing in my hearing not too long ago. He was citing the cultist or almost religious aspects of the mental health movement, which have been noted before during the day. From his standpoint, as a defender of the human values, he was arguing that he would much prefer to see a restricted conception of mental health—"positive mental health"—that did not try to embrace under this one glamorous tent all the ideals and values and aspirations of humanity. He would like to be able to say that this is a fine creative poet, but his mental health is terrible; or, that this is a very healthy individual, but he is uncreative and immoral. Trilling abhorred this trying to comprise all the values we believe in under this new and currently glamorous heading. This issue, which faces us with—quite frankly—philosophical values, is one that we come up against when we're trying to evaluate the effectiveness of particular mental health programs.

A second point of needed research came out clearly in Dr. Hollingshead's fascinating presentation this morning and in the ensuing discussion; namely, the need for data on the true prevalence of mental disorders. In the context of our concern with prevention and with positive mental health, I would add that we need some approximation to this kind of epidemiological information about the conditions and the associations of healthy, positive personality development, as well as the occurrences of failures and disease. This is a topic which is, I think, very tricky to study. Certainly there are people who have been much concerned with studying creativeness, the flowering of personality, etc., who have brought under their conception of mental health all the good things of life and who have, perhaps, lost grasp on any concretely investigable phenomena. I would suggest this, too, as a frontier where we need constructive advances.

Finally, I would single out for special comment a need which I think all of us have been concerned with in this area of empirical research, and that is the generally unsatisfactory nature of the indices which we find that we have to work with, whether they be so-called objective indices—such as attendance at class or hospital rates, etc.—or whether they be indices of psychological variables provided by standardized tests or by newly constructed instruments. It seems to me that our research needs with respect to indices may well be considered as part and parcel of the need for a more conceptual, a more clear-headed theoretical approach to research in this area of the sort that Mrs. Yarrow has recommended. The

index problem is simply the other face of the problem of conceptualizing adequately the outcome variables, the independent variables, and the intervening process variables we are working with here.

Now at this point I throw the meeting open to discussion. I'm sure that there are many people who have things they wish to say.

Dr. Dreikurs: There is one point which I would like to propose, and I think Mrs. Yarrow implied it when she spoke about the need for hypotheses to be tested. It seems to me that our present predicament, both in regard to mental health and the whole question of human dynamics, is tied in somehow with the status of our present-day social sciences. It can well be characterized as being in a pre-scientific state. We have a tremendous amount of research which is all trying to understand events from an analysis of the parts, notwithstanding the fact that we know today, in our wholistic concepts, that vital lesson that the whole is more than the sum total of the parts and that we can never understand the whole from the parts. In our research in the social sciences, in psychiatry, and psychology, we seem to have believed that through some in-part phenomenon we can hope to understand society and man. I don't think that we get very far with this procedure.

The physical sciences began to develop a state and shape only after some fundamental hypotheses had been developed and tested. Starting with the gravitation theory of Newton, the researchers explained all the multitudes of movements of the earth and the heavens by simple principles of mass and distance. And from then on, the development of the physical sciences was a development of basic laws. However, if you speak today about the basic law of social relationships, you are laughed at because social scientists tell you there is no basic law. Until they come to some of these fundamental laws in social relationships—which probably exist just as surely as do laws of the physical relationships—until they come to the point of proposing a set of hypotheses such as, for instance, one that I have made and others before me—that social equality is the basis for proper harmony and that social tension comes from the violation of this law—we will not make progress. That brings us up to the point that we may perhaps have to look for simplicity among the multitude of events—and that is the objection which I want to propose to this meeting, that we should think in terms of universal laws which may be tested and which may bring some order out of the chaos of our present-day research.

Dr. Smith: The issue of what kind of theory we're working with here is so fundamental I wonder whether, Mrs. Yarrow, you wish to comment on this before we have more questions? Another question first?

Dr. Blyth: I should like to take issue somewhat with Dr. Yarrow and Dr. Dreikurs and quote from a friend of mine, Dr. Vaughn Crandall of

Fels Research Institute, who delivered a paper at A.P.A. last year. His main thesis was—at least in the area of parent-child relationships—that we do not have enough inductive research material upon which or out of which to formulate some theoretical conceptions that will lead to hypotheses, at least ones that are meaningful. He was pleading for more research such as Dr. Barker and Dr. Sigel have been doing—just simply exploring the area of parent-child interaction in order to build up some data. The same probably could be said about the area of child-environment interaction, as has been brought up here. We need inductive research in order to get some more comprehensive and meaningful theories upon which to base hypothetical, deductive research later on.

Dr. Rankin: To follow up Dr. Dreikurs's comment and this last one as well, relating to this matter of kinds of research, I would have to go back to Newton and from Newton to Kepler and back from Kepler to Tycho Brahe. Tycho Brahe's observations of the positions of the planets, carried on over a period of forty years, was the basis of Kepler's discovery of the laws of planetary motion bearing his name. It was Kepler's generalizations about planetary motion on which Newton's law of gravitation is based. Now, to my mind, Tycho Brahe, Kepler, and Newton were all research workers of distinction. Whether there's a parallel here I don't know, but I think we have to recognize that all research doesn't have to be experimentation to prove a particular hypothesis. Some research is done in order to develop hypotheses.

Dr. Mullen: I've been sitting here all day trying to think what all this means—these very stimulating discussions—for the public schools. Dr. Ojemann's work is an illustration of one instance of effective cooperation between the public schools and the university and psychiatry. But most of us in the public schools are faced with a terrific job: we don't have the seats for them to sit in; we're trying to keep a roof on the place; we're confronted with a shortage of teachers. With the terrific administrative problems that we're faced with, research is just not in the picture.

In spite of the fact that people have tremendous faith that education can somehow solve all problems, we have never yet written into our school systems any method of evaluation to see whether we're getting the results that we should. When I received the invitation to attend the First Institute on Preventive Psychiatry, I assumed it meant that somebody thought that the public schools could make a contribution to preventive psychiatry. But where and how? We haven't done it so far. I feel that educational psychology has been somewhat sterile. The profession of school psychologists has certainly done little in the way of research. We're giving so many Binet tests and so forth that we might very well make a contribution but we're faced with such terrific difficulties. In Chicago

we're trying to cope with problems of special education, delinquency, over five thousand mentally handicapped children, and all the complications this involves—you've been reading about this in the papers.

Let's face the problem a little bit. Why haven't you gotten more cooperation? Why haven't we done more research, meaningful research, I mean? Why haven't we any real preventive psychiatry, as such, in the schools? The suggestion I want to throw out is that much of the research which has been done has been imposed *on* the school and hasn't been imbedded *in* the school. A lot of people have been telling teachers that this was wrong and that was wrong and the other was right, from the mental hygiene point of view. The teacher has said, "Well, by gum, I'd like to see *him* teach my class!" So often the material offered doesn't meet the needs of that teacher or give her some way of keeping that class together.

We have picked up some very good ideas today. I hope to get some of you folks to help us do a little better job on the problems facing us. But we've got a long way to go to get any real cooperation. I hope you folks can help school systems and that means the school administration, the public, and the board of education. The idea of spending a penny of the tax dollar on research is utterly nonexistent in my system and only exists in superficial fashion in any system I know anything about. But we need to get research into the school budget somehow. If you folks in psychology and psychiatry can come and help us out with problems of real estate, etc., maybe we can begin to do a little more in your area.

Dr. Smith: The problem of being useful to the schools—helping you when you're faced with the crowding problem—and at the same time being Tycho Brahes and Newtons and finding universal laws indicates, I think, the scope of the thing we're facing here. I was surprised that nobody has cited an example from biological research because it seems to me that we've fallen into the habit of looking to the physicist for our research models when we might better think of the great naturalists—the Darwins and possibly the experimental geneticists—as being a little closer to home. Have you comments you'd like to make on these last several statements, Mrs. Yarrow?

Dr. Yarrow: Just a few. I'd like to talk to the point that several of you are making about the detailed or the simplified research in the deductive or inductive approach. I think we might very easily get into a false kind of controversy here, trying to decide which is always the better, and certainly that was not my intention in stressing the need for more complicated researches in the particular areas that I was using as examples. I think research strategy has to include both kinds of research, and what we need to develop more, I suppose, is a sensitivity as to when it is more advan-

tageous to pull out some small variable to study within terms of a specific hypothesis and when it is more advantageous to deal with the very complex and real situation. So I would, in a sense, accept both points of view, or all points of view that have been expressed. I think maybe it would be wise to avoid trying to settle the question of whether it should be one of the other because I think that's not the real issue.

Dr. Smith: Another question?

Dr. Levitt: I'd like to line up behind Dr. Yarrow's earlier stand on complexity, as well as behind Dr. Dreikurs's view of the totality of the organism. And I know your problems, Dr. Smith, because I know a journal editor doesn't like complex things—they make it difficult to evaluate!

Let me begin by pointing out that most of the kinds of research designs or analyses that we use in the social sciences are based on a concept of random samples. Now there's probably not one study in a thousand which actually makes use of true random samples. This means that there's a possibility of all sorts of peculiar things happening with the readymade samples that we use in our studies. For this reason, any study that involves, let us say, the relationship between variable X and variable Y is in danger of being misleading. Let me put it this way. You may find a relationship between variable X and Y, this relationship being a function of variables A, B, C, D, P, and Q which were not measured in that study; and then you base some far-reaching conclusions on the results that you've found. Now the point that I'm making is simply that as many studies as possible within the limitations of time and funds should be done with measures of variables A, B, C, D, and so on. Suppose we're doing a study of the prevalence of mental illness (although I'm not sure this is too good to use as an illustration). Maybe besides age, sex, and social and economic standing, we should be taking in a large number of other variables—as many as possible. Presumably we should have in the ideal situation, a closed system in which we have identified all the variables that are involved in any causal relationship between the other two variables. I feel that this is a complex approach and, therefore, my stand is behind Dr. Yarrow.

Dr. Smith: We have time for only one or two more comments. Dr. McGuire?

Dr. McGuire: I see a strange relationship between some of the remarks that have been made here. First of all, let me say that I no longer bow down to the physicists, whether they be in the East or the West, since they have changed from the early days and now have to work in terms of Heisenberg's principles of probability. They, too, have to look for additional variables. They've almost given up the search for the universal relationship of one to one. The leading physicists today are looking right

now for particles which have at least two qualities; namely motion and energy—and they don't know how many more they will have to add to their explanations.

Frances Mullen came in with another plea—that the world in which school people live is one which has no universality. It's a world of probabilities, and we've got to get what Dr. Smith called indices, some indices that are pretty reliable and the smallest number we can get. Now I think that what Mrs. Yarrow was telling us is a very sensible thing in that if we can conceptualize it, we can get some ideas that have a history. We then need to translate them into some operations which have meaning to a number of people. We can do this specifically in terms of the clinical knowledge of some of you people or in terms of some indices which we can get from some sample populations.

We're not going to study the whole. We haven't come to the time when we have what some have called the "little systems," which would give us a reasonable understanding of parts of human behavior, but to my mind we can get as much understanding as the physicists have of the world in which men live. Our job is to understand men. What the school people and many of the practitioners are asking us to do is not to look for individual differences on one hand and universals on the other; rather we should try to explore this world in between and try to link some of the things we're seeing to what you in the applied fields are doing, and what you're observing to some of the things we're seeing. Then the work of the research person can make a good deal of sense. We don't have to explore everything. However, we can go as far as the physicist does today and he goes only so far as to get that which explains as much of the variation as he can and he does it in probability terms. We're probably going to have to go along with them, and we're finding we're much better equipped than most of the natural sciences in doing so.

Mr. Deutsch: May I add a word as a layman? We take it that prejudice generally, if not invariably, arises from ignorance. Yet as an ignorant layman, I have been very much puzzled and, I must confess, quite often amused by the prejudices that arise from knowledge. In the course of my occupation as a reporter and occasionally as a participant in some professional meetings, I have been very much impressed by the prejudices among professionals—as in the Psychopharmacology Conference in Washington some months ago when bitter prejudices were provoked between the experimental laboratory people on one hand and the clinicians on the other, each of whom felt that they, and they alone, had the proper approach to research in mental illness. As a late student of the history of this field, I wish there were among some professionals more historical sensitivity to the background of research in mental illness. They have

quite often, in my opinion, assumed a sort of phony humility and then gone ahead and become doctrinaire and totalitarian in their own approaches to the subject. I would remind them of the history of faddism in the research into the causes of mental illness. You all recall that during the period of crass materialism in the last quarter of the 19th century, there was an explosive pitch on brain pathology that there could not be a mental disease without a brain lesion. This was followed in the early period of the 20th century by the genetics fad. Every mental illness, every mental deficiency and poverty itself was an inherent trait. This was the period of Davenport and Goddard and the others who proposed the long lines, the royal lines, of the feeble-minded. Now we're on a biochemical pitch, and we have some leaders in the field of research who tell us that there cannot be a crooked mind without a crooked molecule! We've had one fad after another with a great many researchers jumping on a bandwagon or bestowing praise to a passing enthusiasm. In the light of most other fields, I must say that this is a field I feel very conservative in.

Dr. Smith: Thank you. Mrs. Yarrow, do you have anything else?

Dr. Yarrow: I'm not sure of the direction of your remarks but I only want to make sure that you're not accusing *me* of being prejudiced!

Mr. Deutsch: Oh, no!

Dr. Yarrow: The remarks I want to make are not particularly related to the discussions just preceding, but I'd like to play my other role for just one minute—if I may—that of representing Dr. Felix of the National Institute of Mental Health in being here. This is exactly the kind of gathering in which the National Institute has a great deal of interest and he hoped that sometime in the course of the conference I might express for him his regret in not being able to be here but certainly his great interest in the work that is going on at Iowa and, particularly, the kind of thing that has been going on in the day which we have just been through.

Dr. Smith: I was told by Dr. Ojemann that we were to bring this meeting to a close at 9:30. I should like to add to Mrs. Yarrow's remarks a feeling of great indebtedness and gratitude on the part of a participant in this institute. I'm sure that the rest of you feel with me a great indebtedness to Dr. Ojemann for having arranged this very stimulating and challenging occasion and also to the National Institute of Mental Health for having made it possible. Thank you very much on behalf of all of us. Now I'll turn the meeting back to Dean Loehwing.

Dean Loehwing: Ladies and gentlemen. It seems to me the institute has been a very strenuous and long day for all of you. It has finished on a very high note, and I think you're to be congratulated on this. I think, too, your discussion leader has seen it desirable to culminate it before you

got into any real acrimonious arguments here and, you know, I think I could pitch in a couple myself!

Seriously, the State University of Iowa is grateful to all of you for coming, leaving your various posts at very remote points from Iowa City. We're grateful that you have come. We feel that if you go away as well rewarded as we, your hosts, are, this will be the beginning of a long tradition of similar conferences and institutes. As a layman, I am gratified by your ability to talk across disciplinary lines. I sense from these discussions that there are many points of view, specialists in very many fields, psychologists and psychiatrists and others present here tonight. It is extremely gratifying that you are willing to come and cooperate in an institute of this sort and, I think, probably continue this cooperation in your various professional posts. We sincerely hope that you may come again and that we may be your hosts again. Thank you very much. The meeting is adjourned.

Roster of Participants

ADLERBLUM, EVELYN D., Ph.D.
New York University
Washington Square
New York City

ALLINSMITH, WESLEY, Ph.D.
Professor, Graduate School of Education
Assistant Director, Laboratory of Human Development
Harvard University
Cambridge, Massachusetts

ALMY, MILLIE, Ph.D.
Associate Professor
Teachers College
Columbia University
New York City

ANDERSON, CARL L., Ph.D.
Chief, Mental Health Services Section
Public Health Service, Region VIII
Denver, Colorado

ARNOLD, JEAN, M.D.
Assistant Professor, Psychiatry
Psychopathic Hospital
State University of Iowa

BABITZ, MILTON
Consultant in Adult Education
Bureau of Adult Education
Department of Education
State of California
Sacramento, California

BAER, PAUL E., Ph.D.
Assistant Professor, Pediatrics
State University of Iowa

BALDAUF, ROBERT
Psychologist
Cedar Rapids Public Schools
Cedar Rapids, Iowa

BARNES, JOYCE A.
State University of Iowa

BARNES, MILFORD E. JR., M.D.
Childrens Service Center of Wyoming Valley
Wilkes-Barre, Pennsylvania

BARNES, MILFORD E. SR., M.D.
Professor Emeritus, Hygiene and Preventive Medicine
Consulting Director Emeritus, State Bacteriology Laboratory
State University of Iowa

BARNHART, RICHARD, Ph.D.
Assistant Superintendent
Cedar Rapids Public Schools
Cedar Rapids, Iowa

BARTHOLOW, GEORGE W., M.D.
Resident
Psychopathic Hospital
State University of Iowa

BAYLEY, NANCY, Ph.D.
National Institute of Mental Health
Bethesda, Maryland

BECK, HELEN L.
Occupational Therapist
Psychopathic Hospital
State University of Iowa

BAKER, HUBERT H.
Executive Director
Linn County Mental Health Center
Cedar Rapids, Iowa

BELL, RICHARD Q., Ph.D.
Bethesda, Maryland

BERLIEN, IVAN C., M.D.
Chairman, Committee on Preventive Psychiatry
Group for the Advancement of Psychiatry
Detroit, Michigan

BLATZ, W. E., Ph.D.
Director, Institute of Child Study
University of Toronto
Toronto
Ontario, Canada

BLYTH, DAVID D., Ph.D.
Associate Director
Children's Mental Health Center
Columbus, Ohio

Bond, Helen V.
Social Service Worker
Black Hawk County Mental Health Center
Waterloo, Iowa

Brim, Orville G. Jr., Ph.D.
Project Director
Social Science and Parent Education
Russell Sage Foundation
New York City

Britton, Joseph, Ph.D.
Department of Child Development
Pennsylvania State University
University Park
Pennsylvania

Brooke, Clement E., M.D.
Assistant Professor of Pediatrics
University of Missouri
Columbia, Missouri

Bruce, Paul
Child Welfare Research Station
State University of Iowa

Burchinal, Lee, Ph.D.
Assistant Professor of Home Economics
Iowa State College
Ames, Iowa

Busse, Geraldine, R.N.
Nursing Consultant
Public Health Nursing
State Department of Health
Des Moines, Iowa

Caldwell, Bettye M., Ph.D.
Research Associate
Child Evaluation Clinic
School of Medicine
Washington University
St. Louis, Missouri

Campbell, Isabella J.
Principal, Garfield School
Cedar Rapids, Iowa

Carthey, Will O.
Principal, Taylor School
Cedar Rapids, Iowa

Chantiny, John, Ed.D.
Assistant Professor
Child Welfare Research Station
State University of Iowa

Crane, Helen W.
Social Studies Supervisor
Cedar Rapids Public Schools
Cedar Rapids, Iowa

Crawley, Fern W.
Cedar Rapids, Iowa

Davis, Harvey, Ph.D.
Provost
State University of Iowa

Davis, Hester A.
Research Assistant
Institute of Agricultural Medicine
State University of Iowa

Davison, Robert C.
Principal, Tyler School
Cedar Rapids, Iowa

Delmare, Maxine
Research Associate
Child Welfare Research Station
State University of Iowa

Deutsch, Albert
Writer
Washington, D.C.

Donnelly, Madeline, M.D.
Division of Maternal and Child Health
State of Iowa
Des Moines, Iowa

Dreibelbies, Lewis E.
Marion, Iowa

Dreikurs, Rudolph, M.D.
Professor of Psychiatry
Chicago Medical School
Chicago, Illinois

Dybwad, Gunnar, Ju.D.
Executive Director
Child Study Association of America
New York City

Ferguson, William
Chief, Mental Health Section
Department of Health
State of Minnesota
Minneapolis, Minnesota

Field, Mrs. H.P.
Mental Health Committee
Winneshiek County Mental Health Center
Decorah, Iowa

Friedman, Sheldon
Child Welfare Research Station
State University of Iowa

Fulcomer, David M., Ph.D.
Professor of Family Sociology
Iowa State College
Ames, Iowa

Gewirtz, D. Hava
Washington, D.C.

Gewirtz, Jacob, Ph.D.
Resident Psychologist
National Institute of Mental Health
Bethesda, Maryland

Goethals, George W., Ed.D.
Lecturer, Graduate School of Education
Research Associate, Laboratory of Human Development
Harvard University
Cambridge, Massachusetts

Gokbora, Maryjane
State University of Iowa

Gottlieb, J. S., M.D.
Director
Lafayette Clinic
Detroit, Michigan

Grams, Armin, Ph.D.
Wisconsin State College
La Crosse, Wisconsin

Hammerman, William M.
Oregon, Illinois

Hardwick, Ruth A.
Psychologist
Annie Wittenmeyer Home
Davenport, Iowa

Harris, Dale B., Ph.D.
Director, Institute of Child Welfare
University of Minnesota
Minneapolis, Minnesota

Hawkes, Glenn R., Ph.D.
Head, Department of Child Development
Iowa State College
Ames, Iowa

Helfant, Kenneth, Ph.D.
School Psychologist
Manhasset Public Schools
Manhasset, New York

Helmholz, Henry F., M.D.
Emeritus Staff
Mayo Clinic
Rochester, Minnesota

Hollingshead, August B., Ph.D.
Professor of Sociology
Yale University
New Haven, Connecticut

Hoobler, Icie M., Ph.D.
Consultant in Chemistry
Merrill-Palmer School
Detroit, Michigan

Hoppin, Margery
Research Associate
Child Welfare Research Station
State University of Iowa

Horowitz, Frances
Child Welfare Research Station
State University of Iowa

Hoyt, Kenneth, Ph.D.
Assistant Professor
College of Education
State University of Iowa

Huston, Paul, M.D., Ph.D.
Head, Department of Psychiatry
Director, Psychopathic Hospital
State University of Iowa

Immer, Esther L.
Secretary, Iowa Commission of Children and Youth
Des Moines, Iowa

Iscoe, Ira, Ph.D.
Assistant Professor of Psychology
University of Texas
Austin, Texas

Jacobsen, Sylvella
Educational Psychologist and Director of Special Education
Iowa City Public Schools
Iowa City, Iowa

Jacobson, Bernard
Education Clinic
Iowa State Teachers College
Cedar Falls, Iowa

Jauch, Tonia
Child Welfare Research Station
State University of Iowa

JAQUES, MARCELINE
State University of Iowa

JENKINS, BETTY
State University of Iowa

JERSILD, ARTHUR T., Ph.D.
Professor of Education
Teachers College
Columbia University
New York City

JORGENSON, NANCY
Child Welfare Research Station
State University of Iowa

KENKEL, WILLIAM, Ph.D.
Assistant Professor of Sociology
Iowa State College
Ames, Iowa

KRUSH, THADDEUS P., M.D.
Clinical Director
Community Services,
Nebraska Psychiatric Institute
University of Nebraska
Omaha, Nebraska

LAMKE, TOM A., Ph.D.
Coordinator of Research
Iowa State Teachers College
Cedar Falls, Iowa

LEE, J. MURRAY, Ph.D.
Professor of Elementary Education
University of Miami
Coral Gables, Florida

LEVITT, EUGENE E., Ph.D.
Director of Evaluation Research
Institute for Juvenile Research
Chicago, Illinois

LEWIS, JAMES
Special Counselor
Cedar Rapids Public Schools
Cedar Rapids, Iowa

LINZER, EDWARD
Director of Education Services
National Association for Mental Health
Inc.
New York City

LOEHWING, WALTER F., Ph.D.
Dean of the Graduate College
State University of Iowa

LOVELL, LLOYD, Ph.D.
Assistant Professor
Child Welfare Research Station
State University of Iowa

LOWENBERG, RICHARD
Des Moines, Iowa

LUCKEY, BERTHA M., Ph.D.
Chief Psychologist
Psychological Service
Board of Education
Cleveland, Ohio

LYLE, WILLIAM H. JR., Ph.D.
Assistant Professor of Psychology
Southern Illinois University
Carbondale, Illinois

MACY, WILLIAM W., M.D.
Associate in Psychiatry
State University of Iowa

MANVILLE, HELEN
Observer in Preventive Psychiatry Research Program
Cedar Rapids, Iowa

MANVILLE, JOHN
Principal, Hayes School
Cedar Rapids, Iowa

MARTIN, WILLIAM E., Ph.D.
Professor of Family Life
Purdue University
Lafayette, Indiana

MAST, GIFFORD M.
President
Scott County Association for Mental Health
Davenport, Iowa

MATTOX, ANN
Iowa City

MCCORKLE, THOMAS, Ph.D.
Assistant Professor
Institute of Agricultural Medicine
State University of Iowa

MCGINNIS, ESTHER, Ph.D.
Professor of Home Economics
School of Home Economics
Ohio State University
Columbus, Ohio

McGuire, Carson, Ph.D.
Professor, College of Education
University of Texas
Austin, Texas

McHale, Julian
Urbana, Illinois

McLean, Helen V., M.D.
Chicago, Illinois

Meredith, Howard V., Ph.D.
Professor of Physical Growth
Child Welfare Research Station
State University of Iowa

Miller, Mrs. M. J.
Mental Health Committee
Winneshiek County Mental Health Center
Ossian, Iowa

Moerschel, W. J., M.D.
Cedar Rapids, Iowa

Morgan, H. Gerthon, Ph.D.
Institute for Child Study
University of Maryland
College Park, Maryland

Morgan, Mildred, Ph.D.
Professor, Home and Family Life
Florida State University
Tallahassee, Florida

Mulford, Harold A., Ph.D.
Research Associate
Department of Sociology and Anthropology
State University of Iowa

Mullen, Frances A., Ph.D.
Assistant Superintendent of Schools
Department of Special Education
Board of Education
City of Chicago, Illinois

Murray, Leonard C., Ph.D.
Director, Division of Health Education
State Department of Health
Des Moines, Iowa

Ojemann, Ralph H., Ph.D.
Professor of Educational Psychology and Parent and Family Life Education
Child Welfare Research Station
State University of Iowa

Parris, Waverly V.
Cedar Rapids, Iowa

Pepernik, Max C., M.D.
Assistant Professor of Psychiatry
State University of Iowa

Petersen, Dorothy
Postville, Iowa

Peterson, Elmer T., Ph.D.
Dean of the College of Education
State University of Iowa

Phillips, Ruth
Principal, Grant Wood School
Cedar Rapids, Iowa

Pogge, Lucille G.
Principal, Van Buren School
Cedar Rapids, Iowa

Pothast, M.D., Ph.D.
Clinical Psychologist
Cerro Gordo Mental Health Center
Mason City, Iowa

Powell, Robert M., M.D.
Mason City, Iowa

Prescott, Daniel, Ed.D.
Director, Institute for Child Study
College of Education
University of Maryland
College Park, Maryland

Rankin, Paul T., Ph.D.
Assistant Superintendent
Detroit Public Schools
Detroit, Michigan

Reid, Hale
Assistant Superintendent
Cedar Rapids Public Schools
Cedar Rapids, Iowa

Reid, Margaret
Observer in Preventive Psychiatry Research Program
Cedar Rapids, Iowa

Rheingold, Harriet, Ph.D.
National Institute of Mental Health
Bethesda, Maryland

Robinson, Eleanor L.
Institute of Child Welfare
University of Minnesota
Minneapolis, Minnesota

Robinson, Paula
Executive Director
Iowa Association for Mental Health
Des Moines, Iowa

ROLL, MARLIN HENRY
Educational Psychologist
College of Education
State University of Iowa

RUCH, CLAUD C.
Field Enterprises
Evanston, Illinois

SAREYAN, ALEX
Executive Director
Mental Health Materials Center, Inc.
New York City

SEDLACEK, GORDON M.
Linn County Mental Health Center
Cedar Rapids, Iowa

SEDLACEK, LEO B., M.D.
Cedar Rapids, Iowa

SENN, MILTON J. E., M.D.
Director
Child Study Center
Yale University
New Haven, Connecticut

SEVERINGHAUSE, ELEANOR P.
Iowa City

SINGLETON, CARLTON M., Ed.D.
Assistant Professor
College of Education
State University of Iowa

SKODAK, MARIE, Ph.D.
Consulting Psychologist
Flint, Michigan

SLOBETZ, FRANK, Ed.D.
Chairman, Division of Education, Philosophy and Psychology
State Teachers College
St. Cloud, Minnesota

SMITH, M. BREWSTER, Ph.D.
Professor of Psychology
New York University
Washington Square
New York City

SMITHEY, ANNIE
Principal, Arthur School
Cedar Rapids, Iowa

SONTAG, L.W., M.D.
Director
Fels Research Institute
Antioch College
Yellow Springs, Ohio

SPIEGEL, JOHN P., M.D.
Professor, Department of Social Relations
Harvard University
Cambridge, Massachusetts

STEARNS, GENEVIEVE, Ph.D.
Research Professor
Department of Orthopedic Surgery
State University of Iowa

STOTT, LELAND H., Ph.D.
Leader of Longitudinal Studies
Merrill-Palmer School
Detroit, Michigan

SNIDER, BILL, Ph.D.
Research Assistant Professor
Child Welfare Research Station
State University of Iowa

SWIM, WILLIAM E.
Principal, Polk School
Cedar Rapids, Iowa

TASCH, RUTH J., Ph.D.
Child Guidance Clinic
Wichita, Kansas

TOP, FRANKLIN H., M.D.
Head, Hygiene and Preventive Medicine
Consulting Director, State Bacteriology Laboratory
State University of Iowa

TRUMPE, WILLIAM D., M.D.
Cedar Rapids, Iowa

TURNER, WILLIAM J., Ph.D.
Director
Guidance Center
Decorah, Iowa

UPDEGRAFF, RUTH, Ph.D.
Professor of Group Guidance of Children
Child Welfare Research Station
State University of Iowa

VAUGHAN, WARREN T. JR., M.D.
Director of Division of Mental Hygiene
Department of Mental Health
Commonwealth of Massachusetts
Boston, Massachusetts

WELLS, ALBERTA
Research Associate
Child Welfare Research Station
State University of Iowa

WELLS, JAMES P.
Editorial Associate
News and Information Service
State University of Iowa

WHITE, DOROTHY
School Psychologist
Ceres Elementary Schools
Ceres, California

WILLIAMSON, MAX
Secretary
Minnesota Association for Mental Health
Minneapolis, Minnesota

WRIGHT, WAYNE K.
Psychologist, Linn County Mental Health Center
Cedar Rapids, Iowa

WURTZ, CONRAD R., Ph.D.
School Psychologist
Cedar Rapids Public Schools
Cedar Rapids, Iowa

YARROW, MARIAN RADKE, Ph.D.
Chief, Section on Social Developmental and Family Studies,
National Institute of Mental Health
Bethesda, Maryland

YOUTZ, MAY P.
Associate Professor Emeritus
Child Welfare Research Station
State University of Iowa

ZIMMERER, EDMUND G., M.D.
Commissioner of Public Health
State Department of Health
Des Moines, Iowa

ZINTZ, MILES V., Ph.D.
Director of Educational Clinic
Iowa State Teachers College
Cedar Falls, Iowa

The following experimental teachers in the Preventive Psychiatry Research Program were present at the Institute:

Cedar Rapids Public Schools
Cedar Rapids, Iowa

LUCILE DUNN
Kenwood

ROBERT EPPEL
Johnson

MARGARET HULT
Kenwood

MARIAN KENNEDY
Kenwood

GEORGIANNA KNURR
Grant Wood

MARY JEANNE PALMER
Kenwood

ANN PAVLOVSKY
Arthur

LOIS JEANNE SACKETT
Grant Wood

MARY ELLEN SIMMONS
Arthur

JOAN SCHREIBER
Van Buren

Tipton Community School
Tipton, Iowa

KATHERINE GRUENWALD
MARTHA JANE HENRY
ANNA MAE OHL
MARLENE SELL

Iowa City Public Schools
Iowa City, Iowa

JACQUELINE BECKER
Roosevelt

NELLIE REGER
Roosevelt