

Psychology: A Study of a Science

Formulations of the Person and the Social Context

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Psychology: A Study of a Science

STUDY I. CONCEPTUAL AND SYSTEMATIC

Volume 3. Formulations of the Person
and the Social Context

PSYCHOLOGY: A STUDY OF A SCIENCE

The Series

STUDY I. CONCEPTUAL AND SYSTEMATIC

Volume 1. Sensory, Perceptual, and Physiological Formulations

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Volume 3. Formulations of the Person and the Social Context

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STUDY II. EMPIRICAL SUBSTRUCTURE AND RELATIONS WITH OTHER SCIENCES

(These titles in preparation)

Volume 4. Biologically Oriented Fields: Their Place in Psychology and in Biological Science

Volume 5. The Process Areas, the Person, and Some Applied Fields: Their Place in Psychology and in Science

Volume 6. Investigations of Man as Socius: Their Place in Psychology and the Social Sciences

POSTSCRIPT TO THE STUDY

Volume 7. Psychology and the Human Agent: A View of Problems in the Enaction of a Science (*by Sigmund Koch*)

Psychology: A Study of a Science

STUDY I. CONCEPTUAL AND SYSTEMATIC

Volume 3. Formulations of the Person
and the Social Context

Edited by Sigmund Koch

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PREFACE

When one looks back over the history of science, the successes are likely to be stressed and the failures forgotten. Thus one tends to see science as starting with a sure sense of direction and progressing neatly to its present form. Or so it is for the older and well established branches of science; but not for psychology. Psychology has not one sure sense of direction but several quite unsure directions. Growth is erratic and there is much casting about for the most crucial problems and the most powerful methods. These apparent differences between psychology and the older branches of science may result from the difficulty of developing a science of man; it is perhaps significant that many of the problems of psychology were not attacked by the methods of science until so late a date in history. Or the differences may be an illusion resulting from the much closer view we have of the beginning struggles to develop a science of psychology than we now have of the beginning efforts in the older sciences.

Certainly psychology has its problems, and they are not easy. Nevertheless, knowledge has grown rapidly in the short history of man's efforts to develop a science of behavior, and the time seems appropriate for a major effort to examine the progress that has been made in attempting to find a way, or ways, to the attainment of the explanatory power that we like to think of as characteristic of science. A growing body of empirical information, a serious concern over methodological issues, and a variety of efforts to bring a selected body of fact into the organizing framework of theory all emphasize the need for that line of questioning—always going on in science—which explores the shape of knowledge, the range and inner connections of the ideas through which it has been developed and organized, the changing substructures of empirical data, and their emerging relations to each other and to the findings of other sciences. The seven volumes of *Psychology: A Study of a Science* are a response to this need.

The first three volumes, which bear the collective title *Study I. Conceptual and Systematic*, are concerned with many of the systematic formulations of recent and current influence which psychologists have developed to account for the phenomena in which they are interested.

Each systematic position is analyzed by its originator, or a person connected with its development, in a way which gives attention to the problems it seeks to solve, the empirical basis on which it rests, its degree of success, and its relations to other formulations.

A second set of three volumes, collectively called *Study II. Empirical Substructure and Relations with Other Sciences*, inquires, again through the efforts of creatively active investigators, into the organization of various fields of empirical knowledge, the relations of one to another and to work going forward in other sciences. It also examines such problems in reverse through the participation of social and biological scientists who consider the relations of their own special fields to various parts of psychology. The three volumes of Study II, now in preparation, will be published at a later date.

Volume 7—*Psychology and the Human Agent*—will present the Study Director's view of certain problems of psychological inquiry in the light of the findings of the project.

Primary credit for the initiation of these studies goes to the Association's Policy and Planning Board, which decided in 1952 that the time had come for a thorough and critical examination of the status and development of psychology. The National Science Foundation agreed upon the desirability of such an undertaking and has generously supported the effort. When funds from the National Science Foundation were found to be insufficient for all of the expenses of the studies, the American Psychological Association provided the supplementary funds necessary to complete the work.

From the beginning, the study was divided into two parts. One part dealt with the education of psychologists and the factors conducive to research productivity in psychology. That part was directed by Professor Kenneth Clark of the University of Minnesota, who has reported the findings in *America's Psychologists: A Survey of a Growing Profession*, published by the American Psychological Association in 1957.

The other part, the part with which the present series of volumes is concerned, has dealt with the substance of psychological thought and data. Professor Sigmund Koch of Duke University has been responsible for this part of the study. Working closely with him has been a panel of consultants consisting of Lyle H. Lanier, Howard H. Kendler, Conrad G. Mueller, and Karl E. Zener. These men, but chiefly Dr. Koch, have planned, organized, interpreted and edited the work, and successfully enlisted the cooperation of the approximately 80 authors whose original papers will constitute the basic material of the series.

In the background, at a safe distance from the labors that have sometimes engulfed Dr. Koch, his panel of consultants, and the primary authors, has been a steering committee on which I had the pleas-

ure of serving as chairman, and having as colleagues Clarence H. Graham, Lyle H. Lanier, Robert B. MacLeod, Eliot H. Rodnick, M. Brewster Smith, and Robert L. Thorndike. The steering committee helped to make administrative arrangements and helped to decide on the scope of the studies, but takes no credit for their successful completion.

In the preface to *America's Psychologists* we have already acknowledged our gratitude to Kenneth Clark and his collaborators who helped to produce that volume. It is our final pleasant duty to express our thanks to Duke University for making Dr. Koch's time available; to the National Science Foundation for its necessary and generous financial support and for the counsel and support of John T. Wilson, Assistant Director for the Biological Sciences; to Lyle H. Lanier, Howard H. Kendler, Conrad G. Mueller, and Karl E. Zener for their critical and devoted help; to all of the authors whose names appear on the title pages for their original contributions; and—most of all—to Sigmund Koch for directing and driving through to completion what we hope will be an oft-consulted aid to the scholars and research workers who are striving to increase the rigor and further the development of scientific psychology.

Dael Wolfe, CHAIRMAN
STEERING COMMITTEE
POLICY AND PLANNING BOARD

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INTRODUCTION TO VOLUME 3

Psychology: A Study of a Science is a report of inquiries into the status and tendency of psychological science. Some eighty distinguished authors have contributed sustained essays which consider: (Study I) major theoretical formulations of recent importance; and (Study II) the structure, mutual interrelations, and associations with other sciences of the main empirical areas in which psychological research is pursued. The findings of *Study I Conceptual and Systematic* comprise the initial three volumes of the series; *Study II Empirical Substructure and Relations with Other Sciences* is reported in the following three volumes. A final volume by the Study Director—*Psychology and the Human Agent*—includes commentary on the significance of the findings.

The present volume is the third in the series and is part of Study I. Each of the eleven essays in this book is a self-contained presentation which may be read with profit independently of the others, or of the contents of other volumes. Yet the reader must bear in mind that the present volume is the receptacle of a fragment of Study I, and that Study I, in turn, is part of a larger enterprise having certain unifying values, aims, and methods. For a conception of these latter, the reader is referred to Dael Wolfe's Preface and to the two detailed introductory sections in Volume 1—one for the series, the other specifically for Study I.

As an immediate aid to the reader, a résumé of the Study's design is given.

Study I Conceptual and Systematic. This study involved the intensive analysis of thirty-four "systematic formulations" of widely varying type and subject-matter reference and all of established influence in recent psychology. A "systematic formulation" was defined quite generally as "any set of sentences formulated as a tool for ordering knowledge with respect to some specified domain of events, or furthering the discovery of such knowledge": in applying this definition, care was taken that no formulation be precluded by nonconformity to standardized conceptions of the nature of "theory." Since each systematic formulation is the end-product of a human effort to see and state order in a given domain, each analysis was made either by the originator(s)

of the formulation in question or (in a few cases) by individuals creatively associated with the *development* of formulations of which they were not the primary authors.

Each systematist was invited to approach his work with certain common *themes of analysis* in mind. These were designed to invite a convergence of insight on those problems of systematization which had emerged from the practice of the past three decades, more or less. Some of the suggested problems had been conspicuous in previous "meta-systematic" discussion, but required in our opinion exposure to a wider range of systematically schooled sensibilities. Others were problems that seemed critically posed by recent systematic work, yet ones which had received little or no explicit attention.

The dominating hope was for analyses that might illumine the relations between the creative *processes* of systematizing and their publicly expressed *products*. It was thus hoped that the atmosphere of the study might encourage as much concern with background influences, orienting presuppositions, and working methods, as with conceptual content, research achievements, and prospects. It was felt that analysis of this order could itself have creative consequences; reflective scrutiny of the extent and depth envisaged means *rethinking*. The primary intent of the discussion themes (and indeed the constant aim of all editorial effort) was to realize an atmosphere that might invite such emphases. Authors were requested to make explicit reference to the themes in their writing only to an extent they deemed appropriate or congenial. The use of the themes for facilitating the collation of findings was thus a secondary, if still important, aim. As matters turned out, most authors adhered to them sufficiently to give the reader an excellent purchase for the detection of similarities and differences on key issues.

The grounds for the selection of the thirty-four formulations included in Study I are given in Volume 1 (pp. 21-27). The aim was a reasonably balanced diversification of formulations (as judged by many consultants) with respect to (a) subject-matter reference, and (b) conceptual and methodological "type." Many significant formulations that we would have wished to represent in the original list were excluded by spatial and other arbitrary restrictions. Nor are all formulations originally chosen included in the present volumes. Though the proportion of inclusions is remarkable, there were some individuals who could not participate. We do not, then, claim "representativeness" even in an informal and impressionistic sense. We do, however, claim sufficient diversity to extend markedly the range of formulations which in recent years have been given sustained analytic attention.

Study II Empirical Substructure and Relations with Other Sciences. This study seeks increased understanding of the internal

structure of psychological science and its place in the matrix of scientific activity. Over forty contributors, having distinguished research backgrounds in psychology, or in related biological and social sciences, were invited to write papers which examine the organization of empirical knowledge within subareas of these disciplines, and which chart their cross connections. Psychologist contributors consider the relations between their own fields of special competence and the rest of psychology, and inquire also into relations with relevant segments of other sciences. Social and biological science contributors examine the relations between their own fields and psychology.

All authors are individuals whose research interests have bridged conventionally discriminated fields of knowledge. Each was asked to place special emphasis on those "bridging problems" which had been central in his own research experience. As in the case of Study I, an attempt was made to encourage differentiated and stratified analysis, and to invite a convergence of visions on significant cross-cutting issues, by proposing certain common themes of analysis. The "themes" for Study II comprise a detailed breakdown of the senses in which questions of "mapping" subject-matter structure and exploring field interrelations might be entertained.

Though the topography of a science is too vast and labile for comprehensive or final mapping, this very fact makes it more important to assay the contours of knowledge as best we can. Study II exploits the only resource available for such problems—individual vision—but in a novel way. It assumes that a *pooling* of expert, specialized sensibilities can give insight into the emerging structure of a science of a sort not ordinarily available.

A fuller statement of the plan for Study II appears in the General Introduction to the Series (Vol. 1, pp. 1-18). An adequate account of working methods must await publication of the completed study.

Psychology and the Human Agent. This volume is a postscript to the Study, representing certain views formed by the Director in its course. The book (*a*) records those attitudes towards a science and science which necessarily color the spirit of the Study, (*b*) constructs trends from the massive findings of the two group studies, and (*c*) considers, in the light of the Study's premises and apparent trends, certain problems of psychological inquiry suggested by the practice of the past several decades.

In this day in which "self studies" and reductions of enigma by seminar are becoming commonplace in social science, it may be helpful to mention a few of the special features of *Psychology: A Study of a Science*. May we stress (in random order) the following points:

1. Both group studies are "collaborative" but only in the special sense that many creative men agreed to pursue, within the climate of the Study, *individual* tasks of vital interest to themselves. The study is not collaborative or "groupish" in any sense implying an intention to relinquish individuality or even idiosyncrasy to some prissy conception of the common weal.

2. The Study aims for *no* grand "integration" of knowledge. If a "Summa Psychologica" or even a "Synopticon" had seemed even remotely within reach, our inclination would have been to abstain on principle. The Study seeks to reflect the diversity of thought that actually exists and is premised on the value of widespread diversification of approach as a condition to the health of a science. It conceives of its contributor groups as *pluralities* of creative individuals who view those areas which they know best through the screen of their own expert sensibilities. The discussion outlines invite the play of individual sensibilities on commensurable themes, thus helping the reader to collate positions.

3. The Study's aims are neither legislative nor evaluative. It rejects all monolithic codes for the generation and processing of knowledge, or for virtuous scientific conduct. If there is a central bias, it is for the loosening of those constraints which can keep men from significant problems or thoughts through fear of the unorthodox.

The Study by no means devalues the insights of recent "science of science" but would wish them set in a perspective better adjusted to a field barely beginning to test established methods of science on an illimitably diverse and intricate collection of subject matters. Such a perspective can emerge by seeing the end-products of science as everywhere conditioned by human decision, value, creative option; by freeing from staleness that truism which holds scientific inquiry to be continuous with other human activities. Analysis in science, then, becomes more than a succession of routine tasks in the "logic" of science; it becomes an enterprise which can uncover the significance of its objects only by holding in view the relations between creative process and sentential product, strategic gamble and cognitive outcome.

The grounds for the inclusion of the eleven formulations contained in this volume are best conveyed by reference to the planning for the total Study (cf. Introduction to Study I, Vol. 1, especially pp. 21-27). Eleven topics is a stingy allotment relative to the range and density of effort in studies of the person and the social setting. If many significant lines of work have been omitted, the ones included are also significant—and sufficiently varied to suggest a generous range of the problems and tasks that systematists face.

Of the contributions relevant to "personality," three—Murray's

“scaffold,” Rapaport’s systematic examination of psychoanalysis, and Rogers’s codification of his client-centered framework—are representative of conceptual frameworks having broad scientific objectives and long-standing influence. Cattell draws together certain proposals towards systematic thought suggested by the logic and findings of one of the principal *methods* of personality research (factor analysis). By reviewing a delimited but basic problem area (psychogenetic studies of twins), Kallmann gives an exhibit of the type of painstaking, stepwise empirical work on which the resolution of issues common to many systematists must depend.

Turning to the contributions that most would allocate to “social psychology,” again we find represented three lines of work which point towards general systematic objectives. Each of these, moreover, stresses a different one of three principal “levels” at which social “variables” may be conceived. Asch could be said to represent the type of social psychology which sees no basis for conceptual separation from individual psychology. Thelen and Parsons consider instances of the approaches of “group dynamics” and “unified social science theory,” respectively. An important *methodic* formulation (latent structure analysis) is offered by Lazarsfeld in response to a class of problems which he sees as ubiquitous to psychology and social science. Finally, the contributions of Newcomb and of Katz and Stotland present preliminary systematic assessments of problems stemming from one of the more active fields of social psychological research—the study of attitudes.

On one point, agreement among authors is so vehement that it may be not unfair to anticipate it here. Personologists and psychologists called “social” refuse in this volume to compress their concerns into the standard compartments. If they have always seen their concerns as fundamental to the task of psychology, they now seem eager to assert this even within the hearing of “fundamental” psychologists. To such refreshing truculence we could but yield by avoiding the time-worn substantives “personality” and “social psychology” in the title of this volume.

Mention of certain editorial provisions is in order. Readers will find the complete statement of discussion themes, as sent to contributors, reproduced in the Appendix. There is, of course, variation in the extent to which the different presentations adhere to the discussion themes. As an aid to readers interested in the detection of key convergences and divergences of positions, index numbers corresponding to the principal thematic items have been inserted, where relevant, in the individual tables of contents appearing before each paper. The system of index numbers is explained in the Appendix.

This final volume of Study I contains a section of general comment on the study. An editorial epilogue presents a few trends suggested by

the essays in all three volumes. Attention is restricted to conspicuous trends which can give a "fix" on the position of systematic psychology relative to its recent history. There is also a special supplement by Conrad Mueller on certain methodological implications of the contributions in sensory psychology. Dr. Mueller generously served in a capacity much like that of sub-editor in the sensory area.

Further discussion of trends will be offered in the final volume of the series, *Psychology and the Human Agent*. There is, however, no standard "theory" of the meaning of this study. If there were, we would consider the main aim compromised. That aim has been to develop materials of unique comprehensiveness and depth in terms of which each reader may enrich his *own* view of systematic psychology. The thirty-four essays of Study I can reward efforts towards secondary analysis and synthesis—whether by student, specialized scholar, or general reader—for a long time to come. Let there be as many theories of this study as there are readers.

Psychology: A Study of a Science is the result of a project sponsored by the American Psychological Association and subsidized by the National Science Foundation. The project was known as "Project A" of the "APA Study of the Status and Development of Psychology." The work profited from the counsel of an Advisory Committee consisting of Dael Wolfe, Chairman, and Clarence H. Graham, Lyle H. Lanier, Robert B. MacLeod, Eliot H. Rodnick, M. Brewster Smith, and Robert L. Thorndike. Howard H. Kendler, Lyle H. Lanier, Conrad G. Mueller, and Karl E. Zener composed a Panel of Consultants to the Director. The generous part played by the members of both groups is described in the introductory sections of Volume 1.

PREPARATIONS FOR THE SCAFFOLD OF A COMPREHENSIVE SYSTEM

HENRY A. MURRAY
Harvard University

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INTRODUCTION

It seems that the majority of my voices are in favor of this enterprise, for here I am, pen in hand, intending to comply so far as possible with the editorial suggestions.

But a minority of me—and now surely, at the outset, is the moment to give vent to it—believes that certain of the analyses invited by the discussion outline are premature, not for all psychologists perhaps, but for those who are concerned with human lives and personalities. The topics suggested for discursive treatment are broadly defined; but, even when taken with a grain of salt, the task calls for meticulous criticism of one's own speech, semantic niceties, overelegant definitions. Should not criticism and refinement be in balance with spontaneity, exploration, and invention if a science is to grow in a way and at a pace appropriate to its age? Also, do we have sufficient data or sufficient organization of the data to arrive at anything more than a miniature system for a tiny region of transactions? Systematic psychology, being very young, has occupied only a small portion of its legitimate terrain. Its contemporary schools are like our thirteen colonies along the Atlantic coast line, a narrow strip of provincial culture. Their manifest destiny is to move West, order the wilderness with the best available tools, crude as they now are, and eventually achieve a more refined and comprehensive system which embraces all parts and functions of the whole, the total personality. At this stage I should hate to see our center of gravity move any further to the side of perfectionistic rituals, a hair-splitting fussy Conscience.

No doubt this large endeavor will bear fruit; but despite its promise, it is not applauded at this moment by some members of my household because of their suspicion that it is liable to seduce some promising psychologists away from the study of personalities—the domain that is theirs, and only theirs, to explore, survey, and map—away from the humanistically important riddles which we should be creeping up on gradually and craftily.

Another reason for my hesitation in joining this enterprise is the impossibility of my adhering to the suggested ordinance of discourse. It is evident that certain of its terms could be met only by psychologists with other aims than mine. It is an admirable mold—straightedged and nicely shaped—for exclusively experimental specialists, observers of closely restricted animal activities, peripheralists, and positivists; but literal adherence at all points is scarcely possible for naturalists, generalists, and centralists, who study gradual transformations of the dispositions, beliefs, and modes of action of human beings as they manifest themselves in different social settings.

Despite the above reservations, twenty months ago it was decided somehow that I accept the challenge as an adventure in self-discipline; and, in conformity with the committee's outline, I went ahead with what amounts to an intellectual autobiography in so far as this relates to the development of my present scaffold for a theory of personality.

This part of the assignment was easier than I anticipated; but the second part—setting up a logically articulated skeleton of the whole—was so much more difficult that, despite an extension of time as well as every possible guidance and encouragement from a most charitable Director, I was unable to arrive at a satisfactory set of basic propositions before the date line. In short, I proved unequal to the set standard. It happened, however, that more than half of the matter to be ordered in Part 2 has been included in Part 1, and so, the Director, pressed by generosity, decided that the peculiar fragment which lies before you might serve as a kind of substitute contribution. Its title might be this: certain orienting dispositions, impressive observations, and influential theories as determinants of scientific aims, assumptions, methods, and conceptions.

INTEREST IN SIGNIFICANT HUMAN FEELINGS, THOUGHTS, AND ACTIONS. INFLUENCE OF MEDICINE

It is generally assumed by the uninformed and innocent that all psychologists must have at least one "orienting attitude" in common: a stout affection for human beings coupled with a consuming interest in their emotions and evaluations, their imaginations and beliefs, their purposes and plans, their endeavors, failures, and achievements. But this assumption, it appears, is not correct. A psychologist who has been constantly prodded and goaded by these propulsions, as I have been, belongs to a once small and feeble, though now expanding and more capable, minority. Anyhow, this bent of empathy and curiosity toward all profound experiences of individual men and women should be set down as one of the prime determinants of several definitive decisions, which shall be mentioned, respecting the scope of my scientific concern and of a methodology to fit. This is a crucial point because, if my interest in events of this sort had been less steadfast, I might have turned to more manageable phenomena.

My interest in people, their doings and their ills, must have had something to do with my choice at college of history as field of concentration and of medicine as career for later life.

The study of history implanted the idea of the time dimension as an essential part of the very definition of reality as well as a miscellany of coarse facts to support my speculations when I dipped, three decades later, in the stream of sociology and anthropology. But the study of medicine was more influential: it led to two years of surgery and five years of research in physiology and in the chemistry of embryology, with a Ph.D. from Cambridge University in physiological chemistry.

The practice of medicine taught me a lot of commonsensical things, one of which was that among the few almost indispensable methods of

arriving at valid diagnoses (apperceptions, inferences) is that of inquiry—the thorough detailed recording of the patient's memories of interior sensations and pertinent emotional experiences. We were taught to distinguish perceptible *physical signs* (overt sense data) and imperceptible *symptoms* (reports of covert psychic processes) and to value both. The proof obtained on the operating table, time and time again, that a correct diagnosis of an abdominal condition could be made solely on the basis of a patient's reported symptoms was so firmly imprinted on the entablatures of my cortex that when, in later years, I was confronted by Watson's dogma—his radical repudiation of subjective experiences as material for psychology—my head assigned it to the category of eccentric foibles. I was an empirical behaviorist, born, bred, and trained, in the sense that every physicist, chemist, and biologist is necessarily a behaviorist. But when it came to dealing with human beings, I could see no advantage in allowing myself to be converted into an exclusive, half-paralyzed behaviorist who, on metaphysical grounds, elects to deny himself an invaluable source of data. (This does not apply to the current cultural situation: today, after a complete semantic somersault, every psychological process—perception, emotion, dreaming—is called "behavior.")

My above-mentioned interest in people was not at all confined to their physical activities—say, to the routes they chose and the muscles they used in locomoting to the restaurants they preferred to ingest the food that was most appealing to their senses. I was much more interested in their feelings, evaluations, and conceptions relative to other matters, and for the most part, so were they—and so were my militantly behavioristic friends of later years—more interested in the valued products of their intellections than in their own muscular accomplishments. In due course, assured that correctness of prediction is the best index of the relative worth of different methods, I did a few impromptu experiments and found empirically that the most dependable single operation I could perform in attempting to foretell what a behaviorist would do next or in the near future was to ask him. But the commonsensical avowal I wish to make here is this: that first as a doctor and second as a psychologist I have never ceased to elicit direct expressions and reports of interior experiences—somatic, emotional, and intellectual—not only as sources of indications of overt actions to be executed in the future, but as indications of occurrences that are intrinsically important. For example, the occurrence of anxiety, or the persistence of unhappiness, or the generation of a new theory is as important to me when taken as a dependent variable (something to be predicted) as it is when taken as a hypothetical or intervening variable (an aid in the prediction of something else). Though imperceptible to us and therefore inferential, covert

mental processes and products, some with and some without the property of consciousness, happen to be intrinsically attractive to a cogitator of my persuasion, and I see no insuperable barrier to their being incorporated in a unified body of scientific facts and propositions. If the heavenly bodies had memories reaching back to the Big Bang and words intelligible to us, what astronomer would shut his ears to them on principle? Anyhow, this concern of mine, this reliance on a multiplicity of inferences, checked and rechecked, this vision of a theoretical system largely composed of psychological, rather than physical, variables, makes it necessary for me to leave exclusive positivism to those who deal with entities that are incapable of supplying us with valuable verbal representations of what has occurred and is occurring behind their surfaces. But enough said; I must return to my surgical internship and finish listing what I learned that influenced subsequent decisions respecting procedures and objectives in the field of psychology.

From medical practice I derived the "multiform method" of assessment, coupled with the belief that it should be possible for a group of trained collaborators using a wide variety of methods to make a reasonably complete examination, formulation, and appraisal of a whole person as an ongoing order of differentiated functional activities. This objective is achieved over and over again on the physiological level by practitioners of medicine but when transferred to the psychological level its attainment is impeded by innumerable special difficulties. To cut down my hope to size—to make it congruent with what can feasibly be undertaken under existing conditions, with available personnel, with existing concepts and existing methods—has been my enduring but never sufficiently successful resolution.

Also derived from medicine were consequential convictions respecting (1) the determining importance of biochemical occurrences—say digestion, assimilation, metabolism, excretion—after the organism has finished eating and the interest of the average psychologist has faded; (2) the ultimate scientific value of systematic, thorough, and detailed case histories; and (3) the necessity of an adequate classification of the entities and processes within the domain of one's elected discipline.

I have nothing more to say under the present heading except to avow that my special interest in the dispositions and thoughts (rather than the bodies) of human beings was one determinant of the rather sudden decision I made to shift from physiology to psychology. Also influential in some degree were the impressions (1) that human personality, because of its present sorry state, had become *the* problem of our time—a hive of conflicts, lonely, half-hollow, half-faithless, half-lost, half-neurotic, half-delinquent, not equal to the problems that confronted it, not very far from proving itself an evolutionary failure; (2) that psycho-

analysis had already made appreciable progress in exposing and interpreting the deeper processions of the mind; and (3) that my temperament was more suited to the making of coarse maps of newly explored areas than to the refinement of relatively precise maps of familiar ground.

INTEREST IN THE EARLIEST AND INNERMOST ORIGINS OF THINGS

It seems that I was scarcely four years old when, like a cornerstone, the law was laid in me that storytellers should begin at the beginning. The beginning was not only engaging in itself, but necessary to an appreciation of the rest—all succeeding adventures, stratagems, conflicts, loves, and triumphs of the hero. I felt with Aristotle: no beginning, no excitement at the climax, no catharsis. And so, if my father or my mother failed to start a fairy tale with "In the beginning," or its equivalent, "Once upon a time," I knew that I was about to be deprived of essential information and this, in my book of rules and regulations, was ground for protest.

But more consequential than this early requirement for a good fairy tale was my first down-to-earth attempt to latch on to the beginning of a course of actual events. The attempt was prompted by a sudden bellow that originated, I soon discovered, from a strange baby in my parents' room. Puzzled, I was told that this noisy creature was my brother and perfectly adorable. Here surely was a notable beginner; but what was the beginning of this beginner? My inquiry ended with the answer that Dr. Anderton, my mother's red-bearded physician, had brought him in his bag, the very bag from which I had so often seen him lift spatula, swabs, and stethoscope.

That I should have rested—I won't say comfortably—with the words "doctor's bag," that I should have abandoned my quest for basic knowledge after one essay, not followed the path of my intent, the path of infinite regressions, one leg further at the least, a step which would have taken me to the place where Dr. Anderton obtained the babe, that I should have quit so soon, is evidence of a docility or squeamishness so unsuited to the career of science that even now I blush to acknowledge it in print. If all along I have been stopped at the very verge of the unknown by some constitutional timidity, it is possible, yes probable, that I have failed to see, or to interpret properly, or to report candidly occurrences that were beyond the stretch of well-established scientific theories or beyond the bounds of embedded moral sentiments.

I have mentioned my halt at the "doctor's bag" conception of the fount of life—suggesting parenthetically that I might not have lain down too happily with this solution—I have mentioned this defeat of curiosity as a possible indirect determinant of what eventually became a con-

firmed interest in the earliest and innermost origins of things. This hypothesis might help to explain why, twenty-five years later, I elected to spend the greater part of three years in an incubator with several dozen eggs, observing and measuring the chick embryo's earliest manifestations of vitality. The point is that I managed at long last to get inside the doctor's bag, or, better still, at 103.6°F, almost within the womb of the beginning of a beginner. Peering through a microscope, through a little fabricated window in the egg's shell, spellbound as any libidinous voyeur, I witnessed the procession of momentous transformations that mark the hours when the embryo is no bigger than an angel perching on a pin point. Here, it seemed, were occurrences of great significance which into concepts no contemporary intelligence could digest.

The same hypothesis might serve, in some measure, to account for my disappointment, if not aversion, when I encountered the science of psychology at college and listened for a while to what was considered worth announcing about the perceptual processes of the adult mind, the mind of a Western intellectual, a mind without a history, strapped to a piece of apparatus in the laboratory. Also in keeping with this hypothesis was my subsequent embrace of Freud with all his facts and legends respecting the earliest months and years of life. Freud kept my first commandment: he began at the beginning. In my initial enthusiasm I hardly noticed that he never reached the consummation of the allegory, the heroic adult and his tragic end.

Depth psychology was obviously my meat. In the depths one came upon the earliest and most determining dispositions. Whatever initial doubts I had respecting unconscious psychic processes were soon enough dispelled. Several weeks with Dr. Jung at different times, three years with Dr. Morton Prince, an orthodox Freudian psychoanalysis, and a period of training with Dr. Franz Alexander and Dr. Hanns Sachs, ten years of therapeutic practice—these experiences were hugely influential in shaping my personality and my thought. But at no time, to the annoyance of my friends, was I a good Jungian, a good Freudian, a good Adlerian, or a good schoolman of any breed. I held all my teachers in high esteem, but judged that each of them—necessarily at this stage of theoretical development—was more or less one-sided. The notion which invited me was that of attempting, with the aid of additional ideas culled from the writings of McDougall, of Lewin, and of my colleagues at Harvard, a preliminary revision and integration of current academic and psychoanalytic theories to accord with a large collection of reasonably solid facts obtained by the multiform method of assessment. This effort resulted in the crude blueprint for a system which a number of us submitted in *Explorations in Personality* [3], a blueprint which stressed the earliest and least accessible determinants of behavior. We did not do this to the satisfaction of the psychoanalysts, first, because *all* behavior

was not traced to infantile sexuality and aggression and second, because we classified overt actions as they occurred, whether or not we had reasons to believe that they were subsidiary to deeper, hidden aims.

But now, if I may, I shall mention another disgrace of childhood which seems relevant to this topic—what I have called my interest in origins and beginnings. If, awhile back, I almost disqualified my childhood self as a potential truth seeker, by mentioning that moment of scarcely pardonable poltroonery in the face of the Great Riddle, what I now have to confess is evidence of something bordering on complete damnation in the scales of science. Not going beyond Dr. Anderton and his bag signaled a defect in daring and determination to solve problems; but worse than this is the inability to know a pithy problem when you see it. So far as I can recall, if truth will out, I was never prompted to ask about the *very* beginning, the beginning of mankind or the beginning of the world. Passively and without suspicion or comment I received the news that some six thousand years ago God—who, in pictures I was shown, had a big beard, not red like Dr. Anderton's, but white as my venerable and remote grandfather's—that God had constructed the first man in a single day, and, a little later, molded from one of this man's ribs the first woman, et cetera, et cetera.

I suspect that it was the swallowing and digesting of this fable, trustfully and without complaint, which determined, to some degree, my gust for Darwin and the evolutionists who succeeded him, as well as the joy I felt in shedding the constraining creeds of orthodox religion. It was as though a strait jacket had been unfastened and I stepped out to breathe and move and think for the first time without embarrassment.

It was from biology and chemistry that I received the exciting notion that man is descended from the very humblest of parents, a more or less fortuitous combination of chemical elements—such low-caste stuff as hydrogen, oxygen, carbon, and nitrogen—and that, instead of a day, it took two billion years or more to shape him. Also noteworthy was the evidence that the wondrous evolutions of man and of his productions may be credited, in some measure, to the very tendency which in the Garden of Eden version led to his disgrace and fall, that is, the inborn tendency to explore and to experiment among forbidden things.

My enthusiasm for this theory becomes more intelligible when viewed in conjunction with the next orienting disposition to be listed.

INTEREST IN PROCESS, CHANGE, DIFFERENTIAL DEVELOPMENT, CREATIVITY. INFLUENCE OF CHEMICAL EMBRYOLOGY

It is hard to decide: should I speak here of a predisposition that sensitized me to a certain class of facts or should I speak of a certain

class of facts which engendered a disposition to accept them and look for more of the same kind? I have always thought it good emotional policy not only to enjoy, so far as possible, the inevitable, but to will the obligatory. In this case, however, I am inclined to stress the inner bent ahead of the compelling facts because to the majority of psychological theorists these facts are not particularly compelling.

I am referring to facts which particularly attracted me during my studies at the Rockefeller Institute of the physiological ontogeny of chicken embryos. To summarize a long story, what seemed both most obvious and most important about the interior of the embryo were (a) the givenness, the inherent spontaneity, of its cellular activities and (b) the continuous sequence of orderly metamorphoses (clearly perceptible under the microscope) which resulted from these activities, and hence the necessity of including formative (constructive) processes in one's scheme of variables.

Unintrusive observation was enough to nail down the self-evident proposition that chemical and physical activity, metabolism and movement, are integral properties of every animate body, things to be included in the very definition of life. Also, it appeared that organic processes are not only primarily endogenous, autonomous, and *proactive* (initiated and sustained from within, rather than being *merely* reactive to external stimuli) but especially in the early stages of development are, so far as one can see, not perfectly coordinated with other processes, not constantly directed toward the achievement of effects *extrinsic* to themselves. This view of things was initially implanted by observing, time after time, the very first beat of the uncompleted embryonic heart and noting that it contracted irregularly and then regularly for quite a while—I forget how long precisely—before the blood vessels and the corpuscles were far enough along in their development to make it possible for this organ to perform its predestined function, namely, to pump oxygen-refreshed blood through the arteries of the body. The primitive heart was merely exhibiting its emergent capacity to contract, like a playful child or puppy, achieving for a period no effects outside its own growth of form and potency.

This notion of endogenous, initially undirected and uncoordinated, *process-activity* constrained me, in later years, on the one hand, to qualify my acceptance of the fashionable stimulus-response formula, with its implicit assumption of a nothing-but-reactive organism and, on the other hand, to qualify my acceptance of the proposition that all activities are motivated. In short, I take "life"—say, the ceaseless processes of metabolism—as *given*, just as Newton took motion as given, and do not look for something antecedent to it, except in an evolutionary sense.

The other influential impression I received from my studies of embryonic physiology was that, during the first phases of its career, a relatively large proportion of the totality of processes within a living organism is involved in the development of somatic substance, in the work, let us say, of anabolism, of incorporating and combining new elements, and so of constructing and of reconstructing parts unexampled in the history of that particular unity of animation. In other words, the most significant characteristic of the embryo is not so much the arrangement of its perceptible component forms of matter at this or that moment, as its activity in forming and transforming forms of matter. Defining "energy" as the capacity to produce change, change of relations, we can say that most of the energy of the embryo is devoted to generative changes, that a host of processes *precede* forms, one of the effects, or "functions," of some processes being to build and to rebuild them. That is, the organism, being an open system (as Bertalanffy showed me later), selects from its environment, incorporates and synthesizes, potentially energetic matter, and thereby increases its resources, taking a course opposite to that defined by the second law of thermodynamics (which applies to closed systems). Furthermore, clearly perceptible under the microscope were divisions of the soma into regions and in each region the production of distinctive structures, in short, morphological differentiation, preparatory to specialization of functions.

It was these observations of embryonic developments, besides what I could understand about the science of energetics, which initially predisposed me to stress "mythologies" of energy, process, change, function, more than "mythologies" of matter, structure, permanence, and to regard the organism as ordered successions of different kinds of processes, the effects of some of them being primarily internal—formations and reformations of component structures—with a re-ordering of the processes occurring consequentially. In short, according to this way of thinking, creativity is an inherent property of the organism and stability is another.

Four of the ultimate resultants of my interest in process, development, and creativity were (1) the adoption of the whole history of an organism, the entire life span of a personality, as the macro-temporal unit that requires formulation (although it may be half a century before a satisfactory way of doing this—an adequate conceptual scheme and an adequate methodology—is devised); (2) an interest in all manifestations of significant changes of personalities—progressive transformations, eliminations, and reconstructions, learnings, extinctions, and relearnings, regressions and deteriorations—and in the determinants of such changes, and hence a special, but by no means exclusive, emphasis upon the influential experiences of childhood; (3) a devotion to all forms of the imagination—dreams, fantasies, prospectings, ordinations (plans), plays,

story-constructions, myths, rituals, religious conceptions, works of art, and scientific speculations—as manifestations of involuntary and largely unconscious process-activities which, when influenced by a strong and continuing intention, may, in some cases, have a definitely creative outcome; and (4) the construction of a large number of methods (most of them unpublished) for the education and exposure of imaginal processes and products (so-called projective tests).

Imaginal processes and products appeal to me not only because of their intrinsic interest, but because they have been shown to be the best source of dependable clues of underlying (and often unconscious) dispositions and conflicts of dispositions. An often verified hypothesis is that some of these inferable dispositions are residua of deformative infantile experiences and that a few of them are prodromes of conditions in the offing.

OBSERVATIONS OF THE INTERDEPENDENCE AND
HIERARCHICAL INTEGRATION OF FUNCTIONAL PROCESSES:
ADOPTION OF THE ORGANISMIC CONCEPT. INFLUENCE
OF L. J. HENDERSON

It was in 1920, during my studies of oscillations of the physicochemical equilibria in the blood, under the tutelage of Lawrence J. Henderson, author of *The Order of Nature* [1], that I first became familiar with the organismic, or organismal, proposition, as formulated by E. S. Russell in 1916 and elaborated by W. E. Ritter in 1919. Belief in its essential validity was confirmed a little later (sometime before I heard anything about gestalt psychology) by observations of the embryo—perceiving the sequential effects of Spemann's genetical "organizers"—and by studies of the regulatory functions of the autonomic nervous system in conjunction with the endocrines. Clearly demonstrable in higher animals are *vertical* integrations of superordinate and subordinate loci of control, levels of directors and coordinators, "lines of command" starting from some center in the segmented neuraxis, or lower brain, and ending in regional plexuses and local nervous networks, a hierarchical *system*, depending on "feedbacks" (as we say today), which executes the genetically determined "domestic policy" of the organism.

Here it might be appropriate to refer to Cannon's principle of homeostasis, and to the fact that consideration of the radical developments during the embryonic period led me to stress the concept of progressive disequilibrium, continuity through expansive, constructive change, as a supplement to that of homeostasis (which is more applicable to the interior activities of adult organisms). The concept of homeostasis (the maintenance and, if disturbed, the restoration of the *same* state

of equilibrium) is a basic scientific induction, defining as it does the measurable relationships of multifarious interdependent elements and processes, relationships which either persist unchanged, or, if modified by some intruding element of exigency, are in due course reestablished. But it should be noted that this principle, as commonly defined, is valid only within a certain time span. The time span varies with the age of the organism as well as with the system (physiological, psychological, or sociological) that is under consideration. In the body of a healthy adult, the morphological, physicochemical, and physiological relationships are quite stable, or soon restabilized, over a period of many years despite the slow changes which eventually result in the signs and symptoms of senescence. But in the embryo homeostasis has virtually no span at all, or an extremely short one: the organism as a system being characterized in all its manifestations by perpetually changing states of equilibria, states that move in an *irreversible* direction. In short, the embryo is in disequilibrium or, at most, transitional equilibrium from first to last. Comparable, I thought later, though less striking to the eye and less susceptible to precise measurement, are the seasons of transitional equilibrium on the psychological level, which occur most obviously in childhood but also later, during the early phases of some new enterprise, let us say, or when the creative imagination is steadily advancing. At such times psychological processes are transformative, and when they terminate, the person is a different person, or his sphere of relationships is different, and there is a different equilibrium to be sustained.

Although I came away from my embryological studies with a firm belief in the unity of the organism through change, in orderly differentiations and integrations, my medical training had established a special vigilance in respect to signs and symptoms of functional imperfection, and I soon discovered how normally abundant are such evidences on the psychological level, evidences of disunity, of retardation, deviation, deformation, and retrogression. It appears that millions of years of evolution have resulted, on the one hand, in an almost perfect system, let us say, of somatic operations, and on the other hand, in a human brain which contains at birth no comparably ordered system of dependable proactions and reactions, but instead, a matrix of potentialities in a relatively amorphous state, potentialities for unprecedented developments of talent, at one extreme, and for idiocy and lunacy, at the other. Hence, especially for human beings, life is a continuous procession of explorations, surmises, hunches, guesses, and experiments, failures and successes, of learnings and relearnings—aging consisting of a sequence of gradual and occasionally abrupt *indurations* (rigidifications, solidifications, fixations, hardenings), both of forms and functions. Consequently, a psychologist has to deal conceptually with doubt, distrust, indecision,

and postponement of behavior among his subjects, together with occurrences and continuities of competition and paralyzing conflict between their dispositions.

My bent toward organismic, holistic, molar, or "gestalt" conceptions of the personality and its activities scarcely fitted me to wax avid when I encountered, later on, the then dominant elementalistic, connectionistic, chained-reflex, molecular theories of learning, theories that were being hungrily ingested by all who cared on what side their academic bread was buttered. Of the two fallacies, reductive and seductive, so nicely discriminated by Herbert Feigl, I was more liable to the second, though I had no use for those lazy white elephants of the mind—huge, catchall, global concepts signifying nothing. Eventually I was persuaded by Professor Boring—more generous of his time than any teacher I ever had—that the principles of elementalism and associationism are applicable under many circumstances, especially, let us say, to the establishment of certain neurotic symptoms as well as to conditionings that occur below the level of conscious control or when the mind is tired or confused and functioning below par. In short, elementalism (emphasis on parts, integrants, components) and holism (emphasis on wholes, integrates, ordinations of components) are necessary complements.

INTEREST IN THE DIRECTIONALITIES AND EFFECTS OF OVERT BEHAVIORS

One passes by inseparable gradations from an interest in the autonomic-endocrinal coordination of the multifarious somatic processes of the body and in the local effects of their different operations, to an interest in the cortical ordination of sensory, muscular, and verbal processes toward successive achievements of different overt effects, most of which endeavors, if successful, contributing in some way or other to the well-being of the total organism.

Hence, it was already in the cards I held that, on entering the domain of psychology, I should very soon become concerned, not so much with reflexes and patterns of muscular movements, as with the various changes effected by such movements and the changes in the states and thoughts of other people effected by spoken words and sentences.

The fundamental fact, it seemed to me, is the survival of the living organism, the continuation of its metabolic processes, and the dependence of this procession upon the periodic attainment of a number of distinct effects, such as the inspiration of oxygen, the expiration of carbon dioxide, the ingestion of water and food, and the excretion of waste products. The different processes, modes, and subeffects whereby the

same kind of terminal effect is achievable in different species of organisms or even in the same organism at different times were, at the start, a matter of considerable, but subsidiary, importance. A man shivering with cold may restore optimal body temperature by moving to a warm place, putting on an overcoat, closing doors and windows, lighting a fire, turning on the radiator, taking a hot drink, or exercising strenuously. Defined in terms of physical vectors (locomotions or manipulations in space) these are different actions, but the beneficent effect in all cases is the same. Indeed, a person may obtain all necessary "goods" with the minimum of activity on his part: they may be furnished providentially by nature, gratuitously by parents or friends, or in exchange for money, by domestic servants or employees. I had a good friend who lay in bed, blind and completely immobilized from his neck down, for twenty years. He had a sensitive and brilliant mind that was bubbling over with unimpaired effectiveness and charm until his death, and yet he saw nothing and never moved a muscle. Every act necessary to his survival, to the stimulation of his feelings, and to the increase of his knowledge had to be performed by someone else. This was but one of countless observations which persuaded me of the necessity of providing concepts for the analytical dissection, whenever necessary, of any short segment of activity into (1) kinds of exciting *initial situations*, (2) kinds of *processes* (e.g., covert psychic processes, overt psychomotor or psychoverbal processes) with or without kinds of utilities (e.g., tools, weapons, conveyances, telephone, typewriter, etc.), (3) kinds of *modes*, or styles, of processional activity, kinds of psycho-expressive processes (e.g., speed, grace, gestures or tone of voice expressive of uncertainty, anxiety, self-confidence, anger, good will, deference, compassion, etc.), and (4) kind of *effect* (*change* from the initial exciting to the terminal gratifying situation).

In my persistent efforts to move, step by step, toward an adequate solution to such problems, I was greatly assisted by the reported observations and formulations (1) of biologists from Darwin on, and of others, particularly McDougall, who had used the concept of instinct as their tool; (2) of Freud relative to the sex instinct, aggression, and anxiety, and of Adler relative to the craving for superiority; (3) of Tolman and other animal psychologists who had carried forward the endeavor to define and measure rigorously different drives; (4) of Lewin with his constructs of tension system and of quasi need; and (5) of sociologists regarding the wants of men for status and for power.

In *Explorations of Personality* I attempted to define a number of actional dispositions which, in the absence of a less objectionable designation, were termed "needs" (or "drives"). These constructs proved useful in categorizing inferentially the overt behaviors of the subjects we

studied as well as the behaviors of the characters in the stories they composed. But this particular working inventory of human drives (kinds of motivations, purposes, intended effects, goals) was, in several respects, deficient, and ever since, these deficiencies have kept provoking me to prolonged efforts to conceive of fitting remedies. An account of today's resultant of my arduous and still continuing endeavor to arrive at a more comprehensive and integrated system will be presented in a later work.

Before leaving this topic I should say that I have not been satisfied to limit my objective to the formulation of overt behaviors, certainly not to the formulation of purely physical behaviors. Indeed, after perceiving that the food-ingesting activities of animals and of men are not at all representative of the majority of human actions (as Maslow has pointed out) but, being most readily formulated in physical terms, are repeatedly used nonetheless to illustrate this and that concept or generalization or to serve as foundation for this and that postulational system, and that they thus constitute an alluring conceptual trap for the unwary theorist—perceiving all this, I established in myself a prohibition (which I guiltily break occasionally) against using the hunger drive and its ensuing motor patterns and effects as paradigm of directional behaviors or even as a reliable reference point for speculation.

As I see it, a psychologist should be concerned not only with the formulation of overt interpersonal verbal communications, the immediate (intended) effects of which are changes of some kind among the dispositions, evaluations, represented facts, interpretations, or commitments of the other person, but also with the formulation of covert intraverted mental activities, the immediate (intended) effects of which are such things as: a better interpretation and explanation of some recalled event or of some current physical symptom, a reevaluation of one's own enactions (past behaviors) or present abilities, the definition of the content and boundaries of a required concept, the composition of the plot of a story to be written, the resolution of a conflict between two purposes, or the ordination of a plan of action (tactics) to be executed at some future date.

INFLUENCE OF WHITEHEAD AND LEWIN: CONCEPTS OF PHYSICAL FIELD, CATHEXIS, PROCEEDING, SERIAL, ETC.

I owe much to the incomparable Alfred North Whitehead and the incomparable Kurt Lewin, nothing less than the conviction that concrete reality is to be found only in the momentary. With theoretical physics in mind, Lewin devoted a good deal of his unusual imaginative powers to the definition of space constructs, topological and hodological,

the *momentary field*; whereas Whitehead, founding his penetrating reflections on organic and mental phenomena, emphasized the *momentary process*, the perpetual becoming and perishing of "actual occasions" and the historic continuity or progression of these occasions. Although I have never gained sufficient understanding of Whitehead's terminology to apply the categorial scheme of his philosophy of organism to the realm of ordinary human experience and behavior, I am indebted to him for a number of conceptions which I have revised to suit the purposes of a psychologist. First among these is the concept of an *event*, or fact, as a participation of processes in which two or more interdependent entities are involved occurring in a certain place or along a certain path, within a certain medium, through a certain segment of time, and resulting in a certain kind of change. I conceive of a range of events of different molarities. Theoretically, an ultimate *submicro event* would have the smallest *spatial scope* (smallest containing field), the smallest *entity scope* (fewest component particles), smallest *process scope* (fewest distinguishable changes), and shortest *temporal span* (duration). (For example, it is estimated that tau and theta mesons are composed and decomposed in about a hundred-millionth of a second.) Some micro events occur within the boundaries of solids, i.e. entities that can be treated as solids under most conditions (anything from a crystal to a planet), but others are integrated, synchronously and sequentially in time, in such a way as to constitute an event of greater scope and span, and this event, in turn, can be seen to constitute a necessary part, or phase, of an event of still greater scope and span, something that can be defined in terms of a single resultant process—secretion of a hormone by one cell, a single color sensation, influxion of a single image, contraction of a single muscle fiber—or in terms of a longer or more massive process—secretion of saliva, contraction of the heart, perception of a configuration, momentary feeling, evaluation of an object, movement of a limb, etc. Such an event may be a part of a yet larger, longer whole—say, a stimulus-response unit (perception, apperception, and evaluation of a pertinent entity, concurrent emotion, actuation of a pattern of muscular movements against resistance, production of an effect, perception, apperception, and evaluation of this effect). Thus, by increasing step by step one's scope and span of concern, one arrives at the largest and longest definable unit of activity, a *macro event*. A personologist usually has to deal with macro events, or *proceedings*; and from the fullness of each of these he abstracts those variables which are relevant to his purpose, in the knowledge that numberless other variables will be unrecorded and hence omitted from his formulation. Thus, the major concepts of the scaffold to be built—such as need, entity, configuration, process, succession, effect, place, route, time—are all considered to be abstractions from an event or progression of events.

As twentieth century inhabitants of the Western world, we seem to be living and acting—partly as a consequence of our acquired Indo-European language—in euclidean space, moving about on the supporting surface of an assumptively permanent material planet amid a great variety of substantial objects, inanimate and animate, natural and artificial (man-made), some transient, some relatively permanent, each with its distinguishing physical attributes. “Such presumptions,” as Whitehead says, “are imperative in experience,” and “in despite of criticism,” we still employ them “for the regulation of our lives.” And so, for better or for worse, I too have employed them, not only in the regulation of my life but, with certain qualifications, in the regulation of my theorizings.

If I were forced to choose one side of the age-old antinomy between the “metaphysics of substance” and the “metaphysics of flux,” my temperament would decide in favor of the latter, the version of the universe that is linked in our minds with some vivid sentences attributed to Heraclitus. But, as I see things and events, it is not necessary to go to one side or the other, either of this classical division between different aspects of nature or of other dichotomies, such as that between matter in space and motion in time, or between instantaneous configurations of material bodies and modifications of these configurations, or between chemical structure and chemical properties and processes, or between form and function, or between anatomy and physiology, or between entity and activity, or between actor and action, or between noun and verb. It is possible to choose both sides and combine them in single propositions.

Perhaps my most influential basic model is that of biochemical metabolism, repetitive and restorative as well as progressively and irreversibly transformative: the lifelong succession of *compositions, decompositions, and recompositions* of conrescences and perishings, of vital chemical substances. Here is incessant *flux* certainly, with the catabolism of anabolized materials liberating the energy for every manifestation of vitality (thermal, chemical, electrical, mechanical—emotional, dispositional, mental, and muscular); and here also are countless instantaneous configurations of *substances* within cells, of cells within organs, and of organs within a body, some parts of which (skeleton, ligaments, connective tissue, skin) are relatively solid and enduring like the framework of a house. Consideration of anabolisms, in which two or more chemical entities combine to form or to re-form a more complex entity, where one can attribute the course of events to no single actor and his act, has led me to conceptualize, in many cases, *systems of participant entities and participating processes* rather than placing the major burden of determination on one person or on one person’s conscious purpose. Here one might think of the mental participations involved in creative activity, with conscious intention playing but a minor role, or of the emotional, verbal, and actional participations of two lovers.

At this point let me explain for clarity's sake that in view of the mind's tendency to "spatialize" everything, as Bergson pointed out, and in view of the ambiguous usages in the social sciences of such words as structure, configuration, form, pattern, integration, etc., I prefer to restrict the word *configuration* to the instantaneous (transient), and the word *structure* to the enduring, *spatial relations* of the *substantial components* of an entity, assemblage of entities, or region (extended surface area), and to use the word *succession* to designate the once-occurring, and the word *integration* to designate the recurrent *temporal relations* of the *component processes* of a *proceeding* (uninterrupted activity, endeavor, interaction). According to this terminology it would be proper to speak of the structure of a house, of a painting, of an organism, of a chemical compound, of a crystal, or of an atom; and it would be proper to speak of the integration of mechanical, electrical, chemical, mental, verbal, or musical processes, through a certain period of time. One could also speak, in a highly abstract way, of the hypothetical structure of the mind or of the personality, although mind and personality are known to us only through successions of covert (subjective) and overt (objective) processes. You see I am wary of the word "structure," because, if used to describe concatenations of activities one gets that impression of permanence, regularity, and lawfulness which is so dear to the hearts of scientists and yet so incongruent with the facts in many instances.

The debt I owe to Lewin can be most simply set forth if we restrict thought, for the time being, to the motor activities of one person from the beginning to the end of a single simple *proceeding*, or endeavor, a goal-directed and goal-attaining course of action. In such a case the "whole" effect (attained goal) of the pattern of muscular processes can be defined by designating the relevant differences between the structure of the physical field at the initiation of the activity and the structure of the physical field at its termination. This will tell us what the person "did"—he moved, let us say roughly, from one location to another, or moved an object from a table to his mouth, or put a new tire on his car, or hung a picture over his desk, etc. But more than this, ideally considered—and here is where Lewin comes in—a sufficient characterization of the field at the start of the activity, and at every instant from then on, would set forth the immediate determinants of the over-all direction of the activity as well as of each successive part, or unit, of the whole. As Lewin put it, "the behavior b at the time t is a function of the situation S at the time t only," where S denotes the total situation (field)—the field of forces within the person (internal situation) as well as the field of forces exterior to the person (external situation), as apperceived and evaluated by the S . The initiating total field (a momentary cross section,

or time-slice, through everything that is influential) determines the behavior resulting in the next field, which, in turn, determines the behavior resulting in the subsequent field, and so on, until the occurrence of an act resulting in a field which determines the cessation of that variety of endeavor. In Lewin's scheme of constructs, as in mine, the major variable of the internal situation (internal field) is some kind of excitation (with direction and magnitude)—a quasi need, need-aim, or drive; but here I am stressing the external situation.

It should be noted in passing that an adequate formulation of the immediate, or antecedent, determinants of behavior can never be given in terms of the *instantaneous* external situation (configuration of space or of objects or of forces in space). Even in the extreme case of a wholly stationary external environment one must take account of the process through time of the subject's perception, apperception, and evaluation of the situation; and this brings us to Whitehead's actual occasion, the "real thing." In most cases, the so-called momentary external situation (set of antecedent determinants) is likely to consist, not so much of a spatial configuration, as of a rather long pattern of symbolic processes, such as a paragraph of instructions read to the subject by an experimenter. But, let us return to the simple case of a stationary physical field in which a mobile person is positioned and consider what kind of map should be made of this so-called momentary situation.

Man being a terrestrial organism for the most part—for the duration of this discussion, anyhow—the space to be represented will be a two-dimensional flat surface, natural or artificial—either a circumscribed area of ground (composed of rock, soil, or sand) or a floor area within a building. This area we shall call the *territory* (the total spatial scope of our concern), and this territory (say, a sparsely settled rural area) we shall divide into *regions*, and these regions into subregions, and so on indefinitely, if necessary, until we arrive at a multiplicity of *places*. Each region will have a certain area and shape and will be distinguishable from other regions by the number, position, and physical attributes (size, shape, color, etc.) of its *occupants* (say, an assemblage of trees, of potato plants, of weeds, or of buildings), or by the absence of occupants, and/or by boundaries (walls, fences, hedges), not to speak of brooks and rivers. Furthermore, there will be strips with smooth surfaces connecting some of the differentiated regions, which I shall call *routes*, one of which will run through a subregion occupied by buildings, each marked by sets of symbols, one set indicating that food may be purchased there, another indicating tools, another drugs, and another clothes. Let this suffice as an account of the structure of the space relevant to our problem. Now Lewin was shrewd enough to see that a map of such a territory showing the location of physical objects and their attributes,

mere patterns of sense data, or *mere* primitive perceptions, is of little relevance to psychology. A modern artist, by a conscious effort, might view his environment in this way, or possibly a visitor from Mars; but even in the above-given bare description of the territory I could not without misunderstanding omit such words as trees, potato plants, buildings, fences, routes, food, tools, drugs, and clothes, all of which words refer in a rough way to objects which not only make themselves known to our senses by means of their physical attributes, but which, under certain conditions, are capable of contributing to (or, in other cases, subtracting from) our welfare. Hence, it is not so much the physical attributes as such but the known or supposed man-pertinent capacities of objects which influence behavior (including the capacity of some objects to delight the aesthetic sensibilities of the subject). It was these pertinent capacities that Koffka and Lewin had in mind when they spoke of the "behavioral," or "psychological" environment, the environment of meanings or significations.

This point of view was congenial to the one at which I had arrived with the help of Uexküll. Accustomed to the distinction between the attributes and properties of chemical compounds, I had made a comparable distinction between what a human object "looks like" and what he "does" under specified conditions. Here I am leaving out, for the moment, what an alter does to the subject solely by virtue of her or his physical attributes (beauty, ugliness). What an alter does, the kind of thing he does, *to the subject*, I called a *press* (plural: *press*). For example, the press of Mr. X vis-à-vis a given subject might be "to animate him (the subject) intellectually," just as the usual press of the drug Benzedrine when taken by mouth is "to stimulate mental processes." The *capacity* to stimulate is one of the biochemical *properties* (latent press) of Benzedrine, and when Benzedrine passes into the blood stream the property becomes manifest as a process distinguished by its effect. Similarly, a known alter, regarded from the subject's point of view, can be represented as an assemblage of *subject-pertinent properties*, or latent press, which will be manifested as processional effects (operative press) either spontaneously or after appropriate stimulation, when the subject and the alter meet. Thus, as I saw it, the physical structure of the environment was representable in terms of the geometric configuration of regions, places, and objects, each with its potentially effective subject-pertinent properties (latent press). That strip of smooth surface over there is called a *route* because from position A to position B it has the property of supporting a human body or a conveyance and of facilitating locomotion; and boards cut from those trees have properties suitable for the excluding walls and supporting floors of houses, etc., etc. As a consequence of countless past experiences, such properties seem to be re-

vealed to us immediately by *mere* perception, but at this point I prefer to speak of apperception, or apperceptive perception, since it is convenient and often important to distinguish verbally between the clear impression and identification of a particular kind of object—say, a hickory tree—and the realization of its properties—say, the properties of hickory which make the wood especially suitable for ax handles. The chief difference between the conceptualization of a pertinent property of an inanimate object, such as a drug, and a pertinent property of a person (alter) is that, in the case of the latter, one is dealing with a mobile object whose activity may be unprovoked by the subject, and one must distinguish between an endeavor that fails (through incapacity) and an endeavor that succeeds. It is the difference between a pressive disposition without ability and a pressive disposition with ability. But more of this later; I must return to my topic.

There was a wide gap, it always seemed to me, between Lewin's symbolic constructs on the level of physics (representative of public physical events) and his constant references to a miscellany of wholly private psychic processes in his subjects which he cleverly distinguished by intuition, but which he spoke about as if they were overt and obvious to everyone, or could be reliably inferred on the basis of observed behaviors. Not many psychologists realized so clearly as did Egon Brunswik, that for Lewin the exterior field (the environment) was within the subject's head. What Lewin called the "psychological environment" is the subject's apperceptions of the environment—a necessary construct; but it stood alone, no place having been provided for a more "objective" definition of the environment, say, as apperceived by the psychologist, by selected judges, or by the conventional majority. Thus by Lewin's scheme it is not possible to distinguish between a morbid delusion and a realistic, or congruent, estimation of the external situation: the situation is exactly what the subject thinks it is, or more accurately—since Lewin rarely, if ever, asked a subject—it is what you think the subject thinks it is as you empathically perspect his thoughts during the course of his behavior. Furthermore, if the humanly pertinent properties of other environmental objects (as estimated by the psychologist) are never mentioned, we shall never know how much of the external situation was rejected by the subject.

As a step toward the clarification of this issue, a number of us, stimulated by an extended definition of Freud's important concept of projection, conducted numerous investigations of differences between the external situation as carefully and systematically perceived and apperceived, say, by a consensus of trained observers (the *alpha* situation), and the same situation as perceived and apperceived (under conditions less favorable to accuracy) by subjects with different personalities (each

a *beta* situation), or by subjects in a certain experimentally engendered temporary state. This is the sphere of concern which is now called "personality and perception."¹

The bulk of our experimental findings were unanimous in their verdict respecting the importance of dispositions (interests, evaluations, and needful tensions) in determining the outcome of perceptual, apperceptual, conceptual, compositional, and ordinal (planning) processes. In short, as antecedent determinants of overt behavior, one must include, not only the structure of properties and processes of the confronting exterior situations as arranged by the experimenter (cluster of independent variables, or *alpha* situation), but perceptions and apperceptions of certain of these things (*beta* situation) as determined by the dispositional state of a given personality or type of personality (cluster of intervening, hypothetical, or conventional variables).

Besides many other things, this meant to me (with my memories of chemistry) that a psychologist will bring in less knowledge by viewing a person as a mass-point of indifferent constitution in a field of forces, as Lewin (with his interest in physics and his image of Galileo at the tower) was tempted to do, than he will by viewing him as an entity with a particular conjunction of distinguishable properties. It is, of course, true that in establishing some sorts of lawful relationships be-

¹In this and in other related enterprises, fortune favored me with early colleagues of the stature of Erik H. Erikson, Donald W. MacKinnon, Saul Rosenzweig, R. Nevitt Sanford, and Robert W. White, of William G. Barrett, Kenneth Diven, Isabelle V. Kendig, Walter C. Langer, Christiana D. Morgan, and Carl E. Smith; later of Thelma G. Alper, Leo Bellak, Vera V. French, Elliott Jaques, Robert R. Holt, Daniel Horn, Morris I. Stein, Silvan Tomkins, and Frederick Wyatt; and, more recently, of Gardner Lindzey, of Anthony Davids, Richard V. McCann, and Robert N. Wilson. I have also been advantaged by collaborations, all too short, with Freed Bales, Tamara Dembo, Cora DuBois, Walter Dyk, Jerome D. Frank, Christopher Fried, Åsa Koht, Philip Lichtenberg, Goodhue Livingston, Charles C. McArthur, H. Scudder McKeel, James G. Miller, Merrill Moore, Hobart Mowrer, Benjamin J. Murawski, and Henry W. Riecken, as well as with a host of others on the OSS assessment staff during the war years, of whom Edward Tolman and John Gardner have, in interior dialogues, admonished me most often. Among these warm friends and coworkers I have no reliable way of apportioning the credit for leading me to relatively valid concepts and for canceling many of my least propitious errant speculations, and no reliable way of apportioning the blame for withholding criticism at moments when I might have been deterred from this or that cognitive folly. Anyhow, I am grateful for the opportunities I have had to serve as one of many channels for the ebullient ideas that have swirled and eddied round the table at the Harvard Psychological Clinic. And here I must make public my profound indebtedness to my good friend and critic, Gordon W. Allport, staunch champion of minorities, without whose timely advocacy the Clinic might have been dissolved and left no wrack behind.

tween entities it is possible to disregard differences of constitution, but even in physics, how often can one predict the outcome of an experiment without taking into account the internal structure of the molecules, or such properties of substances as conductivity or melting point? In short, on the down-to-earth empirical level (as contrasted with the sphere of transcendent, or purely hypothetical, entities) one must include in one's formulations the properties (in specified states) of the entities engaged in the observed transaction. For example, some material entities are nourishing to human organisms, others stimulating, others soporific, and others lethal, and one property of some human organisms in a certain (suicidal) state is to select a lethal rather than a nutritive entity for incorporation. I would delete these references to the obvious, if it were not for the fact that most of us, in our endeavors to be objective, to formulate behavior in terms of perceptible movements—say approach and ingestion for survival—forget, for example, that poison is attractive to persons in a certain state. In short, we cannot throw Aristotle to the dogs and restrict our diet to the more elegant formulas of Galileo: chemistry is still among the reputable sciences and closer to psychology—think of oxygen, digestion, metabolism, and endocrines—than is its more admired older brother.

Another related conclusion supported by our findings was that the historic succession of the dispositions and experiences of a scientist has a great deal to do with the concepts and theories that he comes out with, and largely because of this conviction, I have often taken pains, as by request I am taking now, to expose my inborn and acquired bents and biases, rather than to make a great to-do about my exemplary scientific objectivity. It happens that one of my inductions from experience is that many of those who spend most time asserting their immaculate empiricism are somewhat below average in their awareness of the distorting operation of their own preferences and ambitions and, therefore, are more liable than others to sally forth with reductively incongruent versions of reality.

Additional concepts for the present scaffold. Among the other conceptual consequences of our studies of personalities and their apperceptions of other personalities and of my attempts to analyze single proceedings, six may merit definition.

Cathexis. From Freud I gratefully accepted the concept of cathexis (value, valence) as a useful variable in formulating personalities as well as single interactions of personalities. But instead of limiting its application to a loved person (the power of an alter to attract, enchant, and bind the affections of a person), I defined it as a possible disposition-evoking capacity of *any* kind of entity, or of any kind of activity of an entity, chiefly the capacity (1) to excite attention (interest, concern,

thought, talk), or (2) to excite attention plus evaluation, either positive (favorable—say, gust, wonder, admiration, love, approval) or negative (unfavorable—say, disgust, contempt, disapproval, distrust, resentment, fear), or (3) to excite attention plus evaluation plus pertinent activity. All types of entities seem to be capable of such evocation—a certain kind of food, a homestead, a utility, a person, a social institution, a novel, a moral code, a scientific theory, a philosophy of life—and similarly capable are all types of activities of entities. Not only a total entity, but any part, integrant, or component activity of an entity may have the power to attract attention, to please or to displease, to instigate activity. You may like a person as a whole but not like certain things he does, or you may like certain things he does but dislike him as a whole. A father spansks the boy he loves because he hates lying and hopes to spank this habit out of him, and so forth. The same might be said of the negatively cathected (and hence deleted) parts of a book in process of composition, a book which, taken as a whole, may be highly cathected by its author.

My present notion of cathexis is not far from the elaborate definitions of it that were published in *The Clinical Study of Sentiments* [4], except now the more favored term is "value" and the concept has been incorporated in a larger system. The term "sentiment," "attitude," or "established evaluation" points to dispositional property of a personality which corresponds to the cathexis of an entity. One can say that subject A has a strong sentiment or attitude (established disposition) *pro* X, or that his consistent evaluation of X is highly positive, or that X has a high positive cathexis or value for A. Both terminologies are useful. The concept of cathexis is also useful, perhaps most useful, in indicating the *subject's* effect on other people: in what quarters and to what extent he will evoke positive evaluations, based, say, on affection, erotic love, admiration, or compassion, and leading to accessions or invitations, associations and conjugations, compliances, services, or donations, etc., in what quarters and to what extent he will evoke negative evaluations, engendered by disgust, contempt, moral condemnation, or envious resentment, and leading to rejections, exclusions, decessions, expulsions, or inflictions, etc. It is not sufficiently acknowledged, I surmise, that a full characterization of a personality should include, as does the characterization of a chemical compound, the varieties of dispositional effects the subject has on different kinds of alters.

Dyadic system. The notion came and stuck that a dyadic (two-person) relationship, whether transient or enduring, should be formulated as a single system, equal analytic attention being devoted to each participant. Although I have never been inclined to accept Harry Stack Sullivan's restriction of the domain of psychology to the sphere of inter-

personal relations, I use dyadic interactions as a test of every formulation or theoretical system I encounter in the literature. If the proposed set of antecedent environmental variables does not provide for the definition of an alter's subject-oriented verbal or physical behavior (e.g., such kinds of "stimulation" from the alter as petition or command, praise or reprimand, inquiry or offering of information, expression of good will, and so forth), if it does not provide tools of this sort, then the system is not suited to the representation of the great bulk of human reactions. It may, of course, have other virtues, but not those I require: variables appropriate to the prediction of concrete social episodes.

Thema. The idea matured that the basic pattern of a single dyadic interaction might be most simply represented by *i*, a symbol denoting the immediate direction, the need-generated orientation (goal), of the proactivity emanating from the first interactor, followed by *ii*, a symbol denoting the emotional response of the second interactor, and when indicated, a symbol denoting the need-generated orientation (goal) of his reactivity. Whether the goal of the first interactor's (subject's) activity is the aim of an independent need (and hence intrinsically satisfying if achieved), or the aim of a subneed (satisfying if achieved although it is no more than a subordinate component of a large system of need-aims), or the aim of a quasi need (merely instrumental and hence not intrinsically satisfying) would be a question for further investigation. Months of antecedent study and subsequent exploration might be required to determine the probable status, or relative potency, of all the needs involved in a single sentence. The same applies to the need-determined response of the alter. On this level of formulation (the formulation of a single proceeding), it would be sufficient to represent the immediate need-aim of the subject (proactor) and the need-response of the alter (reactor). The need-response of the reactor, viewed from the subject's standpoint, has been termed a *press*, the *alpha press* being the alter's actual response and orientation (in so far as he and the psychologist can define it) and the *beta press* being the subject's apperception of the alter's response and orientation. The simplest formula, then, would be either an N-P (if the subject initiated the interaction) or a P-N (if the alter acted first). This I termed a *simple micro thema*, a *simple macro thema* being an over-all, and hence much coarser, formulation of a longer transaction, and a *serial thema* being an articulated procession of simple micro themas, which might or might not be representable as a macro thema.

I might clarify this a bit by illustrating *complementation*, the simplest type of dyadic thema (others being reciprocation, cooperation, competition, opposition). Let us assume two interactors: X a confirmed *transmittor* and Y a confirmed *receptor*; and then, out of a large number of

complementary needs, let us choose the following pairs, and finally, let us assume that in each case the episode is completed to the satisfaction of both parties (criterion of a veritable complementation).

<i>Subject X, transmitter</i>	<i>Subject Y, receptor</i>
Need to inform (to relate facts, rumors)	Need for information (state of interest, curiosity)
Need to explain (to interpret events)	Need for explanation (state of perplexity)
Need to counsel (to give advice)	Need for counsel (state of indecision)
Need to amuse (to tell a funny story)	Need for amusement (readiness for mirth)
Need to console (to express sympathy)	Need for consolation (state of distress)

These pairs can be taken to represent the state of affairs in a dyadic system, at the start of five different proceedings. The transmitter is characterized by the tension of a valued fullness (*pleni-tension*)—he has a mental possession and the need to impart it—whereas the receptor is characterized by *lack-tension*, that is, by a need for something, something which, in this case, the other person is capable of giving him. Assume, for example, X has a mental possession (a funny story) which he is keen to communicate and Y is keen to hear a funny story. As a rule, there will be mutual satisfaction if the story strikes Y as funny and he responds with a hearty laugh. Further analysis may reveal that the apparently pleni-tensive transmitter has nothing very interesting to say but merely a strong (processional) disposition to babble (verbosity), and/or a lack-tensive need for attention and appreciation. Similar is the next type of dyadic pattern, *reciprocation*, except in this case we have a reciprocal complementarity, the second phase being marked by a reversal of roles—the former receptor transmits with an appropriate degree of zest and the former transmitter receives with due appreciation.

Consideration of long sequences of interpersonal themas of this sort has pretty much confused me respecting the proper usage of the venerable S-R concept. The intended effect (need-aim) of much proactive talk (reactive to the mere sight of another person) is an appropriate kind of sympathetic response (press) from the alter (e.g., expression of agreement, compliance, interest, mirth, affection, admiration, gratitude, and so forth), and there seem to be a good many hypomanic (chemically stimulated) self-starters and transmitters in the world who, instead of predominantly *responding* to other persons, sail forth each day full-freighted with a miscellany of impatient stimulations for any acquaintance (releaser) who might be capable of the complementary responses; and when a conversation is once launched, every response is a stimulus to a response which is a stimulus to a further response, and so forth, until the tidy S-R model has been so thoroughly rolled through all things that it looks as if it needed treatment, some sort of radical

rehabilitation. Perhaps it has already been rehabilitated, without my knowledge, by the more advanced S-R theorists.

It became evident in due course that a simple thema, whether micro or macro, is no more than a very coarse, though often meaningful and convenient, classification of an episode. To formulate an episode in a more refined way numerous other variables must be included until one's initially simple representation of its major dynamic components has been transformed into a *complex thema*. Among the immediate determinants, for example, of Y's positive or negative reaction to a "funny story" told by X, might be the "appropriateness" of the situation (never mind now how this is judged), the relative status and degree of intimacy of X and Y, the mirth-potency of the story, whether it is new or stale to Y, how well it is told by X, whether Y is momentarily at odds with X, the current mood or state of Y, the acuteness of Y's sense of humor in general and for this kind of story in particular, how fastidious is his standard of wit, to what extent is Y's system of values susceptible to offense by this kind of story, and so forth. Just as some psychologists have profitably devoted a professional lifetime to the study of a hungry animal in a maze containing food, so might others spend rewarding years in investigating the interior and exterior determinants of any one of a hundred other common types of themas, say, a thema with an unsuccessful or unexpected outcome, such as "the joke that fails," "the command that is defied," "the conjugal proposal that is rejected," "the injury that is forgiven," and in each case, why?

My own attempts to practice what I am now preaching—to explain in some detail the course of a single type of interaction—have been spotty and rather crude, and, for the most part, this side of publication. Christopher Fried, Philip Lichtenberg, and I have separately spent two years or more investigating a few of the determinants of the dyadic themas that occur during film-recorded competitive and cooperate attempts to arrive at a common plan of action; and, of course, there have been countless "clinical" occasions for minute perceptions of other common patterns. But, on the whole, the facts compel me to acknowledge that, except for resolute endeavors over the last twenty years to analyze and formulate the apperceptible processes and products that occur during impromptu compositions of dramatic stories, I have not focused long enough on any single type of thema or on any single method of observation and measurement to come out at last with a brilliant cluster of decisive findings. Decision has been difficult, because if a would-be personologist should elect to devote his energies to the building of a miniature system of postulates and theorems applicable to the understanding of one kind of thematic unit, he would have no time for the observation of other varieties of behavior; hence he would never get

around to the practice of his profession, namely, the investigation of the *interrelations* of the more determining gross components of personality.

Thematic dispositions. It has become more and more apparent to me that the energetic components of personality can be better defined as thematic dispositions than as general actional dispositions. For example, instead of saying that X possesses the trait of aggressivity, or that he has a ready and intense need for aggression, one should, if possible, specify the nature of the pertinent press (stimulus) and say with more precision that two of the properties of his personality (I won't translate this into symbolic shorthand) are supersensitive dispositions to react with resentment and aggressive words (1) to apperceived insults to his self-respect and (2) to apperceived vainglorious boastings by an alter.

Serials. I was slow to perceive that current psychological theories of behavior were almost wholly concerned with actions of relatively short duration, reflexes and consecutive instrumental acts which reach their terminus within one experimental session, rather than with long-range enterprises which take weeks, months, or years of effort to complete. Here, it seemed, was one of the most striking differences between men and animals, namely, the capacity for time-binding (Korzybski) or the span of time-perspective (Frank, Lewin). The behavior of animals can be explained so largely by reference to attractive or repellent presentations in their immediate environment and/or to momentarily urgent and rather quickly reducible states of tension; whereas a great deal of a man's behavior cannot be explained except by reference to persistent "self-stimulation" in accordance with a plan of action, which often involves the subject's commitment to a distal goal or set of goals, as well as to a more or less flexible (or rigid) temporal order (schedule) or subsidiary, or stage, goals. Observing his behavior over several months or years, we see, not only the recurrence of a large number of patterns devoted to the repetition of valued experiences and the prevention of disvalued experiences, patterns with homeostatic effects, but a number of interrupted successions of proceedings (which I am calling *serials*, or long enterprises), each temporal segment of which is progressively related to the last (carrying on from where the other stopped), though separated from it by an interval of time (commonly a day). A successful *serial* is different from many day-by-day reactions in so far as its effects are *transtatic* rather than homeostatic, that is to say, it transforms or transcends the existing steady state by carrying a person from one level or form of equilibrium—dispositional, material, ideational, or social—to another: a new interpersonal relationship (an additional commitment) becomes established; a new house is purchased and furnished (which must hereafter be kept up); knowledge is gradually assimilated, and a new orientation (directing one's efforts toward another target) is

acquired; the subject graduates from college, gets a job, and takes on the responsibilities of a new office; a novel is written and published, and so forth. Progressive enterprises of this sort constitute the bulk of a healthy young adult's endeavors in a "civilized" society.

Ordination. It took me years to realize that the psychology of the higher mental processes had been equivalent, in the minds of most psychologists, to the psychology of cognition, and that the psychology of cognition was largely concerned with the processes whereby a person acquires objective knowledge and understanding of his physical environment—the very processes and the very aims which are dominant in us psychologists—and that *i*, the more fundamental and important knowledge of the satisfying and dissatisfying, the beneficial and the harmful properties of the environment and of the self's capacity to cope with them, and *ii*, the still "higher" mental processes involved in the construction of a plan of action, were pretty generally neglected. What should we call the persistent, self-critical, conceptual, and often logical mental processes that continue over several months in the mind of a psychologist until they terminate with the construction of an integrated design for his next experiment? These processes commonly take off from perceptions and explanations of previous experiments and results; but their immediate aim is not so much to conceptualize already observed events (cognition), but to imagine something unobserved—new conditions and new experimental operations—and, by logic or intuition, to predict the outcome. During his months of planning the scientist (or anyone else for that matter) is more frequently thinking, one might say, on the efferent, rather than on the afferent, side of the cortical arc, and some psychologists might, therefore, be disposed to subsume his mental processes (processes which sometimes occur very rapidly—within a few seconds) under conation, on the grounds that their function is to orient and coordinate action. But against this is the fact that they are often very "intellectual" (higher mental processes in the strictest sense), engaged in a most difficult endeavor (since rational prediction is usually harder than rational explanation), and superordinate to other processes, in the sense that the goal and strategy which is ultimately selected will determine behavior for a good many months to come.

For better or for worse I have been calling such mental processes—processes concerned with the selection and integration of plans of action—*ordination*. The preliminary processes of the imagination—fantasies and trial experiments in the mind—I am calling *prospections*. Here, instead of entertaining recollections (replicative imaginations of past events), the subject is concerned with the future, prospectively picturing himself in this and that situation, seeking this or that opportunity for gratification or for the advancement of his ambitions. Here creativity

may operate to a marked degree. The selection from numerous alternatives of a concrete and specific goal, purpose, or aim to appease one or more needful dispositions, I am calling *orientation*. It is the subsequent phase—the selection and temporal articulation of ways-means, strategies, or tactics (represented by images or words)—that I am calling *ordination*. I have found that the word can be used without confusion, both for the process of constructing (ordinating) a plan and for the construction (ordination) that results from this process. An ordination may have a very short or very long time span; it may be vague and global or clearly differentiated into discrete behavioral units; it may be disjunctive or conjunctive (temporally integrated in a logical manner); it may stand at any point along the rigidity-flexibility continuum; and it may have more or less of the property (power) of “imperativeness” (indicated, partly, by shame or guilt if adherence to the ordination is imperfect); and so forth. One significance of this concept is its discrimination of a major antecedent determinant of behavior in a “civilized” society, namely, a fixed schedule, the time set for a certain kind of activity, a prearranged appointment, a prescribed order of procedure—quite regardless of the mood, dispositional state, need, or what not, existing at the moment. A good part of socialization consists in acquiring the capacity to keep promises, and hence, to do something which, *at the appointed time*, you are not inclined to do. Furthermore, we need a concept of *prospective time* reaching into an imagined future, some of which is *filled* (committed, planned) and some of which is still *unfilled* (open, available for use).

INFLUENCE OF FREUD, JUNG, AND OTHER PSYCHOANALYSTS

I came to psychology via Jung’s *Psychological Types* and his *Psychology of the Unconscious*, the first of which initiated my professional interest in types of human nature, and the second, my interest in unconscious processes as revealed by mythologies and religious imagery as well as in the more central and integral transformations of personality. What I gained from Freud was somewhat more specific and more applicable in practice and, in due course, became so much a part of my regular and irregular modes of thought that there have been times when I forgot my debt and took his huge gift for granted. In the late twenties and early thirties when Freud’s name and works were anathema to the majority of academic psychologists, I was a staunch advocate and defender—as I am now—of his greatest contributions: (1) evidences of the theory of unconscious psychic processes and their effects, (2) evidences of the determining importance of early family relations and of the experiences of childhood, of the persistence of complexes established

in those years, (3) countless illustrations of the multifarious manifestations of the sex drive, (4) division of the personality into id, ego, and superego (conventional constructs), (5) definition of several mechanisms—repression, isolation, denial, etc.—that operate in the service of adjustment, of self-esteem, and of serenity of consciousness, and a host of other more restricted constructs and theories illustrated by abundant case material.

I was one of the founding members of the Boston Psychoanalytic Society and throughout the thirties was so closely identified with its cause that President Conant decided, primarily on these grounds, that I was not qualified for tenure. Similarly, in the opinion of the psychologists who reviewed it, *Explorations in Personality* [3] was a treatise out of Freud, or, more accurately, an attempted adaptation of psychoanalytic theory to academic standards. In short, what I have seized from Freud is so very obvious that it should not be necessary for me, at this late date, to lay it on the line.

The present situation is entirely different: Freud has conquered. He has captured a large portion of the Western mind, his revolutionary theories are learnedly and respectfully discussed in General Education courses, he is now an indispensable fixture in the domain of psychology, and so venerated by his professional disciples that his most casual comments are repeated ritualistically as absolutes. Clearly his position is assured and what we all owe to him is plain. The danger now is precisely the opposite of what it was in the twenties when it looked as if professors were built to shut their minds to him. Caught up as we are today in a great wave of Freudolatry we are inclined to take it all as gospel, to feel that the greater part of what the Master said is so astute that the gestalt which he created should not be spoilt by calling attention to a few trivial defects. This attitude would have been impossible to Freud himself and if continued its only consequence can be sclerosis of the mind and rigor mortis.

As I weigh it, Freud's contribution to man's conceptualized knowledge of himself is the greatest since the works of Aristotle; but that his view of human nature is exceptionally—perhaps projectively and inevitably—one-sided, an extraordinary abstraction from the abundant facts of life, facts which may have little bearing on the etiology of neurotic symptoms but great relevance to other issues. My chief objection is the commonplace that in his system, the libido has digested all the needs contributing to self-preservation, self-regard, and self-advancement, together with a host of others, and rebaptized them in the name of Sex; and that sex itself is never given either its profound evolutionary status or its interpersonally creative status. In the last analysis, it is reduced to transient, superficial, localized sensations. But then, who

at this preliminary stage of knowledge can cover everything and be right?

INFLUENCE OF DARWIN, BERGSON, AND OTHER EVOLUTIONISTS: ADOPTION OF THE CONCEPT OF CREATIVITY

My Heraclitean concern with process, change, and transformation, dating from incubator years at the Rockefeller Institute, did not gain the impetus of a possession until, with Lucretius vastly and vaguely in the background, I came upon Bergson's theory of creative evolution, Lloyd Morgan's concept of emergence, Whitehead's philosophy of organism, Leibniz's monad, and the speculations of L. L. Whyte, Oparin, Wald, and others, respecting biochemical evolutions. What I abstracted from these authors, in conjunction with a few miscellaneous influxions from the "unconscious," brought me to the conclusion that creativity—the formation of new and consequential entities and of new and consequential patterns of activity—is a centrally determining capacity of nature, more especially of human nature. I had observed the progress of morphological maturations in the embryo and later, the establishment of new ordinations of serial enterprises and of new tactical patterns and skills in personalities; but not until I paid attention to analogous proceedings on the physicochemical, sex-genetical, societal, and symbolic-representational levels and in the sphere of technology, did I arrive at a general conception of formative, or constructive, processes operating throughout nature.

What does this amount to? First, a comprehensive generalization respecting a widely distributed capacity of entities, namely, under favorable conditions to associate and remain associated, to combine and remain combined, to become involved in the creation of new entities with previously unexampled properties, and thereby to participate in the making of an irreversible route of events. Finding manifestations of such formative capacities at all integrative levels, we become more assured of their importance, more convinced that they deserve a place in our catalogue of fundamental dispositions. Also, we are invited by the possibility that detailed investigations of new productions at one level may suggest analogies, correspondences, and hypotheses to be tested at another. Second, the observation that matter has formative capacities makes us realize that creativity is *immanent* in nature, not the prerogative of some transcendent craftsman, such as Plato's Demiurge or the Yahweh of Genesis, nor imposed on nature by the will of man. On the one hand, it permits a natural explanation of some of the phenomena on which the doctrine of vitalism once built its case, and, on the other hand, it shows us why the term "mechanism" (with its implicit reference

to a man-made machine as model) was not the happiest choice to characterize the procession of open systems under natural conditions. Our conviction that the old vitalist-mechanist opposition is a dead issue is supported, I believe, by the abandonment of classical mechanics by physicists and chemists as basis for their theoretical inventions. Third, the addition of the formation (creation, construction, reconstruction) process and effect to our inventory of dispositional properties of personality provides us with the otherwise missing necessary factor not only for an adequate conception of the liveliest course of mental processes through time (the work of the imagination), but for the systematic representation of the functional interdependence of other members of the inventory during the growing, expanding, and developing phases of a person's life.

The concept of survival in one or another guise—self-preservation, continuation, maintenance, homeostasis, and so forth—can fulfill the same construct-integrating function in a theoretical system that is designed to apply to mature lower organisms, since the great majority of their activities may be partly understood historically, in terms of their generally beneficent contributions to the continuation or the restoration of a steady state. But the principle of survival is applicable only to the status quo, not to mutation resulting in ontogenetic and phylogenetic emergent evolutions. In my view of affairs, anyhow, it is necessary to put the processes of composition and decomposition at the center of things, between the terminus of the afferent side and the initiation of the efferent side of the energy conversion arc of personality.

But this is not the accepted view today—despite our great concern with learning, with developments of personality, and, very recently, with some forms of mental creativity. The Freudian inventory of drives, for example, includes sex, aggression (destruction), and anxiety-avoidance, but *not* construction. Construction—which, being exemplified on the chemical level, is more fundamental, in my view, than any of these instincts as operationally defined by psychoanalysts—is subsumed, in a vague and general way, under the concept of sublimation of infantile eroticisms. Similarly in other special fields—sociology as well as experimental psychology. It may be a matter of time-perspective. If we are in the habit of performing short experiments with a peripheral subsystem of personality, no products of formative energies may strike our apprehensive mass; but if we take a longer view we are struck by nothing else.

Let us assume a comfortable position on Ganymede, satellite of Jupiter, about two billion years ago and with supernatural eyes take a morning look at the surface of this planet. We shall perspect, according to those who are entitled to a guess, nothing save a fairly hot solution

of inorganic salts keeping company with the simplest carbon compounds and enveloping this broth an atmosphere of gases from which oxygen is absent. In the evening let us take another look. Since we have temporarily assumed the power of a celestial being, a thousand ages in our sight is as a day gone by and we shall now be in the twentieth century gazing, I should hope with wonder, at a tremendous miscellany of natural productions—500,000 kinds of organic compounds, over 250,000 species of plants, over 1,000,000 species of animals already identified by man. We shall perceive numberless societal formations: human beings almost everywhere, behaving rather regularly as members of a family, clan, tribe, state, or nation, small or great, with fairly consistent governments, laws, and policies. More obvious will be the territorial and habitational constructions: land masses studded with settlements, villages, towns, and cities, surrounded by cultivated fields and connected by paths, roads, boulevards, and iron rails, running through tunnels and over bridges. How long would be a catalogue of man's material manufactures, architectural, mechanical, electrical! Think of the palaces and temples, tools and armaments, machines and dynamos, waterworks, heating systems, lighting systems, automobiles and airplanes, and gadgets by the millions. Enough said. In the name of brevity, let's skip the rest and consider the manifold combinations of sounds—the songs and symphonies—and the combinations of images and imagined episodes—the mythologies and dramas, sonnets and heroic epics, histories and novels, and their representations in paint, wood, and marble—and the combination of concepts and reflections—the ethical philosophies, mathematical formulations, and scientific systems—which engage the minds of men, and with these let's end our swift survey of entities and activities on the earth's skin. All these things, all varieties of social governments, material conveyances and utilities, symbolisms and ideas, are productions of the human part of nature, and in all probability, the vast majority of them had their genesis in the imaginations of a single individual or of a cluster of individuals.

And yet, the word "imagination" has been absent from the index of most textbooks of psychology, and one has to search diligently to find a little reference here and there to planning processes (prospection and ordination), and despite the emergent interest in creativity, only a few authors have seen fit to include, in some indefinite guise or other, a formative disposition—habitational, implemental, interpersonal, social, or symbolic—among the properties of human personality.

Darwin was primarily concerned with the occurrence of successively more effective variations of mature morphologies from generation to generation. In his day, biochemical science was not so far advanced as to assist him with suggestions of plausible hypotheses respecting the

determination of these gross changes. Knowing nothing of the role of chromosomes and genes, of nucleoproteins and DNA, it could hardly be realized that chemistry is the instrument of heredity. Today, however, we can reasonably postulate the creation of new genes along the route of evolution, the mutation (by the transposition of a single atom within a molecule) of a gene, and a stupendous variety of possible combinations of genetical clusters from male and female. The chemists of Darwin's time were not prepared to cope with the problem of the emergence of living entities from nonliving entities, the virus was unknown; and the physicists were speculating about matters other than the possibility of the evolution of increasingly complex chemical elements and compounds, say, out of light atomic nuclei. No one had yet suggested that as the universe expanded new matter was constantly coming into being. In psychology, prevalent interests and conceptions were far from the idea that formative (gestalt-making) processes were involved in perception and apperception, not to speak of their engagement in the psychologist's own business of making concepts and formulating propositions. In short, the data necessary for a systematic representation of constructive processes on different levels of integration were not available in the nineteenth century. Today, however, a multiplicity of facts and of reflections are at hand, enough, it seems to me, for a rough preliminary draft of meaningful analogies.

The very briefest outline I can devise, omitting several important vectors and all details, includes the *movement* (motility, exploration), and hence, by *chance*, the inevitable *contiguity* of different entities, one or each of which is inherently attractive to the other—*attraction* (gravitation, valence, cathexis) being one of the ever-present forces of the universe—and, consequently, either symmetrical or asymmetrical *accession* (approach) resulting in an *association* or structural *formation* (creation, construction, synthesis, conjugation, or incorporation of a smaller by a larger entity) new to this planet, and the *cohesion*, the sticking and staying power, and hence the relative stability and longevity of this unprecedented form of whatever category—organic compound, genetical configuration, family relationship, tribal federation, governmental law, religious belief, creed, or rite. If the established form is to have further evolutionary value it must have the attribute of *plasticity*, or flexibility, the capacity, that is, to play a part or to become involved in subsequent *transformations* or reconstructions. The picture is one of continuity through change. Only by losing its particular identity, by perishing as such, can a variation become a link, stage, or episode, in an evolutionary sequence, such as the one and only sequence that led to the human species.

Some of my more earnest and literal-minded friends remind me that a

psychologist should abjure fantasies of temporal omniscience and keep off of Ganymede. Formative processes lie outside the sphere of psychology: they occur in the "depths," behind the scenes, take a long time to get worked out, and are wholly unpredictable. A psychologist should attend to the precise particulars of today's circumscribed field of observation. Agreed, but suppose I ask one of these friendly critics to serve as a subject and request him at the first session to demonstrate his ability to design an experiment which will confirm or unconfirm a hypothesis that is unfamiliar to him. In the second session he might be asked to invent two different parables to illustrate the evil effects of fanaticism, and in the third, to outline a course of action that might happily settle a specifically defined dissension among four members of an academic group. If, in each case, my friend gives voice to the thoughts that successively come to mind, the chances are that we shall apperceive the components of a constructive process operating before our ears from the beginning to the end of the experiment—influxions of ideas from the "well" of mind (What are they? How fast do they come? How varied are they? How definite? How appropriate to the given task?), interspersed with evaluations of these influxions, the rejection of some and the acceptance of others (How much consideration is given to each idea? How exacting is the standard of assessment? How excellent are the judgments in the opinion of experts? How much inhibition, hesitation, censorship, self-criticism occurs along the route? How quick are the acceptances? How decisive?), and then, to make a long story shorter, the temporal allocations, or ordinations, of the accepted components of the design, the parable, or the plan (Are the concatenations actually logical? Clearly expressed? Have all probable contingencies been met? Has anything essential been omitted? How superficial or profound is the offered solution or composition, and so forth). In every such experiment I submit, we shall obtain a unique mental composition which, at one extreme and perhaps in the majority of cases, may be socially worthless in the estimation of qualified judges and advisedly forgotten, but at the other, might be a rare gem of creativity, something memorable that may eventually find a place in the great body of cultural transmissions. We may, for instance, be dealing with a Whitehead equal to such utterances as these:

Insistence on clarity at all costs is based on sheer superstition as to the mode in which human intelligence functions.

No science can be more secure than the unconscious metaphysics which it tacitly presupposes.

Murder is a prerequisite for the absorption of biology into physics as expressed in (its) traditional concepts.

A self-satisfied rationalism is in effect a form of anti-rationalism. It means an arbitrary halt at a particular set of abstractions.

A science which hesitates to forget its founders is lost.

Scientists animated by the purpose of proving themselves purposeless constitute an interesting subject for study.

Of course, creativity—the real thing—is an autonomous and capricious process which rarely shows itself when called upon; hence, impromptu tests are not likely to bring forth anything but rather shallow forms of originality and inventiveness. Nevertheless, to my way of thinking, there are compositional processes at work, ordering ideas and shaping sentences—sometimes brilliantly—in the course of every communication. Most of us, to be sure, make use of the same worn words and trite phrases time and time again, and integrativeness in speech or writing is limited to the joining of one commonplace to the next; but were we to abide by the current laws of learning and in talks with friend or spouse repeat tomorrow the response—the bit of news, the joke, the idea—that was reinforced today, we would be heading for press rejection or divorce. What we have to learn is to break a specific speech-reward connection and on a subsequent occasion substitute some variation. In short we will be rewarded only for saying something different from, but as stimulating as, that for which we were rewarded last. Conclusion: a gust for novelty and emergent forms is widely distributed among members of our breed.

For the present, we may define participant creative processes in terms of their effect, result, achievement, namely, an unprecedented form, and confine our attention to stable forms which are retrospectively apperceived as valuable and as having further consequences in an evolutionary context. Striking to many of us is the blindness of these processes, their experimental character, and their resistance to the coercions of conscious purpose, which is something that is worth considering in connection with human imaginations, and the occurrence in some people of a strong disposition to create: to combine sounds, images, words, concepts, propositions, ideas, ordinances, people, things, strategies, or techniques in new and significant forms which express something that is worth expressing, order things that are worth ordering, build something that is worth building, or solve a problem that is worth solving. Mobilized by a need of any other class than this, a human subject is likely to have a picture in his mind's eye of what he wants—water, sexual intercourse, a habitation, an automobile, world news, membership in a certain group, promotion, prestige, or what not. Under most circumstances, what he wants already exists somewhere, actually or potentially, in the environment, and he must take it pretty much as it is or as it

comes. There is food in that restaurant, information he requires in that book, a person over there whose friendship he might win, a job to be had and money to be earned, et cetera, et cetera. But the aim of creativity—say, a design for a more efficient machine, an architectural innovation, a symbolic plot for a drama to be written, the explanation of an enigmatic phenomenon, a more enlightened foreign policy—has no existence anywhere. A person with this need must work, think, brood, daydream, rest, sleep, turn his thoughts to other things—perhaps drink and read detective stories—until his mind will favor him with a representation which possesses, in his prospecting eye, the attributes that he seeks, and then he must be favored further by representations of suitable embodiments. A man may rack his brains throughout a lifetime without receiving the vision or idea for which he longs, or if the idea has come to him, he may labor for years without finding the way to expound it in a persuasive manner or to implement it in an actional endeavor. That is to say, we are dealing here with energies of the human mind that do not respond directly to voluntary efforts. Voluntary efforts can influence their direction, defining, so far as possible, the target of their endeavor, but they cannot force them to render up the desired form or answer.

Nowadays it is pretty generally agreed, I would suppose, that imaginations of any real consequence are generated outside, or “below,” the stream of awareness, after a more or less prolonged period of incubation, and they are apt to leap to consciousness abruptly at the most unexpected moments. Sometimes, like a dream, they seem to come from without rather than from within the mind. A vision has been called a vision because it is a visual presentation, a present, a gift, to the inner eye, just as the heavenly constellations at night are a presentation, or gift, to the outer eye. It was partly on this ground, we may surmise, that the ancients believed that visions of import came from the gods, as best among their blessings to deserving men. Today we are disposed to say that they come from the unconscious. But the proposition I am submitting here is that the witting purpose to create something with certain valued properties is almost wholly blind, its goal being to conceive a goal; and though voluntary effort is one determinant of success, the processes on which creativity depends proceed, for the most part, spontaneously and autonomously outside of consciousness and give rise to hundreds of influxions which do not survive because consciousness rejects them, and if a certain influxion is considered worthy of survival it may not be what consciousness was seeking, but something else entirely.

Facts of this order constitute the basis for the not uncommon experience among creative men of serving as a vehicle or mouthpiece of some supernatural or superpersonal imperative, of being an agent of evolution instead of a feverish egoistic little self. “This is the true joy

in life," Bernard Shaw has written, "the being used for a purpose recognized by yourself as a mighty one."

INFLUENCE OF SOCIAL EVOLUTIONISTS, CULTURAL ANTHROPOLOGISTS, AND SOCIOLOGISTS

As one of the charter members of the Department of Social Relations at Harvard, I could hardly fail to be inspired and directed in my thinking by our largely shared ambitious aim to advance by successive trials toward a common theoretical system for basic social science. If it had not been for this association, for the continuous influence of such learned and persuasive colleagues as Clyde Kluckhohn and Talcott Parsons, I might still be representing personalities in so near a social vacuum as we did in *Explorations*. As a biologist I had been attached to the concept of the herd instinct, as elaborated, say, by Trotter, and as a psychoanalyst, to the concept of identification in each of its different meanings, as well as to the several propositions respecting the internalization of the parental superego. Nothing is more apparent as we look at others and ourselves, especially in the United States—despite or because of our loudly avowed ideology of freedom and individuality—than the tremendous prevalence of unconscious imitation and conformity, of the educating and constraining force of public opinion and behavior. But I did not become aware of the numerous cultural differentiations one had to make, differentiations of socioeconomic classes, of special subgroups, of rank in the decision-making hierarchy, of role and function, until I gave a joint seminar with the encyclopedic Kluckhohn, who consented to the office of tutor in these matters. There I once again experienced the truth of the old adage: the best way to learn a subject is to teach it, in this case in conjunction with an expert. Besides my indebtedness to the elaborate classifications and generalizations of Talcott Parsons, I should mention among other respected instructors in the social sciences: Pareto as expounded by L. J. Henderson in a memorable seminar, Malinowski, Sapir, Margaret Mead, Ralph Linton, John Dollard, Florence Kluckhohn, Edward Shils, Robert Merton, Harold Laswell, Ernest Cassirer—the list is long; many congenial influences have necessarily been omitted.

Since the anthropological and sociological concepts that I employ are pretty nearly all derivative, I need not say much on this score. Here again I have been influenced by Darwin, specifically by the theory that the group more than the individual has been the evolutionary unit. Being of this persuasion, I have come to think that no theoretical system constructed on the psychological level will be adequate until it has been embraced by and intermeshed with a cultural-sociological system. Al-

though every individual has some measure of inner life, a host of private and largely secret feelings, fantasies, beliefs, and aspirations, and has some extent of free play outside the coercions and restraints of the social system, the great bulk of his overt behaviors are regulated by the memberships and roles to which he is committed, his actual behavior being the resultant of a fusion or compromise between cultural specifications and standards and his own dispositions and abilities. Such is the conventional doctrine of our time, in one guise or another, and I have little to add to it. But, since the group theory of evolution is rarely mentioned today and since, for better or for worse, it has strongly influenced my speculations, I am yielding to the temptation of quoting a few paragraphs from a recent attempt I made to expound it in a condensed form.

Surveying the evidences of man's development on earth, the later Darwin concluded: first, that the survival of the fittest is a principle which applies decisively not so much to individuals as to rival groups—tribes, states, or nations—and second, that mutual sympathy, aid, and collaboration among members of a group are conducive to its solidarity, and hence to its combative power and survival. To put it another way, one of the critical variations established long ago was a clannish combination of families more powerful than any single person, a flexible yet stable social system with some differentiation of functions and consequently with an enhanced capacity to cope with various tasks and crises.

From the beginning, if we follow Sir Arthur Keith's composition of the evidence, every successful group has adhered to a double code of conduct, a Janus-faced morality: one face preaching submission to authority, reverence, cooperation, loyalty, good will, and generosity within the group, and the other more contorted face shouting with rage and murderous aggression toward members of opposing groups. Other things being equal, it must have been the clans or tribes which embodied this dual standard in the best balance that triumphed and endured, and passed on to their descendants down to the present day the dispositions which sustained it.

This theory of group evolution helps us to understand why man is a social, rather a solitary, self-sufficient creature and why, as a social creature, he is both humane and brutal. Illustrative of his *social* properties are such familiar facts as these: that the vast majority of men are reared in one particular society, a society that is prejudiced in its own favor, and are satisfied to be lifelong interdependent members of this society, that the bulk of their enjoyments come from interacting with its members, that they are at peace with themselves only when they

feel and act in accord with its customs and ideals, and that, even in their furthest reaches of self-forwarding ambition, they choose for their most delectable final prize the applause of their fellow-beings, and after death, fame, "that last infirmity of noble mind." The dual morality of groups—tribes and nations—accounts, in some measure, for the failure, the halfheartedness and insincerity, of all attempts to abolish war and for the fact that human beings have been generally so willing, even eager, to suppress their fears of self-extinction and fight for their country to the tragic end, as well as for the fact that a man who kills a hundred members of an enemy society is declared glorious, but is condemned to the severest punishment if he stops the life of a single fellow citizen.

It is supposed that the generally victorious groups were those which most fully incorporated and exploited the vaingloriousness and pride, the greed and will to power of their individual members. But what is the significance of the will to power? Power, intoxicating as it may be to some men and to some nations, is a means to something, not an end. Power for what? To this question the response of a creative evolutionist might be: power to construct ever larger and less vulnerable social systems controlling ever larger areas of the earth's resources, or in other words, power, spurred on by greed, to grow and to develop, by invading, conquering, subjugating, and assimilating weaker units, or more peacefully and happily in some cases, by federating with other units. History reports a great number of such sequences: the integration of primal groups into clans, and of clans into tribes, and of tribes into small nations, and the integration of small nations into great nations that subsequently broke apart, the rise and decline, the evolution and involution, of mighty civilizations, as Toynbee has shown us, but as yet no orchestration of state sovereignties into a world order, no political embodiment of that dream of universal fellowship which centuries of idealistic men have recommended to our hearts.

In short, everything I have said relative to formations, transformations, malformations, and deteriorations on the psychological level is applicable in a general, though not specific, way to the level of group dynamics.

COMPELLING NEED FOR COMPREHENSIVENESS

Although I was educated on the principle that limitation of aim is the secret of success in science, and that the scientist is responsible for particulars, it must be only too apparent to you that I have been tempted to depart from the wisdom of this strategy by the dream of an all-embracing scheme, a unified science, not, of course, to be achieved in my

own lifetime but in the distant future, if there is to be a future for our species.

I suppose it would be proper to speak of:

1. A comprehensive concept (such as energy, process, matter, form, motion) which refers to something that is always and everywhere observable or inferable.

2. A comprehensive conceptual scheme (such as the periodic table, classifications of botanical and zoological forms) which differentiates relationally all entities and all attributes and properties of entities within the domain of a single discipline.

3. A comprehensive formulation, theory, or law (such as $e = mc^2$, the laws of thermodynamics, the theory of evolution) which is applicable over a wide range of phenomena.

4. A comprehensive spatial scope of individual concern within a single discipline, such as (to limit consideration to the biological and social sciences) that of a physiologist who takes the total organism as his province (rather than specializing in kidney function), that of a psychologist who takes the whole personality (rather than specializing in cognition), or that of a sociologist who takes the total community (rather than specializing in family structure). Scope of data.

5. A comprehensive temporal span of individual concern within a single discipline, such as that of a biologist who is interested in genetics and heredity, that of a psychologist who is occupied with parental as well as subsequent determinants of personality, or that of a sociologist or anthropologist who studies historic transformations. Span of data.

Now, one of the best appraisers of the status of psychological theory in this country, the wisely chosen Director of this project, stated not so long ago that the development of our science had been more retarded in recent years by straining after comprehensiveness than by any other variety of ambition. But since it is not clear to me which of the above forms of comprehensiveness he had in mind, I have not yet had to square my shoulders to the verdict *guilty*. There is at least one form of comprehensiveness for which I have not reached, the comprehensiveness of a neat net of postulates and theorems that is expected to catch every kind of fish that swims in the stream of human experience and behavior. I have never been so optimistic as to think that we psychologists were anywhere near the day when some master mind might achieve so much. Instead I have been a perpetual catcher and collector of facts and figures, a perpetual classifier of concepts, and a promoter, in a little way, of marriages of concepts, believing that these pedestrian occupations were appropriate to the stage of conceptual evolution at which psychology has arrived. Here I am not speaking for the psychobiologists who study the ways-means learning processes of imprisoned animals. They,

so far as I can see, have already arrived at that state of knowledge and mastery of their variables from which lawmaking for their territory makes sense.

The forms of comprehensiveness of which I am most surely guilty are comprehensiveness of territory in space and time. I have spoken of my interest in creative evolution down the ages and in developments of personalities from birth on (temporal comprehensiveness); and I have indicated how I was forced, to put it bluntly, by my colleagues at Harvard to become socio-spatially comprehensive, concerned with the supraorganism of which every personality is imperatively a functioning component. Nor can other groups, out-groups and foreign nations, be excluded from the picture, it being all too evident these days that a little shooting incident on some distant surface of our planet might initiate a global conflict which would change the roles, the activities, and the effects of millions of human beings. Belief in the imminence of a catastrophic war is currently one of the determinants of anxiety in a large number of people occupying statuses of responsibility. And then, beyond the earth and its contentious nationalities, revolve the sun, the moon, the planets, stars, and Milky Way, all of which have influenced the minds of countless individuals and collectivities, not as the Chaldean astrologers surmised, but by drawing aspirations and cognitions upward, by engendering images and stories of celestial divinities and powers, of resurrections and ascensions to a heavenly paradise beyond the grave, and of life everlasting in a society of musical winged beings, not to speak of the attraction by cosmic bodies of astronomers and poets.

TOLERANCE OF UNCERTAINTY

From what I have confessed so far it must seem as if the need for certainty, powerful in most scientists, is very weak in me. But, as I weigh them, my hopes and expectations in this regard are no higher and no lower than they legitimately can be nowadays in the sphere of endeavor to which I am committed. Were my demands greater, I either would be perpetually defeated or, to escape from this, would be impelled to quit personology and return to chemistry for peace. I take heart from Aristotle: "It is the mark of an educated man to look for precision in each class of things. . . ."

But this is not the whole story. There is something more in me which is not irrelevant to this issue: the induction from experience that a compulsive need for intellectual certainty—abetted, I would suppose, by longings for personal security—is very apt to lead to deadly falsifications and distortions of reality. Leaving aside the changeless eternal forms and absolutes of philosophers and theologians, and confining ourselves to

scientists, we can find innumerable examples of the operations of this need: the selection of the most fixed, permanent, or recurrent things to study, the unnatural stabilization of the experimental environment, the prevention of all but two or three possibilities of response, the circumscription of the area of observation to a small part of the total field of influential forces, et cetera, et cetera. Such choices and constraints are valid parts of the strategy of science and not criticizable as such. They are to be criticized only when the results obtained in highly focused studies of this sort are generalized across the board and the notion propagated that the entities with which we are concerned are far more structured, rigid, stable, orderly, consistent, and predictable than they really are.

In my philosophy there are no absolute or inevitable laws, no enduring certainties: every observation, every inference, every explanation, and every prediction is a matter of less or greater probability. To this most psychologists, I trust, would be ready to assent.

INTEREST IN SYSTEMS

My interest in systems was confined at first to shifts of equilibria, as a function of oxygen tension, among the electrolytes of blood within the walls of a glass vessel. The scope of the next system I studied with some care was a volume bounded by an eggshell, closed to material substances but open to intakes and outputs of gases. Here my chief source of illumination was *Elements of Physical Biology* by Lotka [2]. But the relevance of these investigations and formulations to psychology was not apparent to me until the thirties when I was introduced to Pareto's representation of society as a system, and somewhat later to the conceptualizations of the Chicago group as set forth, say, in *Levels of Integration in Biological and Social Systems*, edited by Robert Redfield [5]. Ever since, encouraged by Whitehead's speculations, I have been addicted to the perilous practice of discovering analogies among events at different levels. This hobby, once private and covert, has become more articulate of late, partly owing to parallels discovered in the writings of L. von Bertalanffy, A. E. Emerson, R. W. Gerard, and other men who are concerned with correspondences and differences between various kinds of systems—what is now known as General System Theory.

I am wary of the word "system," because social scientists use it very frequently without specifying which of several possible different denotations they have in mind; but more particularly because, today, "system" is a highly cathected term, loaded with prestige; hence, we are all strongly tempted to employ it even when we have nothing definite in mind and its only service is to indicate that we subscribe to the general

premise respecting the interdependence of things—basic to organismic theory, holism, field theory, interactionism, transactionism, etc. For example, the terms “personality-as-a-whole” and “personality system” have been very popular in recent years; but no writer, so far as I know, has explicitly defined the components of a “whole personality” or of a “system of personality.” When definitions of the units of a system are lacking, the term stands for no more than an article of faith, and is misleading to boot, in so far as it suggests a condition of affairs that may not actually exist. It suggests not only that one is dealing with a set of recurrent, orderly, lawful interactions, but that the number, constitution, position, and effects of the interacting units remain relatively constant. That is, it is usually taken for granted that “system” refers to a homeostatic, boundary-maintaining system. Finally, overtones convey the impression that the speaker has a steady, coherent theoretical system in his head which conforms to the steady coherent system he is studying. Hence I am wary of the word. But, having found that I cannot get along without it, I must do my best, when the time comes, to define my restricted usages of this term.

I might say, in a general way, that, for me, system applies to a more or less uniform integration of *reciprocating* and/or *cooperating* functional activities, each of which, under favorable conditions, contributes to the continuation of the entire cycle of activities which constitute the system. As a rule, such a system is boundary-maintaining. According to this view, each entity (form of matter) involved in a cooperating system may be called an organ, relative to that system, each organ being defined in terms of process and its contributing effect, or since organ processes are not always capable of achieving a contributing effect, in terms of their direction, *endeavor*, or intended effect. Thus, each unified, boundary-maintaining system may be partially defined by representing the integration of successive processes and effects which are required to keep it growing and/or to keep it going as a unique and vital whole. The major unitary functional systems with which a social scientist is concerned are these: personality systems, dyadic social systems, polyadic social systems, representational (symbolic) systems, each of which may be divided—according to different spheres of concern—into large subsystems. For example, a personality system may be divided into:

1. A psychosomatic system, consisting of all needs and activities concerned with the growth and welfare of the body: procurement and incorporation of water and food, transposition and allocation of food particles, differential construction of frame and organs, excorporation of water and waste, actuation and integration of muscular patterns, development of manual and athletic skills, defense of the integrity of the body, etc.

2. A psycho-material system, consisting of all needs and activities concerned with the acquisition, restoration, or construction of a territory and/or of a habitation (stead and shell), as well as with the acquisition, restoration, or construction of implements or machines, utilization of these implements, development of technical skills, defense of property, etc.

3. A psychosexual system, consisting of all needs and activities concerned with erotic love: stimulations and interactions, the formation and continuation of an erotic dyad, conjugations, and the conception of offspring, etc.

4. A psychosocial system, consisting of all needs and activities concerned with nonerotic social reciprocations: transmissions and receptions of affection, of food, money, and material entities, of information and evaluations, of orientations and ordinations, directions and compliances, development of social skills, etc.

5. A psycho-representational system, consisting of all mental (cognitive and ordinative) needs and activities associated with the above-mentioned systems—acquisition of knowledge, explanations, and postulations—as well as mental needs and activities concerned with impersonal symbolic systems (explicit culture), with law, art, science, morals, ideology, and religion, development of mental skills.

The personality system, as such, is concerned with the allocation of time and energy among these different subsystems and sub-subsystems, the ordination of their component serial endeavors, the repression of unacceptable emotions and impulses, and the reduction of conflicts and strain.

A dyadic system consists of the interplay of two personality systems, each of which is given equivalent attention. This is enough to indicate, very roughly, the way the term “system” is applied in the scaffold as now constituted.

APOLOGIA

When, after finishing part 1 of this assignment—my autobiography of somewhat relevant cerebrations—I got round to a closer examination of the scheme provided us, I discovered that it was even more exacting than I had initially believed. It was definitely beyond my reach, beyond the reach, I judged, of anyone who is primarily concerned, at this stage of things, with the formulation of different types of personalities as manifested, say, by different classes of reactions to a variety of similar situations, rather than with the reactions of most people, say, to modifications of one particular experimental situation.

I might have profited by the moral of the Icarian thema, as repre-

sented in the careers of several young persons assessed at the Baleen, annex of the Harvard Psychological Clinic. Its moral is that of the inevitable fall of over-reaching aspiration, the nemesis of *hubris*, so familiar to the Greeks. But the prospect of this outcome did not bring about a reasonable abandonment of the project. It merely served to check me to the point of regarding the committee's standard as an unrealizable ideal, but yet something to be held in view while I labored over the development of the scaffold. As it turned out, the effect of this ideal was an almost continuous procession of very general as well as of very particular conceptual compositions, decompositions, and recompositions, which kept informing me of the intricate influence of more and more variables in the determination of the course and outcome of almost every unit of behavior that could interest a personologist. Thus, I was led on from complication to complication, and though many were resolvable, the resolutions served only to increase the number of aspects to be considered and of discriminations to be made in analyzing, explaining, or predicting any sequence of significant transactions. After a year or more of this sort of thing, the produce of variables had reached an unmanageable degree of refinement and of magnitude; and, approaching the deadline, I was reminded of the judgment of Hippocrates: life is short, the art long, occasion instant, decision difficult.

REFERENCES

It is not possible to pinpoint in the vast libraries of books and periodicals the precise source of each assumption, concept, method that has been mentioned in this paper. I have included the names—all well-known—of the more influential theorists, but what I have acquired from some of these—Henderson, Jung, Prince, Alexander, Whitehead, Lewin, Kluckhohn, Allport, Parsons, and others—came very largely through conversations and discussions, and what I have acquired from the writings of these and others came, not from one article or book, but from pretty nearly all their works. This is not the place, it seems to me, to list the relevant works of Aristotle, Darwin, William James, Bergson, Lloyd Morgan, Santayana, Whitehead, or Cassirer, or of such social scientists as Pareto, Parsons, Lasswell, Malinowski, Sapir, or Kluckhohn, or of such authors as Janet, Freud, Jung, Adler, Rank, Alexander, Horney, Sullivan, Kris, or Erikson, or of those psychologists who are concerned with personality, such as McDougall, Allport, Murphy, Maslow, Adams, or McClelland.

My constant disposition has been to select new fields of investigation and to avoid those which have already been occupied, if not packed, by competent experimentalists. For example, I have had no first-hand experience in dealing with the intricate problems of perception or of animal learning, and hence I have mentioned but few names of psychologists who have contributed to our understanding of these phenomena. Many of these have in-

fluenced me directly as well as indirectly. But it would hardly be appropriate in this place to list the works of such men as Pavlov, Thorndike, Watson, Hull, Tolman, Dollard, Mowrer, Neal Miller, or Skinner, or of those who have been concerned with the philosophy, logic, or semantics of theory building, such authors as Bridgman, Stevens, Hull, Lewin, Koch, Egon Brunswik, Else Frankel-Brunswik, Bergmann, Meehl, or Feigl.

So far as my own bibliography is concerned, the latest edition of it can be found in C. S. Hall and G. Lindzey, *Theories of Personality* (1957), published since the completion of all that I have written here.

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THE STRUCTURE OF PSYCHOANALYTIC THEORY: A SYSTEMATIZING ATTEMPT¹

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Formerly I found it extraordinarily difficult to accustom my readers to the distinction between the manifest dream-content and the latent dream-thoughts. Over and over again arguments and objections were adduced from the uninterpreted dream as it was retained in the memory, and the necessity of interpreting the dream was ignored. But now, when the analysts have at least become reconciled to substituting for the manifest dream its meaning as found by interpretation, many of them are guilty of another mistake, to which they adhere just as stubbornly. They look for the essence of the dream in this latent content, and thereby overlook the distinction between latent dream-thoughts and the dream-work. The dream is fundamentally nothing more than a special *form* of our thinking, which is made possible by the conditions of the sleeping state. It is the dream-work which produces this form, and it alone is the essence of dreaming—the only explanation of its singularity [S. Freud: *The Interpretation of Dreams*, 98, pp. 466–467].

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¹The completion of this study was aided by the Ford Foundation's grant in support of research at the Riggs Center.

* The bracketed numbers, when they occur in the tables of contents of the essays in this volume, indicate items in the *Suggested Discussion Topics* relevant to the headings which they follow. See *Note on the Use of Discussion Topic Index Numbers* in the Appendix.

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INTRODUCTION

Neither Freud's nor other psychoanalysts' writings give a systematic statement of the psychoanalytic theory. This fact, combined with my acceptance of the outline suggested by Dr. Koch (reflected in my section headings), imposed problems that the writers of the other essays may not have had to face. It is proper, therefore, to state the premises of this essay.

1. Freud's writings are the source of psychoanalysis and provide the frame of reference for its systematic treatment. Thus this essay centers on Freud's work.

2. A systematic treatment of the theory should also take into account other contributions which decisively shaped the present form of the theory. Thus this essay draws extensively on Hartmann's and Erikson's work.

3. The systematic statement of the theory should establish its relation to the alternative ("Neo-Freudian") theories which arose from it. But an early attempt at systematization, such as the present one, can neglect them without prejudice. Thus this essay barely touches on Adler, Jung, Rank, Horney, Kardiner, and Sullivan.

4. An attempt at systematization should stay as close as possible to the formulations of the literature, but it should also interpret these. This essay, therefore, although it hews close to the existing theory, does draw inferences and does make judgments. Consequently the theory it presents may appear unfamiliar to the reader whose conversance with psychoanalysis is exclusively clinical or only cursory.

5. A systematic statement need not follow the emphases of the literature. Its emphasis should be dictated by systematic considerations. Thus this essay only touches on the theory of symptoms, psychosexual development, therapy (e.g., transference and resistance), and concentrates heavily on what Freud called metapsychology. It makes a distinction between what might be called the special or clinical theory and the general or psychological theory of psychoanalysis.

6. One of the aims of Dr. Koch's outline was to make the essays of these volumes comparable. To fulfill this requirement I found it necessary to present some considerations (e.g., on independent, intervening, and dependent variables, as well as on quantification) which have no direct roots in the psychoanalytic literature and which enter frames of reference somewhat alien to my own thinking.

7. Dr. Koch suggested that the authors of these essays assume the reader's familiarity with previous statements of the theory and dwell primarily on systematic issues. Complying with his outline made something of this sort unavoidable. Yet I had to conclude, from recent writings of psychologists about psychoanalysis, that *familiarity with the psychological theory of psychoanalysis* (as distinguished from the psychoanalytic theory of neurosis) *cannot be generally assumed*. The historical relationships—which play an important role in all unsystematized theories—seem to be particularly unfamiliar. Thus, time and again, I found it necessary to summarize theories and to sketch historical relationships. The result of my attempt to reconcile these conflicting demands is not a happy one. In the beginning of the essay the reader will find familiarity with many concepts and theories taken for granted, only to encounter some of them later on, again and again discussed in detail, with further information added each time. The time limitation—unavoidable in such collective endeavors as these volumes—permitted me no better solution; it is also responsible for the length of the essay. Had I prepared it on my own schedule, it would have matured for a few more years, and it might have become more comprehensive and tighter in its structure and “logical joints.”

To my mind it is too early to attempt a systematization of the psychoanalytic theory. A science can be a “good science” without being ready for a systematic presentation: all old sciences were once in this position. The existence of this essay is thus in need of explanation. I was

prompted to write it partly by the urgings of Drs. Gill, Hartmann, Holt, Klein, and last but not least, Dr. Koch, the coordinator of this APA project, and partly by my wish to pave the way for an adequate systematic presentation of psychoanalysis.

The very prematurity of this attempt had curious consequences. The essay presents several cross sections (for instance, models, points of view) of the theory which, though they are linked by identical concepts and by common empirical referents, are not systematically related to each other. The clearest indication of prematurity is the uncertainty whether we are *not yet able* to connect these systematically, or whether they *need not* or *cannot be* connected.

Since the literature directly bearing on the system of psychoanalytic theory is meager, I refer—contrary to custom—to mimeographed material of limited circulation and even to unpublished manuscripts. The English *Standard Edition* of Freud's writings is still incomplete, therefore the references are to that medley of editions which I have used over the years in my studies. Some of these involve inaccuracies corrected by the *Standard Edition*. While I am aware of these, I did not attempt to correct them.

The contradictions between this survey and the Rapaport-Gill study, which went to press since this manuscript was prepared, are explained partly by the survey character of this study and partly by the time lag.

As much as space permitted, I have referred to sources and acknowledged the specific help I received. Drs. M. M. Gill, R. R. Holt, G. S. Klein, and R. Schafer read the manuscript, and their suggestions and corrections were so numerous that without a heavy addition of footnotes, this is the only way I can acknowledge my indebtedness to them. To Dr. Holt I am particularly grateful, not only for his repeated readings, suggestions, and criticisms, but also for the share he had in shaping the considerations on variables and quantification. But I am in even greater debt to Erik Erikson, Merton Gill, Heinz Hartmann, and Samu Rapaport. Last but not least, I want to express my gratitude to Mrs. Ruth Shippey, Mrs. Barbara Kiley, and Miss Suzette Annin. Mrs. Shippey and Mrs. Kiley did the secretarial work on the several versions of this manuscript, and Miss Annin did the editorial and bibliographic work.

I. BACKGROUND FACTORS AND ORIENTING ATTITUDES

A. Background Factors

The formative influences in Freud's background were the Jewish tradition, an early developed interest in literature (particularly a devotion to Goethe and, through him, to ancient Rome), courses with

Brentano of act-psychology fame, the impact of Darwin's theory of evolution, clinical and laboratory research in neurology and neuro-anatomy (in the orbit of men from Helmholtz's circle), clinical psychiatric work (with Meynert), clinical work with neuroses (at first with Breuer, Charcot, and Bernheim), and self-observation.²

The *influence of Helmholtz* on Freud's theory is seen in the postulate of thoroughgoing determinism, in the central position of the pleasure-pain principle (and the primary process) which is patterned on the concept of entropy,³ in the reality principle (and the secondary process) which is patterned on the principle of least action, and in the "economic principle" which is patterned on the principle of conservation.

The experience in *neurological research* is responsible for Freud's conception (derived from Hughlings Jackson's view of the nervous system) of a series of psychological organizations (instances, structures) hierarchically and topographically superimposed upon each other. That experience is also responsible for the conception of associative networks organized superficially by contiguity but fundamentally by drives, for the conceptions of inhibition and facilitation, at first bodily transported into his system from neurology, and for his early assumption that psychodynamics is neurodynamics. Even when abandoned, this assumption still lingered on in the form of the belief that sooner or later psychodynamics would be placed on the "solid footing" of neuro- and/or biochemical-dynamics.

But Freud's laboratory research was also closely related to the *theory of evolution*, and it is probably this conjunction which is reflected in the genetic cast of Freud's thinking, particularly in the close relation hypothesized between phylogenesis and ontogenesis,⁴ in the emphasis on epigenesis, in the regression concept and many others. A Neo-Lamarckian version of evolution theory also seems to have influenced Freud's thinking [115, p. 64].

The effects of his *clinical psychiatric experience* with Meynert and his related readings (e.g., Greisinger), though probably crucial, have not been studied in detail.⁵ It seems reasonably certain, however, that the contents of the hallucinations in "Meynert's amentia" served as the prototype for the concept of "wish-fulfillment" [cf. 35, p. 136; 98, pp.

²This list represents a narrow view of "formative influences." For a broader, more psychological one, see Erikson [63, 64, 65] and Gross [154]; see also [15, 16, 17, 18; 193; 309].

³Freud refers it to Fechner [69, sec. 11, p. 94, note]; see [123, pp. 3, 4].

⁴Dr. F. Schmidl (Seattle) calls attention to Haeckel's particular influence.

⁵But see Hartmann's recent study [165].

509, 533] and as the foundation for what will be described below as the primary model of cognition. It is also probable that the often claimed influence of Herbart [see 193] and the less noticed one of Hering⁶ on Freud came indirectly through the work with Meynert and related reading. Neither his neurological research (dissection and microscopy) nor his clinical psychiatric work provided Freud with experience in the experimental method: both fostered his bent toward observation.

The *experience with neurotics* left its trace on Freud's system in the recognition that psychopathological phenomena, such as the unanatomical delineation of hysterical symptoms, are organized on principles other than those familiar to neuroanatomy and neurophysiology; in the recognition of the power of psychological forces (through the observation of hypnotic and waking suggestion with Breuer and Bernheim); in the recognition of the existence of nonconscious psychic formations (through observations of hypnosis and alternating states of consciousness by Charcot and Janet), and the conceptualization of these as the System Unconscious; and in the recognition of the crucial role of sexuality in neuroses (Freud attributed his first inkling of this to Charcot's, Chrobak's, and Breuer's incidental comments).

The influence of *self-observation* (including his self-analysis) is ubiquitous in Freud's theory, and accounts for the method of free association, for the role of dream interpretation as an investigatory tool, and for many specific discoveries.

The traces of *Brentano's act psychology*⁷ are less obvious and have never been explicitly discussed.⁸ Yet the central position of instinctual drives in Freud's theory parallels Brentano's interpretation (which contrasts sharply with that of Anglo-Saxon empiricists) of both stimulation and response in terms of acts of intending. In the early phases of Freud's ego psychology, Brentano's influence seems even more striking. The term *intention* crops up, the problem of reality testing leads to an analysis of the "belief in reality" [119, p. 146] along Brentano-like lines, and the distinctions between what is perceived and what is conceived, what is real and what is only thought, etc., come into play. This influence

⁶ Ernst Kris (personal communication, Jan. 11, 1957): "I have noted one of the most obvious sources for Freud's thinking, namely Hering's paper on memory. The evidence of Freud's interest reaching up to 1922 is absolutely conclusive and as far as I know never noticed. It might amuse you to look in this connection at Anna Freud's translation of Levine's book on the Unconscious. The translation of the chapter on Butler is by Freud and so is an interesting footnote."

⁷ Concerning Freud's contacts with Brentano, see Merlan [232, 233].

⁸ Dr. F. Schmidl suggests that it was through Brentano that Freud came to know of Maudsley, to whom he refers in *The Interpretation of Dreams*, and whose concept of the unconscious may have influenced Freud's.

pervades the *Papers on Metapsychology* [108, 110, 114, 115, 116, 117, 119, 120]. And although Freud deliberately refused to have anything to do with philosophy, he did acquire some familiarity with it through Brentano. In one of the few specific references he makes to philosophy, he characterized psychoanalysis (and particularly its concept of unconscious determination) as the psychological counterpart of Kant's philosophical views [117, p. 104]. Indeed, the epistemological implications of psychoanalysis are closest to Kant [see 262] and most remote from Anglo-Saxon empiricism.

The *influence of literature* in general and Goethe in particular are again hard to trace. They certainly shaped Freud's interest in and grasp of human nature. They provided the pattern for the case history as a tool, which medical case histories of his time did not supply (compare the best of these, Charcot's, for example, to Freud's). Indeed, it might be said that the intrinsic validity⁹ of his reasoning and descriptive writing often had to serve him as that indicator of validity which in older sciences is usually provided by quantitative measures. He became one of the outstanding masters of thought and pen in the German language (Goethe Prize). These influences also fostered in him that sensitivity to the subtleties of verbal communication and that readiness to seek meaning behind meaning which, combined with a knack for metaphor and symbol, are the requisites of interpretation. Indeed, they probably guided him to his central conceptions—motivations, affects, and conflicts—which are the raw material of all art.

The role of *the Jewish tradition* in Freud's thinking, methods, and theorizing has not been explored in detail either. Wittels [327], Reik [286], and Erikson [63, 64, 65] have elucidated some aspects of it. It is possible that much of what we attribute to Freud's interest in literature comes from the tradition of "the people of The Book." Associative and interpretive methods have some of their most striking archetypes in the methods of the Talmud. The stereotyped Aramaic phrase, introducing Talmudic interpretation, translated into English reads: "What does he want to let me hear?" But the degree of Freud's direct conversance with his Jewish tradition and its effect on his thinking have not yet been documented.^{9a}

⁹ By intrinsic validity I mean what literary criticism means when it speaks of a "valid statement": the great writer achieves a form which makes the expression of his observations, feelings, and thoughts a "valid statement." But even in everyday life, some of us convey an experience so that it is clear, convincing, and pregnant with meaning, while the reports of others are pale, pointless, and diffuse, as if they were third-hand.

^{9a} In the period of reading proof of this article, I noted advertisements for an apparently pertinent publication (D. Bakan. *Sigmund Freud and the Jewish Mystical Tradition*. Princeton: Van Nostrand).

We cannot abandon this survey of background factors without pointing to one whose significance has so far not been explored, the *Zeitgeist* [see 31, 32]. Darwin and Helmholtz were certainly part of it. Freud's Jewish background and fate, and their influence on his thinking, have never been discussed in the broad terms of *Zeitgeist*. Only Erikson¹⁰ has discussed the influence of the general atmosphere of Victorian Vienna, which was probably as much a limiter of Freud's social psychology as a source of his emphasis on sexuality and its vicissitudes. The assessment of the *Zeitgeist* attains particular urgency because of the fundamental similarities of Freud's theory to one immediately preceding it, and one immediately following it in time. Marx, Freud, and Einstein, who continued the Copernican, Kantian, and Darwinian revolutions, relativized our conceptions of the world. Marx, reversing Hegel's dictum, asserted that "man's [economic] existence determines his consciousness and not his consciousness his existence," and thus made man's view of his world relative to his socioeconomic status. More broadly, Freud asserted that man's view of and relation to his world are dependent upon (relative to) his impulses and are not simply imprinted on him by his experience. Most broadly, Einstein asserted that observation is relative to the observer's position. If it should turn out that the commonality of the three theories is as real as it seems, and is rooted in the *Zeitgeist*, then we would have before us a background factor which, though subtle and nonspecific, might prove the most pervasive and most powerful of all.

B. Orienting Attitudes

1. The nature and limits of psychological prediction. Prediction in psychology implies the postulation of thoroughgoing determinism in human behavior. Freud's assumption of exceptionless *psychological* determinism, which is perhaps too easily taken for granted today, provides the necessary foundation for prediction.

Since the empirical material first dealt with was the already present neurotic symptoms, Freud's primary causal problem was postdiction rather than prediction. This initial situation is not unique to psychoanalysis. It has its counterparts in the social sciences, e.g., in history, and in the natural sciences, e.g., in the theory of evolution. A theory is not invalidated by being postdictive, as long as postdiction is carefully distinguished from *ex post facto* explanation.

Because the observations were made in the therapeutic situation, the predictions were of necessity related to the effects of therapeutic interventions and thus were fraught with the same difficulties which have

¹⁰ Erikson [64] also calls attention to the influence that the economic theories of the time seem to have had on Freud's thinking.

beset recent investigations involving "participant observation" and "action research." Freud believed that only first hand experience with the psychoanalytic method of treatment could make understanding and valid tests of psychoanalytic predictions possible, that the psychoanalytic theory could be validated only by the psychoanalytic method, and that it *had*, indeed, been so validated and needed no further validation. The method, the theory, and its validation were considered inextricably tied to one another.¹¹

While these limitations on verification and prediction were professed, from the beginning observations of hypnotic phenomena were invoked to validate the propositions concerning the unconscious [19, 35]. Hypnotically induced dreams [306], hypnagogic [310] and daydream phenomena [319] were hailed as independent evidence corroborating the predictive (and postdictive) power of the theory. Moreover, ethnology [109], literature [102], and psychotic products [107] were increasingly invoked as independent confirming evidence, though their use to corroborate the theory tended to merge with the use of the theory for their explanation.¹²

More recently, it became increasingly evident that direct studies of infant and small-child behavior were needed for the independent confirmation of the theory's postdictive reconstructions of these phases of life, and many such investigations were undertaken. Psychodiagnostic and experimental evidence has also been increasingly invoked as confirmation of the theory, though the investigations by which this evidence has been obtained have rarely shown due regard for the complexity of

¹¹ The discussion of "critical tests," in Section IX.C. below, points out that whereas in other sciences tests validating a theory decide between alternative and mutually exclusive possibilities, as a rule this is not possible for psychoanalytic theory. The alternatives envisaged by psychoanalysis are not mutually exclusive but rather equivalents which can substitute for each other, according to the dynamics of the situation. Thus the theory is not built by tests of predictions excluding all but one of several alternatives, but rather by the inclusion of all observed alternatives which are consistent with the existing theory. Only those alternatives which clash with the existing theory are excluded. The observation which suggests such incompatible alternatives is rechecked by further clinical observations. Rechecks which confirm the incompatible alternatives, and thus do not permit their exclusion, lead to the modification of the theory. It is thus that postdiction—guided by the aim of preserving the internal consistency of the theory, rather than by the principle of parsimony—becomes the principal means of theory-building in psychoanalysis.

¹² All sciences must subject observations to interpretation in order to establish their evidential significance for the theory. This is particularly conspicuous in psychoanalysis, where the concepts are by and large at a considerable distance from the observations. For a further discussion of this point, see pp. 116 ff.

the theory, and their methods have rarely been such that the results could pass as confirmations of the theory.¹³

Psychoanalysis did concern itself with one class of predictions, namely, prognoses. These are part of the subject matter of applied psychoanalysis (clinical psychoanalysis and psychiatry) and not of theoretical psychoanalysis proper. The problem of prognosis has three facets: the prognosis for treatment by the psychoanalytic method, the prognosis for "spontaneous remission," and the prognosis for treatment by modified psychoanalysis or other therapy. So far the study of the criteria of prognosis has yielded rules of thumb rather than theory, yet the concepts of "ego strength" [158], "model technique" and "parameters of technique" [51] did arise in this context.

Once the postdictive character of psychoanalytic propositions is clear, another characteristic of the theory also becomes obvious. The detailed study of dreams, of symbolism, of slips of the tongue, of wit, of association sequences, and the like, suggests that psychoanalysis studies and predicts behavior on this "microscopic" level; yet the actual aim of the theory was always to predict or postdict life-sized ("macroscopic") segments and sequences of behavior.¹⁴ This curious duality is characteristic of the theory: it is holistic, but not because it lacks methods for studying and predicting the "microscopic"; and it is atomistic in the sense that it can and does study the "microanatomy" of behavior, but not because its methods and interests limit it to "microscopic" phenomena. Naturally, the verification of its theory of slips of the tongue by post-hypnotic suggestions [53] or the verification of its theory of symbolism by means of suggested dreams [306, 288, 244, 83], which involve "microscopic" predictions, does not verify the "macroscopic" relationships predicted by the theory; in turn, verification of macroscopic relationships (e.g., that of homosexuality to paranoia [cf. 246]) does not necessarily confirm the detailed mechanisms (such as projection) which, according to the theory, mediate these macroscopic relationships.

In conclusion: the nature of the material Freud worked on led him to overemphasize postdiction and underemphasize prediction in building his theory. In this he was also influenced both by the type of neurological work he and his teachers pursued, and by the methods of the biological science of the time. But it may be questioned whether or not any science in its beginnings has been free from such imbalances. The basic necessary

¹³ The trouble with these investigations is that either their status as a source of *independent* evidence for the theory is not established, or their relevance to the theory is not established. Cf. Section V., below.

¹⁴ The terms "microscopic" and "macroscopic" are used here in the sense indicated by the examples, *without* reference to any of their various usages in the literature.

condition for predictions and for their confirmation is present in the theory of psychoanalysis, and certain types of psychoanalytic predictions have been confirmed. Moreover, postdiction, if properly handled,¹⁵ is as valid a confirmation of a theory as prediction. The task ahead is to add to the *necessary conditions* of prediction the *sufficient conditions*,¹⁶ by tightening the theory and by developing adequate methods of quantification and confirmation.

2. Level of analysis. The level of analysis has changed repeatedly in the history of psychoanalytic theory.

First, Freud (1895) made an attempt [94] to account for all behavior by neurodynamics, though even in this period he already had a clear outline [35] of his psychological theory, which centered on the conflict between environment and ego (memory of traumatic experience vs. social propriety and self-respect). At this point, he equated the ego with consciousness (i.e., the dominant ideational complex) and the unconscious with what the environment disapproved of. Thus, early psychoanalysis operated with three "levels of analysis": neuroanatomy and neurodynamics, environment vs. ego, Conscious vs. Unconscious.

Second, in the next phase (1900) of the theory [98], "intrapsychic dynamics," centering on the drive vs. censorship conflict, becomes the causal referent of all behavior and the ultimate causal factor. But even in this period censorship and secondary process are connected by Freud with reality and interpersonal relations (environmental and psychosocial referents). Yet the dominant level of analysis is the intrapsychic one, in terms of drives vs. censorship.

Third, with the development of ego psychology (1923), a dual intrapsychic reference system crystallizes [126]: drives and structures are juxtaposed. The dominant level of analysis is still the intrapsychic one, in terms of drives vs. structures.

Fourth, (1926) the structural concepts are recognized in part as representing external reality referents¹⁷ and the drives are recognized as representing biological referents.¹⁸ Thus the intrapsychic reference system

¹⁵ The difficulties in confirming postdictions are these: the data on which a postdiction is based must in some inferable form imply the relationships to be postdicted; however, the relationships implied in the data must not be so obvious as to make postdiction superfluous. "Proper handling" of postdiction thus has to make explicit both what is given in the data on which the postdiction is to be based and what is not given and can be only inferred by postdiction. This is easier said than done, however.

¹⁶ See Benjamin [11] for the first discussion of this issue in the literature. See also footnote 20.

¹⁷ Cf. Section II. H., below.

¹⁸ Freud wrote: ". . . 'instinct' appears to us as a borderland concept between the mental and the physical, being both the mental representative of the stimuli

is reduced to organism vs. external reality, and a variety of hypothetical constructs (drives and structures) are interpolated. There are now three levels of analysis: biologic, intrapsychic, and real, though all of these are handled in terms of their psychological representations.

Fifth, (1937–1946) the psychosocial referents crystallize in the work of Horney [181, 182], Kardiner [194, 195], and Sullivan [313] on the one hand, and in that of Erikson [56, 57, 59, 60] and Hartmann [157] on the other. A system of multiple levels of analysis evolves, including the dynamic, economic, structural, genetic, and adaptive levels, whose foundations had already been built in the earlier phases.

In conclusion: the psychoanalytic theory, by its conception of “over-determination,” kept itself open to all relevant “levels of analysis,” and was not limited to a single one as were many other theories. Yet the “intrapsychic” concepts in general, and the drives in particular, remain central to the theory.

3. Utility and role of models. Freud’s theory contains four distinct models. They are united in the theory itself, but not in one single model. We will first present each of these, and then attempt to develop a combined model.

a. The reflex-arc (or topographic) model. This model [98, pp. 498ff.] represents—as it does in the stimulus-response theories, too—the tendency of the organism to respond to stimulation. The Freudian model, however, has additional specifications:

1. This tendency is regarded as a *direction* of psychological processes.
2. It is one of the two directions excitations can take, the other being the regressive.
3. In the ideal case the excitation begins in a sensory stimulation, passes through the Systems Unconscious, Preconscious, and Conscious, and terminates in motor action: this is the “topographic” course.
4. Not every excitation, however, need pass through the complete topographic sequence.

For instance, excitations can originate in the Unconscious: drive-excitations usually do so, though drive action is often triggered by a stimulus. Excitations can also originate in the Preconscious: dreams are initiated by preconscious day-residues. Nor is it necessary that excitations initiated by a sensory stimulus run the whole topographic course; they may terminate, temporarily at least, in the Unconscious or Preconscious: that this is the case with “unconscious” and “preconscious” perceptions, which are clinical commonplaces, has been confirmed by the experiments of Poetzl [257], and others [e.g., Huston, Shakow, and Erickson.

emanating from within the organism and penetrating to the mind, and at the same time a measure of the demand made upon the energy of the latter in consequence of its connection with the former” [115, p. 64].

188; Diven, 47; Fisher, 84; Klein et al., 201]. Similarly, an excitation can terminate in consciousness without initiating a motor response. Recent developments in psychoanalytic ego psychology demonstrate that autonomous functions of the ego (particularly automatized ones) may short-circuit the topographic course. Thus a place within psychoanalytic theory is allocated to automatized (one-to-one) stimulus-response relations. The topographic locus of origin is an important characteristic of excitation processes.

This model was useful in two ways. On the one hand it coordinated descriptively a welter of otherwise disparate observations, such as the vicissitudes of stimulations, the alternative (ideational, affective, action, and abeyant) responses to stimulations, the lack of one-to-one relationships between stimuli and responses, and the wide variety of apparently "spontaneous" ideational, affective, or action responses (ranging from dreams, daydreams, delusions, blushing, sweating to parapraes and random movements). On the other hand, it served as the foundation for the topographic point of view in general, and for the concepts of the Systems Unconscious, Preconscious, and Conscious in particular, and these in turn were the predecessors of the structural point of view.

b. The entropy (or economic) model. This model [98, pp. 509, 533]—implicit in the direction attributed to the course of excitation in the topographic model—is the crucial, topographically incomplete¹⁹ sequence of infant behavior: restlessness → sucking on the breast → subsidence of restlessness. This sequence, which makes behavior the referent of tension-reduction processes, is regarded as the basic model of all motivated behavior, and—in keeping with the postulate of determinism—pertains to obviously motivated behaviors as well as to apparently accidental ones. *It can be modified*—as we shall see—to account for tension-maintaining and tension-increasing processes also. The merit of this model is that it coordinates a wide range of phenomena, and serves as the foundation for the concepts of the pleasure principle and wish-fulfillment in particular, and the economic point of view subsuming them in general. It plays an important role in the transformation of the topographic into the structural point of view, and also contains the core of the dynamic and adaptive points of view. Since this model already implies some of the others, we will later present a sketch of a previous attempt [267] to develop it into a unified psychological model of psychoanalytic theory.

c. The Darwinian (or genetic) model. This model [101; cf. also 1], which asserts that the course of ontogeny abides by inborn laws, served Freud as the frame of reference for systematizing the data of his patients'

¹⁹ See p. 67, above.

life histories, and became the foundation for the genetic point of view in general, and for the theory of psychosexual (libido) development, including the concepts of fixation and regression in particular. Freud's inclination to ally the Darwinian model with Haeckel's biogenetic law (ontogeny repeats phylogeny) on the one hand, and with the Lamarckian view of evolution on the other, led him to some inferences which have been seriously questioned. Yet some of these inferences proved fertile, useful, and independent of the grounds they were built on. Haeckel's biogenetic law helped Freud in elaborating and using the Jacksonian model. Lamarck's model enabled him to conceive of processes of adaptation for which Darwin's theory did not provide the conceptual means. The core of Freud's genetic conception, namely psychosexual development, is probably the most familiar segment of psychoanalytic theory, and we need not dwell on the evidence which led Freud to make it the center of his genetic model. The model's usefulness was not exhausted by Freud: both Hartmann's [157] concept of "change of function" and Erikson's [58, 62, 66] "epigenetic" conception (which extends the postulate of "the lawfulness of ontogeny" to behavior development far beyond the confines of psychosexual development) are based on it.

But this sketchy statement does small justice to the pervasive significance of the genetic model in Freud's theory [see Rapaport, 279]. Actually, concepts as high in the theoretical hierarchy as identification and transference, and theories as complex as that of object choice have their roots in this model. It was the genetic model which enabled psychoanalysis—unlike contemporary learning theories—to put, instead of *prior learning*, *prior inborn givens* in the center of its conception of learning. [For similar attempts, see Lorenz, 225; Tinbergen, 315; Piaget, 254; Schiller, 303.] Such genetic considerations made it possible for Freud to realize the significance of early experiences for adult behavior. It took academic psychology fifty more years to come to this realization. [Cf. Hunt's confirming experiment, 186, 187, Hebb's theory, 169, and Beach and Jaynes' review, 10].

Erikson's [61] as well as Hartmann's [157] and his collaborators' [167] work has advanced our genetic understanding, as has Hartmann and Kris's discussion [166] of the genetic and the dynamic propositions of psychoanalysis. Werner's [322] and Piaget's [254, 255, 256] work in genetic psychology were advances in the same direction. Normative and longitudinal studies have contributed considerable systematic observational material concerning genetic sequences. Yet the methodological problems involved in the study of such sequences and in the application of the genetic point of view have still not been solved.²⁰

²⁰ See John Benjamin, "Prediction and Psychopathological Theory," in press.

d. The Jacksonian (or neural integration hierarchy) model. According to this model, the nervous system consists of a hierarchy of integrations in which the higher ones inhibit or control the lower, and damage to or suppression of the higher ones reinstates the function of the lower. When Freud abandoned his neurological anchorage (1898), he ceased pursuing neuropsychological speculations and hypothesized hierarchically organized psychological systems patterned on Jackson's hierarchy of neural levels [98, p. 488]. This is implied in one of the specifications of the reflex-arc model, namely, in the sequence of the Systems Unconscious, Preconscious, Conscious. Freud's Jacksonian model is closely related to both the genetic and the topographic models, and its utility is that it provides the means for coordinating systematically those behavior phenomena which are not attended by voluntary control and/or consciousness with those which are. Not only are the concepts of the Systems Unconscious, Preconscious, and Conscious (as well as those of the id, ego, and superego) organized according to this model, but Freud assumed that every advancement in psychic organization goes along with a new censorship [117, pp. 122-127], and his conception of the multiple layering of defenses within the ego also follows the same pattern [116, 131].

But this does not exhaust the unique significance of this model in Freud's theory. After all, Janet and Prince based their conceptions on a similar model, even if Janet did not assume that the "subconscious" existed under the control of consciousness (in the Jacksonian sense), but rather that the "subconscious" was created by dissociation caused by degeneration and precipitated by trauma. In Freud's theory, inhibition of lower levels by higher ones served as the model for the conceptualization of conflict. Thus inhibition became a dynamic event: the result of a clash of forces. To begin with (1895), these forces were conceptualized as the libidinal affects vs. the ego, the latter being the "ruling ideational mass" which serves reality, society, and morality [35, p. 116]. Later (1900), this conception of conflict yielded to that of drives vs. censorship, the latter representing ego (self-preservative) drives [101, 114]. The final conception (1923) was that of the interstructural conflict between the ego and the id, with the participation of the superego on one or both sides [126, 137]. Thus, the Jacksonian model coordinated those observations which of old were labeled "conflict" with those from which "unnoticed conflicts" could be inferred, and it served as the foundation for the concepts of unconscious conflict, inhibition, unconscious drive forces and counterforces, which led to the theory of symptoms, and ultimately to the theory of mental structure. In summary: the Jacksonian model served as the foundation for the

dynamic point of view in Freud's theory, and also contributed to the development of the topographic and structural points of view.

e. A combined model. Let us now sketch in more detail the entropy (or economic) model, the behavior sequence *restlessness* → *sucking on the breast* → *subsidence of restlessness*. [For specific references, see Rapaport, 267.] Here restlessness is considered the referent of tension accumulation, sucking on the breast that of tension-reducing action on the object, and subsidence of restlessness that of a state of reduced tension. These are equated with accumulation of cathexis, action on the cathected object, and discharge of cathexis, which in turn are referred to drives reaching threshold intensity, drive action on the drive-cathected object, and drive-gratification. Finally, the direction implicit in all these sequences is conceptualized as the *pleasure principle*.

This is the *primary model of action* (conation). It is an action model because it does not account for thoughts or affects. It is a primary model because it represents only actions motivated by basic drives, without that intervention of psychic structures, derivative drives, and other motivations, which is characteristic of most observed actions. It is the first of the six models to be derived here from the behavior sequence which is considered to be the model of all motivated behavior. We shall now derive the primary models of thought and affect, and then turn to the secondary models.

The *primary model of cognition* (ideation) was formulated by Freud [98, pp. 509–510, 533] in 1900: *drive reaching threshold intensity* → *absence of drive object* → *hallucinatory idea of previous gratification*. When the drive object is absent, drive action is not possible, and a short cut to hallucinatory gratification takes place. Drive cathexis is displaced to the memory of past gratifications, bringing these to hallucinatory intensity. The short cut and direction implicit in this model were conceptualized by Freud as *wish-fulfillment*. It is worth noting that both the *pleasure principle* and *wish-fulfillment* (which is its cognitive equivalent) are abstractions remote from the common-sense meaning of *pleasure* and *wish*. The model extends the economic point of view to cognitive phenomena, and its concept of wish-fulfillment expresses the directed, intentional character of cognition. This model makes it possible to include phenomena like dreams, hallucinations, illusions, daydreams, reveries in the theory of motivated behavior, and serves as the foundation for those concepts which in the secondary model of cognition coordinate these thought forms with the more familiar cognitive phenomena of ordered veridical thought. It provides the theoretical matrix for the understanding of free associations and projective techniques, and concepts for the explanation of the observations in states of need (hunger

and thirst [197]), stimulus deprivation [see Hebb's and his associates', 25, 170, 171, 172, and Lilly's experiments, 222, 223], and hypnotic states [see Gill and Brenman, 33, 148; also Rapaport, 280].

The *primary model of affect* was formulated by Freud [98, pp. 520–521] in 1900: *drive reaching threshold intensity* → *absence of drive object* → *affect discharge*. In the absence of the drive object, drive action not being possible, emergency discharge through affect-discharge channels takes place. Freud characterized affect discharges first as “sally gates” for drive tension [98, p. 520], and later (1911) as discharges into the interior of the organism (autoplastic adaptation), in contrast to alterations of external reality by action (alloplastic adaptation) [108, p. 16].²¹ While other psychological theories postulate direct links between affective stimuli and the bodily changes and subjective experiences involved in affects, this model—like the topographic one—inserts unconscious ideas and drives between affective stimuli and affective responses. This modification makes possible a unified theory which can account for anxiety and other persistent affects—for affects which are not triggered by obvious affective stimuli, as well as for the commonly treated forms of affect. It also eliminates some of the puzzles confronting, and various of the contradictions between, the familiar theories of affect (James-Lange, Cannon, etc.). [For a detailed discussion, see Rapaport, 258.]

The relationship between the primary model of action and the primary models of cognition and affect is indicated by the presence of the drive object in the former and its absence in the latter. The relationship of the cognition model to the affect model is expressed in the *combined primary model of cognition and affect* which was formulated by Freud [116, p. 91; 117, p. 111] in 1915: *drive at threshold intensity* → *absence of drive object* → *hallucinatory idea and/or affect discharge*. It was devised to account for a set of clinical observations. Clinically, the repressed drive is inferred from its ideational and affect representations. While in obsessional ideas only the ideational representation of the drive is observed (its affect representation usually succumbing to defense, e.g., repression, isolation, or displacement), in hysterical attacks only its affect representation is manifest (the ideational representation succumbing to defense, usually repression). Affect and idea are thus conceived of as complementary and/or alternative drive representations.

These primary models unify the traditional trichotomy of conation, cognition, and affection. They are clearly entropic (economic) models,

²¹ Of the interactions between environment and organism, those which result primarily in changes of the organism are called autoplastic, and those which result primarily in changes of the environment are called alloplastic.

though they do imply the topographic (reflex-arc) model in their direction, and the dynamic model in the role played by the drives. They have adaptive implications in that they posit coordinations, guaranteed by evolution, between the drive and a real object (the drive object), and between individuals by means of affect expressions. They have structural implications: thresholds, which must be reached by drive intensities before drive action can take place, and drive-discharge channels. They have genetic implications, in that they are assumed to pertain primarily to early developmental phases. The secondary models elaborate these adaptive, structural, and genetic implications.

The *secondary model of action* was outlined by Freud [98, pp. 533–534] in 1900: *drive reaching threshold intensity* → *derivative drive mobilized by basic drive or reaching threshold intensity* → *structuralized delay in the presence of the drive object* → *detour-activity searching for, and means-activity reaching for, the drive object* → *satisfaction*. Let us take the relationships between the steps of this sequence one by one.

Drive reaching threshold intensity → *derivative drive mobilized by basic drive or reaching threshold intensity*. This step implies both the Darwinian and the Jacksonian models: it has both genetic and hierarchic-structural implications. In the course of development, drives differentiate into a hierarchy of derivative drives. This drive hierarchy, in turn, has both adaptive and structural implications. Derivative drives differentiate according to ontogenetic laws, e.g., that of psychosexual development, yet the *occasions* for this differentiation are environmental (for instance, the periodic unavailability of the drive object, the appearance of substitute objects, the environment's response to and demands for new ontogenetic achievements, etc. [61, 62, 66]) and the differentiation itself is adaptive. The progressive lessening and change of maternal and familial care are the environmental counterparts of this adaptive development and provide the occasions for it. Thus, the development of derivative drives is not a matter of pure learning, nor is it blindly regulated by ontogenetic laws.

When the drive object is absent, the drive-discharge threshold is raised by *countercathexes*, and these countercathetic energy distributions are conceptualized as *control* and *defense*²² structures and derivative drives.²³ This is the outstanding dynamic implication of the model: in-

²² The expression "control and defense" refers to an insufficiently studied set of phenomena. Certain countercathetic energy distributions effectively prevent the execution of the motivations against which they are directed: they are termed defenses. Others merely delay, modulate, and channel motivations: they are termed controls. In actual observation, instead of this sharp dichotomy, we find a fluid transition. For a further discussion see [268, part 7].

²³ The manifestations of energy distributions are always forces, and clinically, defenses are always recognized by the appearance of new motivations.

hibition, resulting from a conflict between motivating forces and structures, gives rise to new motivating forces.

A further implication of this first relationship is that action may be initiated in several ways. The basic drive may initiate it on reaching threshold intensity. In this case the derivative drives (if they have not reached threshold intensity) may either be bypassed, or triggered by the basic drive. But action may also be initiated directly by a derivative drive which has reached threshold intensity. Or it may be initiated when an external stimulation provides the excess excitation which brings a basic drive, or any derivative drive, to threshold intensity. Here again we encounter the hierarchic arrangement, and the short-circuiting possibilities discussed in connection with the topographic model.

Now to the second relationship: *derivative drive mobilized by basic drive or reaching threshold intensity* → *structuralized delay*. This relationship implies all the above-discussed possibilities for initiating action. Here the structuralized delay plays, on the one hand, the role played by the drive-discharge threshold in the primary model: it delays discharge up to a certain point. On the other hand, it plays the same role as does the absence of the drive object in the primary model: it enforces delay beyond the point of the original discharge threshold. Structuralized delay (i.e., control or defense) is conceived as the heightening by counter-cathexes of the original threshold, so that the object of the drive defended against will be absent (unnoticed or unusable) from the point of view of *psychological reality*, even when present in *external reality*. Here the *psychological* absence of the object plays the same role as its real absence does in the primary model.

Controls and defenses are conceptualized as structures: their rates of change are slow in comparison with those of drive-energy accumulations and drive-discharge processes. The delay of discharge, which these structures make possible, is the crucial distinction between the primary and the secondary models. In the primary models, the *pleasure principle* (the direct discharge tendency) prevails; while here, the contrary principle of least effort—which is *one* of the referents of the higher-order concept *reality principle* (Freud, 1911)—prevails. Threshold and drive intensity are relative to each other, yet observations necessitate the assumption that the control and defense structures may become relatively independent of the drives. The relevant observations—for example, the adaptive role of some behaviors which originated as defensive reaction formations—are the same as those upon which the concept of autonomy rests. The structures here are ego structures; their autonomy is one of the implications of ego autonomy [157, 266, 280], which is akin to Allport's [8] conception of "functional autonomy."

Now to the next relationship: *structuralized delay* → *detour-activity*

searching for and means-activity reaching for the drive object. In the primary model, when the drive object is absent, either the memory of prior gratification is drive-catheted, resulting in hallucinatory wish-fulfillment, or part of the drive cathexis is discharged through the “sally gate” of affect-discharge channels. In the secondary model, structuralized *delay* postpones discharge, and makes possible *detours* from the direct route of gratification and search for the drive object. The concepts of *delay* and *detour* are familiar to psychology (Hunter, 1913; Köhler, 1917). Though Freud introduced them in 1900, psychologists apparently did not notice that, once taken seriously as concepts, they can account for the distinction as well as the link between impulsive and controlled behavior. We will encounter delay and detour again in the secondary model of cognition.

Finally, the last relationship, *detour- and means-activity* → *satisfaction*, implies that it is not necessarily *drive-gratification* that is attained by sequences of this sort.²⁴ We have seen that such sequences may be initiated by drives or derivative drives, either of which may be triggered by external excitations. Now—taking the autonomy of the ego into consideration—we must add that external excitation may also directly trigger detour- and means-behaviors: the functions subserving detour- and means-behavior are ego functions. Structuralized delay and detour, and structure in general (defense-, control-, and means-structures) are the concepts which enable this theory to account for tension maintenance and tension increase, and not, as is generally supposed [7], for tension reduction only. The shift from the “gratification” of the primary model to the “satisfaction” of this model indicates that full discharge of drive tension gives way to discharge compatible with the maintenance of tension which is made inevitable by structure formation.

The *secondary model of cognition* was outlined by Freud [98, pp. 509–510, 533–536] in 1900: *drive or derivative drive at threshold intensity* → *structuralized delay* → *experiment in thought with small cathectic amounts to anticipate and plan, locate and act upon the drive object.* The first two steps in this model are the same as those in the secondary model of action, and the considerations presented above apply. The relationship between structuralized delay and experimental action in thought is the only one to be discussed. According to the primary model of cognition—which follows the pleasure principle—when drive action cannot take place, a short cut to hallucinatory gratification occurs, through the mechanisms of displacement, condensation, substitution,

²⁴ K. Lewin's [216, 220, 221] quasi needs are examples of this. More generally, the distinction between drive-gratification and satisfaction corresponds to the distinction between the varieties of action-initiation discussed above on p. 74. For further discussion see [267].

symbolization, etc. In the secondary model, under the same conditions, structuralized delay prevents the short cut, and detour-behavior involving ordered thinking results.

The conception of two types of memory organization rests on this difference between the two models [268, note, pp. 630-631]. In the drive organization of memories, all the memorial (ideational) representations of a drive are organized around it and are equivalent to each other. The syncretic mechanisms enumerated above express this equivalence.²⁵ In the conceptual organization of memories the equivalences have two determiners: on the one hand, empirical coordinations (frequent contiguity), on the other, logical implications (not all frequent contiguities are admitted, but only those which are compatible with logical implications).²⁶

These two memory organizations do not predicate two *classes* of thought, but conceptualize two different *aspects* of any given thought, with the stipulation that the conceptual organization is hierarchically higher than the drive organization, and has a controlling function over it. These cognitive models, and their genetic relation to each other, represent the first consistent attempt to coordinate, within one theory, those forms of thought (obsessions, delusions, dreams, etc.) which are peremptory and those (practical thought, rational thought, rigorously logical thought) which we can take or leave.

In this secondary model, the intentional, anticipatory potential of thought derives from the directedness of the primary model, while its realistic efficacy derives from the structuralized delay which militates against the immediate discharge and gratification tendency of the primary model, and thus permits the development and use of conceptual coordinations.

The *secondary model of affect* was formulated by Freud [131, chap. 8, particularly pp. 76-79] in 1926: *drive or derivative drive at threshold intensity* → *structuralized delay* → *affect signal released by the ego from structurally segregated affect charges*. [A more detailed discussion will be found in "The Psychoanalytic Theory of Affects," 274, and in *Organization and Pathology of Thought*, 268.] The first relationship of this sequence is identical with that of the other secondary models, and the considerations advanced above apply, with one exception: here the role

²⁵ The term "equivalence" as used here is a generalization of the equivalence implied in the concept of "equivalent stimuli." It pertains not only to stimuli but to responses also. It applies not only to "nondiscriminable" stimuli or responses but also to those whose relationship to each other is that of *indicator to indicated*.

²⁶ Further discussion of these memory organizations will be found in *Organization and Pathology of Thought* [268] and in "The Psychoanalytic Theory of Thinking" [265].

of the drive's reaching threshold intensity is changed and the consequences of mounting drive tension are anticipated by the ego.

Early in his theory-building, Freud assumed that when no drive action can take place, the affect-discharge channels serve as "sally gates" for part of the dammed-up drive cathexes (affect charge). As late as 1915 he assumed [117, pp. 109-112] that only the drive intensity and the capacity of the affect-discharge channels determine the affect charge, and that the latter, before its discharge, is not segregated from the drive cathexes. This conception still applies to affects in early phases of ontogenesis.

Later (1926), however, it became necessary to assume that a structural segregation of affect charges from drive cathexes at large takes place, parallel with the development of the motivational and structural hierarchy, as specific affects and affect-discharge channels differentiate at each level of the hierarchy. Originally, accretion of drive intensity was assumed to use the affect-discharge channels automatically when drive action, in the absence of the drive object, is not possible; now, the absence of objects has been internalized in the form of structuralized delay, and the ego structures subserving this delay include such as keep the affect charge segregated and control its discharge also. The segregated affect charges are therefore under the control of the ego: when rising drive tension impinges on the ego's defense structures, the ego uses the segregated affect charge to give an anticipatory affect signal, which—though of small intensity in comparison to affect discharge—mobilizes (by virtue of the pleasure principle) counter-cathexes to reinforce the defenses, and thus prevents drive discharge [131, pp. 18-20, 112-117]. Affects change in the course of ontogeny from discharge phenomena into signals, from safety valves for drive tension into anticipations of the means for preventing drive discharge. Under such "normal" circumstances as bereavement or danger (but also when exposed to wit and drama), as well as under pathological conditions, the signal affects may yield their place to discharge affects [see 75]. Also, the segregated affect charge, like all cathectic amounts, may manifest itself as a motivating force.²⁷ According to the secondary model, affects may serve as discharge processes, as anticipatory ego-signals for mounting drive tension, and as motivations; thus it unites a wide variety of observations concerning emotions. Indeed, most of the observations on

²⁷ For instance, actions related to an unconscious sense of guilt may be motivated by the aggressive impulse which gave rise to the guilt affect, or by a (derivative) motivation which arose as a reaction-formation to this aggressive impulse, or by the guilt affect itself which has attained the status of a relatively autonomous motivation [see 267]. This may be a link to Leeper's [211] motivation theory of emotions.

affects which the various academic theories account for—or fail to account for—are coordinated in this model.

The behavior forms represented by the secondary models arise according to ontogenetic laws from those represented in the corresponding primary models, but their development also depends on the environmental conditions and is thus adaptive.

In contrast to the primary models, all the secondary models involve structuralized delay, that is to say, progressive, hierarchically layered structure development. The structures in question are: defense and control structures, structures which segregate affect charges, and the means structures which subserve secondary action- and thought-processes. A parallel development takes place in the hierarchy of motivations: each step in structure development results in a delay imposed on motivations, which in turn gives rise to new derivative motivations and affects. This multifaceted hierarchic development is the development of the ego [268, 276] and involves the differentiation of the ego from the id, and the superego from the ego. The id-ego-superego trichotomy is the broadest structural articulation of the mental organization and, as such, a crucial conception of the clinical theory of psychoanalysis. Since it can be derived from the models discussed, it is not an independent model and we shall not dwell on it here. [For a similar conception, see Glover, 150.]

The secondary models lean heavily on the Darwinian (genetic) and the Jacksonian (hierarchic) models, and therefore the structural, genetic, and adaptive considerations are central to them. Yet they also include the topographic, economic, and dynamic considerations of the primary models. Thus, this combined model, which is an elaboration of the entropic (economic) model, does unite all the models Freud used. But it does so at the price of falling into six partial models which by their very nature (if not by that of the theory, or even of the subject matter) overlap.

4. The comprehensiveness of empirical reference. From the very beginning, the theory implied a comprehensive empirical reference, though it centered on the psychology of drives and primary processes, and maintained that its findings concerning these were of unrestricted validity.²⁸ Freud asserted as late as 1917 [121, pp. 330–333] that the postponement of the exploration of secondary processes, ego functions, reality relations, and adaptation was a deliberate policy and not a failure

²⁸ That is to say, they are the ultimate determiners of *all* behavior. The conception of *ultimate determiner*, and the restrictions imposed on it later, will be discussed further on pp 93 ff., below.

to recognize their importance. Actually, Freud did take steps toward including them in the theory in 1900 [98, pp. 533, 535] and in 1911 [108], and consolidated these steps between 1917 [121] and 1926 [131]. But only in 1939 did Hartmann [157] give the first systematic formulation of reality relationships and adaptation by expanding the frame of reference of ego psychology. This systemization, which was accompanied by the claim that psychoanalysis is a comprehensive system of psychology, was continued in the studies of Hartmann, Kris, Loewenstein [160, 161, 162, 167, 168, 206], Rapaport [268, especially part 7; 267, 277], Jacobson [190, 191, 192], and Gill and Rapaport [149].

In the meanwhile, and even before these developments in the mainstream of psychoanalysis, adaptation and reality relationships, especially the role of interpersonal relations and society, were central to the theories of Adler, Horney, Sullivan, and Kardiner [see Munroe, 240]. Erikson [56, 57, 58, 59, 60, 61, 62, 63, 66] was the first to unite this tributary of theoretical development, which enlarged the actual realm of empirical reference, with the mainstream of the theory.²⁹

Finally, in the late thirties, forties, and fifties, the influence of psychoanalysis and of the new psychoanalytic ego psychology expanded to the whole of psychology, first through projective techniques into clinical psychology, then into experimental clinical psychology, and finally into experimental psychology proper [see 309]. Thus the original claim of comprehensiveness for this theory is gradually being realized.

If we must single out an outstanding limitation of this theory's claim to comprehensiveness, then we should choose its lack of a specific learning theory. Psychoanalysis has created grounds on which contemporary learning theories (Hull, Dollard, Miller, Mowrer, etc.) can be sharply criticized, and its conception of the primary process (e.g., the drive organization of memories) and of the secondary process (e.g., the conceptual organization of memories) can be regarded as foundations for a theory of learning. But like Lewinian and Gestalt psychology, it has failed to offer a specific alternative learning theory. Though Hartmann's [157] automatization concept seems to open a new approach to the problem of learning—as did K. Lewin's [221] ossification concept—so far no one has used it. The problem of learning—how a process turns into a structure, or in other words, the long-term survival and availability of experience—has not been solved by psychoanalysis either.

5. Quantification and mensuration. Psychoanalytic theory does contain quantitative considerations (particularly in its economic point of view), but the translation of these into actual measurements presents

²⁹ Hartmann, whose work is an indispensable link between Erikson's work and classic psychoanalytic metapsychology, laid the metapsychological groundwork for this unification but did not actually undertake it.

difficulties which have not been overcome. Some of these difficulties will be mentioned here and in Section V. below.

Freud did not take a theoretical stand implying unquantifiability;³⁰ yet neither he nor any other psychoanalyst tried to quantify the variables of the theory. However useful and indispensable the theory is clinically, however much light it sheds on a broad range of human phenomena, and however consistently everyday and clinical experience confirms its helpfulness, as a theory it requires exact tests of confirmation which in turn require the mathematization of the relationships posited by it. The obstacles to mathematization are: (a) The basic independent variable (drive cathexis in general, libido in particular) postulated by the psychoanalytic theory is an intrapsychic one, related to organic changes *and* intrapsychic structural conditions, rather than to external stimuli; thus it is hard to manipulate³¹ and measure. (b) The avenues through which such variables may exert their causal effect are multiple and interchangeable (cf. Tolman's vicarious function, Lewin's substitute tasks, and Heider's equifinality), and thus hard to predict, observe, and measure.³² (c) The distance between the theory's major variables and the observed phenomena makes it uncertain whether or not any measure obtained actually quantifies a particular variable.

But these obstacles need not prevent mathematization (e.g., quanti-

³⁰ We do know, however, that he occasionally took a practical stand to the effect that the theory needs no experimental confirmation [cf. Rosenzweig, 293]. I am indebted to Dr. Saul Rosenzweig for a personal communication which indicates that Freud, in a conversation with H. A. Murray and in one with R. Grinker, seems to have retracted this stand.

³¹ Not the least of the obstacles to manipulation is the inviolable privacy of the subjects.

³² The point is frequently made that Freud's failure to quantify his variables was due to his having come from "another tradition," and that the continued avoidance of quantification stems from the development of psychoanalysis "apart from academic psychology." True, Freud's neurological research was in the area of nonquantitative neuroanatomy. It is also possible that, as Holt [177] suggests, Freud's experience with Fliess's "numbers game"—combined with a general limitation in mathematical thinking, which he mentions repeatedly [94]—made him averse to quantitative considerations. Still, these arguments seem to miss the mark, and distract attention from the lack of quantitative methods applicable to intrapsychic variables. Academic psychology has only recently *begun* to be concerned with such methods. In addition, these arguments disregard that the Helmholtz tradition was the matrix of both Freudian and academic psychology, and that the biology of that time was not centered on quantification but rather on the significance of the single case and on the tracing of genetic connections. The theory of evolution seems to have been a "good science" even though the complex statistics applied to it by G. G. Simpson [311, 312] were not available to Darwin. In just what sense Darwin's and Freud's theories are good sciences is an interesting but so far—to my knowledge—unanswered question.

fication), though they do suggest that the road to it will be long and arduous. It is doubtful that the long hierarchic chain of intermediary concepts interposed between the major explanatory constructs and the observables can be bypassed, and that direct relations can be found between them. This highlights the importance of theory construction, since only a tightly built theory (with clearly stated definitions and implicative rules) can support confirming tests on observables which are at a great distance from the constructs: the models discussed above show that in this theory the implicative rules change with the distance from the basic variables.

Many features of observables can be counted, rated, and measured, but the observables alone cannot tell us which features and what method of counting or measuring them will reveal the relationship between them and the explanatory constructs: only theory can do that. A certain amount of trial-and-error (*ad hoc*) quantification is inevitable, but it will never yield a theory: theory is the product of theory-making. The confirmation or refutation of a theory requires that we quantify those features of the observables which correspond to the *dimensions* of the theory's variables. The *ad hoc* quantifications are not necessarily useless; they may be the means by which the deductions from the theory and the inductions from the observables are brought progressively closer to each other, and by which the essential measurables are progressively selected from the multitude of all measurables. But this selection cannot be achieved by blind measurement unguided by theory: there is no end to that.³³

The first steps toward quantification are (*a*) systematic mastery of the theory as it exists at present, (*b*) systematic attempts to tighten the theory, (*c*) the selection of measurables relevant to the variables of the theory. So far, no attempt at quantification has included these steps. Most of the experimenters who have attempted to confirm or refute the relationships posited by psychoanalytic theory were unaware of the *nature* of, and the variables involved in, the relationships which they set out to test.

This may seem to be a sad picture of the theory and a summary indictment of the experimenters who have tried to deal with it. Neither of these is intended. We are blinded by the rapid development of new sciences in our time. The rapid growth of biochemistry and biophysics was possible because they had the solid foundations of several thousand years of physics and chemistry. Some psychologists are bent on linking psychology to those sciences *now*, hoping for an equally spectacular growth of psychology. Others are more patient. They do not deplore the present state of the theory, nor consider the experimenters to be fools.

³³ For similar considerations in geology, see Rich [287].

In their eyes these difficulties are phenomena of a *very early phase* in the development of a science. Clinical observation shows that conscious information does not eliminate symptoms rooted in unconscious forces and that conscious intent is usually no substitute for the lack of unconscious motivation. Likewise, we may assume that consciously borrowed methodological sophistication, however much it may help otherwise in developing psychology, cannot circumvent the long and time-consuming process all sciences have gone through. The process of development which brings about the interplay between the observables and the theories is always slow.³⁴ Quantification and methodological sophistication are late products of any science and as such they should be long-range goals: mistaking them for proximal goals can render a science impotent.

6. Formal organization. The expositions of psychoanalytic theory have been informal rather than systematic; in the main they were directed by internal consistency within the theory and between observables and the theory. In the last twenty years attempts at systematic formulation [26, 73, 150, 157, 166, 167, 267, 268, 274] have been made, but no hypothetico-deductive system-building is in sight.

II. THE STRUCTURE OF THE SYSTEM

In order to discuss the systematic independent, intervening, and dependent variables of the psychoanalytic theory, it seems necessary to sketch the theory's structure.

A. The Subject Matter of Psychoanalysis Is Behavior (the Empirical Point of View)

This proposition has often been overlooked, probably because the theory's stress on unconscious processes and drives, psychological structures, dynamics and economics obscured the fact that it conceives of all of these as explanatory concepts of behavior.

³⁴ We have some idea why this process is so slow. If logic, methodology, and mathematics were the pacemakers of development in sciences, this development could be fast enough in psychology. But the pacemaker is not methodology—it is human invention. ("Developmental projects," "crash programs," and "interdisciplinary teams" are effective only in highly developed sciences or else in situations where the makeshifts of pooled ignorance are the most that can be had.) Methodology, since it deals with relationships of concepts, all of which are potentially valid, can go on continuously, building ever-new "castles in Spain." But human invention consists of discontinuous events, each of which requires long preparation, since in it an individual's thought patterns must come to grips with patterns of nature, and only those rare encounters in which a unique human thought pattern actually matches a unique pattern of nature will matter. If the match is not specific and precise, or if the individual is not prepared to recognize it, or if he does recognize it but is not ready to use it, the moment is lost.

Behavior in this theory is broadly defined, and includes feeling and thought as well as overt behavior, "normal" as well as "pathological," frequent as well as unique forms of behavior. This corollary too has often been overlooked, probably because of the stress in psychoanalytic literature on "latent behavior" and on pathology, both of which served as points of departure for the theory. Indeed, not before Hartmann's [157] major study (1939) was it directly stated that psychoanalysis is a general psychology which embraces the study of normal as well as pathological behavior,³⁵ though the principle of the *thoroughgoing psychological determination of all behavior* has been the cornerstone of the psychoanalytic theory from the beginning and was explicitly stated in 1905 [99].

Thus, all appearances to the contrary notwithstanding, psychoanalysis does not differ from other theories in its view of the subject matter of psychology (though it defines behavior far more comprehensively than most), nor in its assumption of determinism (though it probably demanded this earlier and in a more sweeping fashion). Yet it does differ from other psychologies in assuming *psychological* determinism, and in its stress on "latent behavior" in general and on the unconscious determinants of behavior in particular (cf. Section II. E., below).

B. Behavior Is Integrated and Indivisible: The Concepts Constructed for Its Explanation Pertain to Different Components of Behavior and Not to Different Behaviors (the Gestalt Point of View)

In the clinical parlance (and even in the theoretical writings) of psychoanalysis, the explanatory concepts are anthropomorphized, reified, or at best presented in existential terms, giving the impression that they refer to entities or at least that each of them refers to a specific behavior. But this is not consistent with the theory. The tendency to anthropomorphize and reify, and the preference for hypothetical constructs probably derives from clinical practice, where there is a premium on the "plausibility" and "uncomplicated everyday application" of concepts.

In concrete terms: no behavior can be described as an id behavior, or an ego behavior, or a conscious behavior. These concepts all refer to specific aspects of behaviors and not to specific behaviors. Every behavior has conscious, unconscious, ego, id, superego, reality, etc., components. In other words, all behavior is multiply determined (overdetermination). Since behavior is always multifaceted (and even the apparent absence of certain facets of it requires explanation), the conception of multiple

³⁵ It is noteworthy, though, that Freud's recently discovered manuscript [94, appendix], which is the predecessor of the theory of psychoanalysis, has the scope of a general psychology.

determination (or overdetermination) may be regarded as a purely formal consequence of this method of conceptualization. This naturally does not rule out the possibility that the conception of overdetermination is also required by the nature of the observations; in fact *overdetermination* as a concept was originally introduced [35, pp. 156, 219] in reference to observations,³⁶ rather than on purely theoretical grounds. From the very beginnings of psychoanalytic theory, observations made the concept of overdetermination both necessary and central. Academic psychologies did not develop such a concept, probably because their methods of investigation tend to exclude rather than to reveal multiple determination. But they did not escape the problem itself: every behavior phenomenon has perceptual, learning (memorial), conceptual (cognitive), motor, etc., components; and the rival psychological theories (perceptual theory of cognition, learning theory of perception, motor theory of thought, etc.) show both the presence of the problem and the confusion resulting from a failure to face it squarely.³⁷

³⁶ For example, when a subject executes the posthypnotic suggestion to shut a door and explains that he did so because of the draft [53], then his action is determined both by the hypnotic suggestion of which he is not conscious and by his conscious intention to escape the draft. Dr. A. B. Wheelis (San Francisco) suggests (personal communication) that there are distinctions among overdetermination, multiple determination, and multiple levels of analysis, which hinge on whether the determiners are *independent* and *sufficient* causes of the behavior in question (overdetermination) or not (multiple determination). It appears, however, that in psychoanalytic theory neither such *independence* nor such *sufficiency* of causes can be demonstrated or perhaps even defined. The fact that to escape the draft would be, under other conditions, a sufficient cause for shutting the door does not make it a sufficient cause in the posthypnotic situation. The matter of the "independence" of causes is an *autonomy* issue (cf. Section II. H., below). Overdetermination, to my mind, implies precisely such a lack of *independence* and *sufficiency* of causes and is inseparably connected with the multiple levels of analysis necessitated by this state of affairs. Mr. J. Zsoldos (Maabaro, Israel) suggests (personal communication) that the "overdetermination" issue crops up where "weak (sensitive) systems" are exposed to overwhelmingly large forces, that under such conditions simple functional relationships do not obtain, and quantitative analysis is possible only in terms of statistics; so that "weak systems" have only statistics, not "laws." This suggestion seems to imply that the overdetermination issue is the psychological counterpart of the controversy between Einstein's theory and present-day atomic physics. The psychoanalytic theory of overdetermination as it stands—if I read it correctly—implies laws and not statistics. To use Einstein's phrase, "The good Lord does not play dice" in this theory either. Nevertheless, the possibility of a statistical interpretation of overdetermination must be kept open, even if reluctantly; a specific and workable statistical interpretation would be preferable to an interpretation which assumes the existence of laws but does not specify any implicative rules and thus permits neither confirmation nor refutation.

³⁷ "Field theories" may be looked upon as attempts to meet this problem.

This psychoanalytic proposition has implications over and above that indivisibility of behavior from which the considerations pertaining to overdetermination stem. It requires that each of the conceptually differentiated aspects of behavior, as well as the spatial and temporal context of behavior, be treated as an integrated whole. But we need not pursue this point further: it seems to coincide grossly with the general postulate of Gestalt psychology.

C. No Behavior Stands in Isolation: All Behavior Is That of the Integral and Indivisible Personality (the Organismic Point of View)

This thesis demands that the explanation of any behavior fit into the theory of the workings of the total personality.³⁸ Freud's most direct statement of this thesis is probably that pertaining to dreams. Once he had developed the theory of dreams, he raised the question: what kind of theory of personality could embody this dream theory. In Chapter 7 of *The Interpretation of Dreams* he proceeded to construct the framework of such a theory of personality [98, pp. 469, 470, 485-486].

Yet this implication of psychoanalytic theory, too, has been overlooked by many psychoanalysts and psychologists, probably because the stress on the central role of drives made it appear to the psychoanalyst that the fundamental drives sufficiently guarantee the unity of behavior and personality, and gave the psychologist the impression that in this theory the "atomistically" conceived behavior fragments are held together only by the "glue" of the drive concept. The organizing, integrative role of the secondary process (1900), however, speaks eloquently against both of these views [98, pp. 533-536], and the "structural point of view" (ego, id, superego, etc.), which clearly embodies principles of cohesiveness other than drives [126, pp. 15-18; see also Nunberg, 245, and Erikson, 63], should have dispelled these misconceptions. It did not.

To be sure, it is easy to find passages in Freud which lend themselves to atomistic interpretation, but the theory itself does not. French [87, 88, 89] devoted his major work to demonstrating the role of the integrative field in psychoanalytic considerations. Psychoanalytic studies in psychosomatics embraced Goldstein's organismic view as "consistent"

³⁸ It may be objected that Freud did not *explicitly* formulate the organismic point of view and that only the organismic biologists and Wertheimer, Goldstein, and Wheeler arrived at it. But our task here is not limited to a collation of Freud's explicit systematic formulations. Recognition of the organismic thesis of psychoanalysis is the more important, since Gestalt as well as personalistic psychologists viewed psychoanalysis as an atomistic and mechanistic theory. Wertheimer was vehement about this in his lectures and conversation, and G. Allport [7] outspoken in his writings. The attitude of many practicing psychoanalysts in regard to symbols, dream interpretations, etc., to the contrary notwithstanding, this thesis appears to be a basic implication of Freud's theory.

with psychoanalysis. The projective techniques, which developed under the impact of psychoanalytic conceptions, borrowed from psychoanalysis the projective postulate [259, 261] that all behavior is integral to, and characteristic of, the behaving personality. Nevertheless, this implication of psychoanalysis remained so remote that French [85] in 1933 (before his familiarity with K. Lewin), and later Mowrer [239], as well as Dollard and Miller [48], found it feasible to link psychoanalysis to the atomistic conditioned-response theory of learning.

What this organismic point of view asserts is not that each behavior is a microcosm which reflects the macrocosm of the personality, but rather that an explanation of behavior, in order to have any claim to completeness, must specify its place within the functional and structural framework of the total personality and, therefore, must include statements about the *degree* and *kind* of involvement, in the behavior in question, of all the relevant conceptualized aspects of personality.

D. All Behavior Is Part of a Genetic Series, and through Its Antecedents, Part of the Temporal Sequences Which Brought About the Present Form of the Personality (the Genetic Point of View)

This thesis implies that every behavior is an epigenetic product [58] and thus can and must be studied genetically for its full explanation [166]. However, it implies neither a view of behavior as the "maturation" of a preformed behavior repertory, nor one according to which behaviors "develop" from accumulating experience; rather, it views behavior as the product of an epigenetic course which is regulated both by inherent laws of the organism and by cumulative experience.

The genetic point of view does *not* conflict with K. Lewin's insistence that only forces and conditions which are *here and now* present can in the here-and-now exert an effect;³⁹ it asserts simply that much of what "exists" here and now in the subject can only be known through a genetic exploration of its antecedents. This implies that descriptively identical behaviors may differ in their psychological significance, depending on their genetic roots. But it also implies that the empirical relevance of a behavior to a situation in which it occurs alone does not necessarily explain it and that the explanation must also take into consideration the epigenetic laws which brought the behavior about. Indeed, it is peculiar that it should have been Lewin who criticized the genetic point of view, when he more than any other psychologist stressed the distinction between genotype and phenotype and sharply criticized the use of achievement concepts. He gave the example: identical typewriting speeds

³⁹ Nor does it clash with Lewin's [218] and Chein's [43] point that the past reconstructed by the patient in psychoanalysis is the past as he views it in the present.

of applicants for a job provide insufficient information, since they may be products of maximal exertion or routine approach, disuse or peak efficiency, recent training or established working level [220, pp. 89-91]. We must conclude that without the exploration of its genetic antecedents, a behavior can only be described in terms of achievement concepts.

The genetic point of view refers to the history of the drive processes which express themselves in a given behavior, to the history of the structures (e.g., those corresponding to "abilities") used in it, and to the history of the subject's relation to the situation in which the behavior occurs. An example of the distinctions implied here: a sudden attack of stammering, which is brought about by defense against an aggressive impulse. A genetic exploration will take into consideration those past experiences, and the controlling structures crystallized from them, which modulated the development of the aggressive drive and thus gave the power to arouse aggression to situations like the one which aroused the subject's anger in this instance. It will also consider the past experiences which led to defense against aggression in general or against that particular kind of aggression which came into play in the given situation. It will extend to those past experiences which made the verbal avenue for the expression of anger particularly vulnerable to defense and to those past events which shaped the stammer, that is, the form which defense took in this situation. In each of these instances, experience denotes both the historical event in its external setting and the internal situation of the subject, including the specific phase of his maturation and development.

Although the genetic point of view does not refer specifically to the contextual (spatial-temporal) determination of behavior, it does imply contextual determination. Moreover, it does specifically refer to the intrapsychic context: to the contemporary state of the personality as a whole and as a genetic product.

The genetic character of the psychoanalytic theory is ubiquitous in its literature. The concept of "complementary series"⁴⁰ is probably the clearest expression of it: each behavior is part of a historical sequence shaped both by epigenetic laws and experience [101, summary; 279]; each step in this sequence contributed to the shaping of the behavior and has dynamic, economic, structural, and contextual-adaptive relationships to it. Such complementary series do not constitute an "infinite regress": they lead back to a historical situation in which a particular solution of a drive demand was first achieved, or a particular apparatus was first put to a certain kind of use [cf. 166].

But this formulation is incomplete because it disregards those observations to which the concept of autonomy refers [157, 161, 280]. Cer-

⁴⁰ *Ergänzungsreihe*, see Freud [121, lecture 21] and Fenichel [73, pp. 121ff.].

tain behaviors do (all tend to) cease to be shaped further by their recurrence: they become automatized [157] and relatively autonomous from their genetic roots; they take on a tool- or means-character and attain a high degree of stability. However, automatization and autonomy make not only for stability, but also for the availability of the behavior as a means of adaptive performance. These automatized behaviors can also be studied genetically, but from that point at which they become automatized, their "complementary series" proves relatively unrevealing, since from there on situation and context may bring them into action, even in the absence of the motivations which gave rise to them originally. In Section II. H., pp. 93-97, we return to the concepts of automatization and autonomy. They are akin to Woodworth's [329, pp. 100ff.] concept of "habits as drives," and to Allport's [6; 8, pp. 76ff.] concept of functional autonomy, but they are more specific and more differentiated than either of these.

Psychoanalysis as a genetic psychology deals with the genetic roots of behaviors, with the degree of autonomy behaviors attain, and with the genetic roots of the subject's relation to the reality conditions which codetermine the appearance of a behavior at a given point in the person's life. Yet the first formal statement of the genetic point of view of psychoanalysis is that of Gill and Rapaport [149].

E. The Crucial Determinants of Behaviors Are Unconscious (the Topographic Point of View)

Per se, this thesis is not alien to any psychology, and particularly not to those psychologies which exclude all phenomena of consciousness from their subject matter, and thus *have to* assume that the determinants of behavior are extraconscious. All psychologies deal with conditions "unnoticed" by the subject, and with "unnoticed" or "unnoticeable" processes underlying his behavior. The psychoanalytic thesis of unconscious determination, however, differs from these [98, pp. 543-544; 110] in several respects: (1) it explicitly conceptualizes that which is unnoticed or unnoticeable [110]; (2) it asserts that the unnoticed or unnoticeable can be inferred from that which *is* noticed by the subject (and/or the observer), by means of the effects of the unnoticed and/or the unnoticeable upon that which is noticed [99]; (3) it asserts that the rules governing the *noticed* are different from those governing the *unnoticed*, and that the unnoticed can be inferred by considering the deviations of the noticed from its usual patterns [117, pp. 118-122]; (4) it makes a systematic distinction between the *unnoticed* and the *unnoticeable* (the *unnoticed* can become conscious, whereas the *unnoticeable*, by definition, cannot); it expresses this distinction by the terms "descriptive" vs. "dynamic" unconscious, and conceptualizes it as the distinction between

the Systems *Preconscious* and *Unconscious* [98, pp. 543–544]; (5) while other psychologies treat the unnoticeable in nonpsychological terms (brain fields, neural connections, etc.), psychoanalysis consistently treats it in the psychological terms of motivations, affects, thoughts, etc.

The differences between the laws governing the Conscious and those governing the Unconscious are expressed in the concept of primary and secondary processes.⁴¹ In the early phases of the theory, the *Cs* and the *Ucs* were considered systems of paramount significance. Later on (1923) they were subordinated to the structural conception id-ego-superego [126], and still later (1938) were relegated to the role of “qualities” [140].⁴² In keeping with Freud’s early formulations [35, 98], recent contributions to ego psychology treat consciousness as a superordinate sense organ. They attribute to it a complex hierarchic layering (states of consciousness) [268, 270, 276], and thus treat it on a level of abstraction different from that accorded the unconscious.

F. All Behavior Is Ultimately Drive Determined (the Dynamic Point of View)

This thesis of psychoanalysis has become only too well known in a doubly distorted form: all behavior is determined by sex. The qualifier “ultimately” was omitted, and sex, libido, drive, and psychosexuality were equated. It is certainly true that, until recently, the drives most closely studied by psychoanalysis were the sexual drive and its partial drives. But psychosexuality was defined in such a broad way that it was by no means synonymous with “sex” [101]. Self-preservative and ego instincts were also discussed early [101, 114, 115, 121], but were dropped later since they did not prove helpful in organizing empirical evidence. The history of the theory of drives (narcissism, instinctual vicissitudes, life and death instincts, monistic drive theory, aggressive drive) suggests that the early centering on libidinal drives helped Freud to explore the nature of drives and their motivational role [115], but did not settle the theory of drives itself [26]. In spite of some recent advances [160, 168], it is still unclear how many and what kinds of drives need to be postulated.

The crucial role attributed to libidinal drives is not a theoretical necessity in this system. It seems to derive from two of Freud’s major achievements: the conception of the determination of behavior by drives and the observation of infantile sexuality. The fact that the theory linked these two to each other very early may have retarded a full assessment of the role of libidinal drives in psychological life.

⁴¹ The relationship of Conscious vs. Unconscious to primary vs. secondary processes is not, however, a one-to-one coordination.

⁴² Section I. B. 3. *a.* presented the issues discussed here from another angle.

Infantile sexuality was an empirical discovery made by the method of psychoanalytic reconstruction and verified by the method of direct observation on infants and children. Though of empirical origin, it grew to systematic importance because it provided the example for the development of drives [101].

The broader principle of drive determination was an empirical as well as conceptual discovery. The empirical discovery embodies, besides its novel aspect, two familiar observations: (1) behavior is not always triggered by external stimulation but often occurs without it, as though spontaneously; (2) behavior (which by scientific fiat is causally determined) evinces a goal-directedness, a purposive, teleological character. The conceptual discovery, which took the form of the definition of the drive concept, was the first large-scale attempt to cope with both of these observations simultaneously. The drive is defined as a causal agent inherent in the organism [115, p. 64], and thus it can account for the apparent "spontaneity" of behavior. Moreover, since the definition makes the effectiveness of the drive dependent on an environmental condition, namely, the presence of the drive object, it can also account for the purposiveness of behavior. This coordination of drive and drive object—which is assumed to be guaranteed by evolution—at first tolerates little if any means-activity and demands immediate consummation (pleasure principle). In the course of development, it becomes more flexible, and permits delay and interpolation of means-activities, though it selects and organizes these in the service of consummation. Later on it permits substitute goals and a variety of means- and consummatory-activities, until finally it prescribes only the consummatory behavior, and provides no more than the motivational framework for instrumental behavior. This conception of motivation accounts not only for the spontaneity and teleology of behavior, but also for behavior elicited by external stimulations, since the latter may be conceived of as drive objects, or substitutes for them.

If psychological theories can be divided into two classes according to whether they consider the human psyche a *tabula rasa* on which experience writes, or an organization of actualities and potentialities which limits and regulates the extent and kind of changes that experience can bring about, then the drive conception certainly belongs to the latter class. In philosophical systems this distinction is crudely paralleled by Hume vs. Kant [cf. 9, pp. 7ff.; 252, 254, 262]. In terms of psychology the distinction is paralleled, for instance, by the conception of passive registration of experience vs. active organization of it, a distinction which involves the nurture-nature controversy. Psychoanalysis was one of the first theories to recognize the interaction of nature and nurture in the development of behavior. Drives represent the "nature" factor; and

their vicissitudes, in the course of experience, the interweaving of nature and nurture. Moreover, the coordination of drive and drive object expresses a primary coordination given by evolution between human nature and its environment and is thus a psychological representation of the biological adaptedness of the species to its environmental, ecological niche.

Finally we come to the “*ultimate* determination of behavior by drives.” Here we meet relationships like those connected with the “genetic point of view.” While early psychoanalysis actually maintained, without reservation, the thesis of “*ultimate* drive determination,” the increasing evidence for the “indivisibility of behavior” led to the realization that behavior, in so far as it can be said to be determined by drives, must also be said to be determined by defenses and/or controls. Moreover, with the development of ego psychology, the question was no longer which of these was *the* ultimate determiner of behavior but rather in what respect and to what extent was *each* the determiner of a given behavior [cf. Waelder, 320]. Finally, behaviors were encountered in which drive determination was in abeyance. This led to the concept of ego autonomy (cf. Section II. H.).

Thus the thesis of the ultimate drive determination of behavior, while it remains valid in psychoanalysis, must be regarded in the context of the other theses here discussed, which qualify it and limit its scope. The concepts of drive fusion, drive differentiation into partial drives, conflict, etc., all pertain to the dynamic point of view and indicate limitations to the conception of ultimate drive determination.

G. All Behavior Disposes of and Is Regulated by Psychological Energy (the Economic Point of View)

This thesis, too, has a history. In the first phase of psychoanalytic theory (abreaction theory—up to 1898), psychological energy was equated with affects, and the “defenses” which prevented abreaction were not conceptualized in economic terms [35, 98]. In the second phase (1900–1926), psychological energy was conceptualized as drive energy, and the methods used in discharging it as the primary process. It was recognized that other (secondary) processes, using minute quantities of energy, exert a regulative function over those which dispose of drive energies [98, particularly chap. 7; 108, 116, 117, 119, 120]. The relationship between these two kinds of processes was conceived much like that described nowadays as obtaining between power engineering and information engineering [cf. Wiener, 326, pp. 53–56; Rapaport, 264]. In this phase, however, little attention was paid to the nature and origin of the secondary process. In the third phase (after 1926), some

understanding was gained of the secondary process, of its congenital roots, and of the progressive ontogenetic transition to it from the primary process [131, chap. 8, especially pp. 82, 112–117; cf. also 274].

All behaviors have both primary process and secondary process aspects, though one or the other may predominate. The primary process operates with drive energies, and its regulative principle is the tendency toward tension reduction (pleasure principle):⁴³ it strives toward immediate discharge of energy accumulations, by a direct route and by means of the mechanisms of displacement, condensation, substitute formation, symbolization. The secondary process operates by the principle of least action, is oriented toward objective reality, and finds, through delays and detours, by experimental action in thought, the safest course toward the sought-for object in reality, suspending the discharge of drive energies until the object has been found [98, pp. 533–536; 108].

In the course of development, hierarchically layered structures arise (defenses and controls) which act as “dikes.” These not only delay or prevent discharge, but also diminish the drives’ tendency toward immediate discharge. These structures are conceived of as built by “binding” drive energies to heighten the originally given drive-discharge thresholds [98, pp. 533–534; 116, 117]. Their effect of diminishing the drives’ tendency toward immediate discharge is conceptualized as “neutralization,” special instances of which are referred to as delibidinization, deaggressivization, or sublimation [126, pp. 61–65; 206, 164]. These processes of binding and neutralization make cathexes (hypercathexes, attention-cathexes) available to the secondary process, to be used in small quantities for experimental action in thought [108, p. 16], as signals in the form of affects [98, p. 536; and 131], and as counter-cathexes (against drives) for building new and for reinforcing existing defensive structures. Once a process of “neutralization” is assumed, the original dichotomy of primary and secondary processes yields to a hierarchic model in which these two represent theoretical extremes and the actually observed phenomena represent intermediary forms [206; 268, e.g., p. 536]. The energies of lesser degrees of neutralization (drive derivatives) show characteristics of their drive origin, whereas those of higher degrees do not, and are at the disposal of the ego.⁴⁴ Sources of

⁴³ We will not discuss here the distinction between an “optimal” and a “maximal” lowering of tension. The latter has been assigned to the so-called “nirvana principle” (associated with the “death instinct”)—a speculative excursion which does not seem to be an integral part of the theory. Cf. Freud [123], Fenichel [74], and Hartmann, Kris, and Loewenstein [168].

⁴⁴ While the ego builds new or employs existing defenses against drive derivatives of low neutralization, it can make use of derivatives of high neutralization, since it can deal with these by means of its controls. This difference seems to be akin to the difference between all-or-none vs. graduated processes.

neutralized energy other than drives are also compatible with the theory [161, pp. 86–87; 162, p. 21].

These psychological energies are not equated with any known kind of biochemical energy. They do not correspond to the muscular energy expended in overt behavior. The differences in the quality (mobility vs. degree of neutralization) of psychological energy⁴⁵ correspond to the observed differences between overvalent thoughts (e.g., obsessions) and passing thoughts (e.g., logical thinking), between impulsive or compulsory actions and actions which are a matter of choice. There is an obvious conceptual similarity between Freud's energies and Lewin's tensions and between Freud's drives and drive objects and Lewin's forces and object valences, though there are also significant differences between them. They are alike in that they cannot be expressed in the mathematical formulas in which physics expresses its energy concepts, yet they are referents of phenomena which seem to abide by the laws of energy exchanges—conservation, entropy,⁴⁶ least action. (However, it is neither implied nor ruled out that biochemical energy exchanges may eventually be discovered which correspond to the exchanges of psychological energy inferred from behavior by psychoanalysis.) They differ, among other things, in that Lewin's concepts do not account for the differences in quality here discussed.

From the point of view of the energy economy of the organism, the exchanges of psychological energy may be considered as the work of an information engineering network which controls the biochemical energy output of overt behavior. But this network itself is multiply layered, so that ever smaller quantities of energy control the networks which carry and dispose of greater quantities of energy [cf. Wiener, 326]. For further discussions pertinent to the economic point of view, see Sections I. B. 3. *b.* and V. A.

H. All Behavior Has Structural Determiners (the Structural Point of View)

The simplest way to put the issue of structure is to point out that drive energies can be conceived of only within well-defined systems which have definite thresholds of discharge. The structural conception could well have been first necessitated by the observations pertaining to such discharge thresholds; and the prototype of the conflict between

⁴⁵ Speaking about the "quality" of energies does not contradict the fact that energy is a quantitative construct. Physics too speaks of different kinds of energies: heat, light, etc. But in psychology we do not yet have transformation equations to express the quantitative relationships of these qualities to each other.

⁴⁶ The validity of the economic point of view is unaffected by Bertalanffy's conception of "open systems" [20].

drives and structures could have been the relationship between drives and thresholds. Actually, however, this was not the origin of the structural point of view, even though the early (1895) neuropsychological form of the theory would allow such an interpretation [94, appendix]. Recently (1939) Hartmann [157; see also 162] pointed out that not only thresholds but ego apparatuses like memory, perception, and motility are also structural givens.⁴⁷ But the structural point of view did not originate in these structural givens either.

It was observed that drives do not unequivocally determine behavior in general, nor symptom formation in particular. In contrast to the drive processes, whose rate of change is fast and whose course is paroxysmal, the factors which conflict with them and codetermine behavior appeared invariant, or at least of a slower rate of change. The observation of these relatively abiding determiners of behavior and symptom seems to have been the foundation on which the concept of structure was built.

In the first phase of the theory (up to 1900), reality was considered the factor which interfered with the drives, through the ego (consciousness) in general and through its defenses in particular [96, 97]. But this view did not reach a conceptual status at the time and was superseded (1900) by the conception of intrapsychic censorship exerted by ego (self-preservative) instincts. A conception of psychological life as a continuous clash of drive forces arose [98], and the abiding character of the interfering factors was lost sight of. Not even the link established between the concepts of censorship and the secondary process [98, 108, 117] conceptualized the abiding character of these two drive-controlling factors. Instead, Freud again (1911–1917) became concerned with the role of reality, and considered it to be the factor which interferes with the drives and becomes a codeterminer of behavior. Yet he still assigned the reality-testing functions to the secondary process *and* to the ego drives [108, 121]. But the further study of censorship (particularly of its repressive function) and of the secondary process shed additional light on these interfering factors: they were now conceptualized as counter-cathexes, which delay the discharge of drive cathexes, and by their permanent deployment, prevent the return of the repressed [116, 117]. This formulation of a permanent deployment of counter-cathexes is the beginning of the structural conception.

An explicit formulation of the structural conception became necessary when it was realized that not only the drives but also most of these invariant factors which interfere with drives are unconscious [126]. The

⁴⁷ The structural givens in question are not the muscular apparatuses of motility, nor the end organs of perception, etc., but rather their psychological regulations: for instance, those psychological structures through which the control and triggering of the motor apparatus is effected.

topographic conception, which relegated all crucial determiners (drives) of behavior to the Unconscious and all epiphenomena and "apparent determiners" (ego) to Consciousness, became systematically untenable, and was replaced by the structural conception. The topographic division into the Systems Unconscious, Preconscious, and Conscious yielded to the structural conception of id, ego, and superego. The "ultimate drive determiners" were conceptualized as id, the codeterminers (whether conscious or not) as ego, and a specialized segment of the ego as superego.⁴⁸ Whereas the id was conceived of as the congeries of drives (coexistent even if contradictory), the ego was defined as a cohesive organization, whose function was to synthesize the demands of id, superego, and reality [126]. The ego was conceived of as a structure which codetermines (along with the drives) every behavior, and is responsible for the coordinated and organized character of all behavior, including specific drive-discharge actions (e.g., sexual intercourse). But the ego as a structure proved so complex that its exploration is even now only just beginning. The recognition of the structure-building and structural role of identifications [126] was followed by the recognition of the role of the ego's defensive substructures [131]. In addition to these two kinds of substructures, psychoanalytic ego psychology also came to recognize orienting (perceptual), processing (conceptual), and executive (motor) substructures, when it was realized that they are ready tools (means) available to ego processes [157, 266].⁴⁹

To begin with, psychoanalytic theory assumed that all psychologically relevant structures arise in ontogeny. But at present some of these structures are considered to be congenitally given. This shift has two implications: *first*, that such constitutionally given apparatuses as motility, perceptual system, memory system, thresholds⁵⁰ are psychologically relevant; *second*, that the ego does not derive from the id, but rather both emerge from the common undifferentiated matrix of the first extrauterine phase of ontogenesis [cf. Hartmann, Kris, and Loewenstein, 167].

While originally all structures were considered to be related to drive and conflict, it is now assumed that the inborn ego apparatuses enter conflicts as independent factors and that their function is not primarily dependent on drives: thus they are termed *ego apparatuses of primary*

⁴⁸ Of these three major structural concepts, in the following we shall discuss only the ego. The structural treatment of the id and superego is still so inadequate that the lengthy discussion it would require is beyond the scope of this presentation.

⁴⁹ The fact that certain ego structures (e.g., defenses) have cognitive representations does not contradict the distinction made here between defensive structures on the one hand and cognitive (means-) structures on the other.

⁵⁰ Cf. note 47 above.

autonomy. [Cf. Hartmann, 157; see also, 266, 280.] This does not imply that they have no relation to drives. They are part and parcel of the apparatus which executes drive actions: they are the only means of action the organism has. Nor does it imply that they are forever free of conflict: under certain conditions they can and do become involved in conflict, as does the motor apparatus in functional paralyses and the perceptual apparatus in the tubular vision of hysterics. Thus even the apparatuses of primary autonomy are only relatively autonomous from drive and conflict. But their autonomy does imply: *first*, that drives only trigger their function and do not determine their course; *second*, that they can and do function even when they do not serve the gratification of a specific drive. Yet reservations must be made on both of these points: *first*, while it is true that the role of drives in relation to these apparatuses is primarily that of triggering their function, there is evidence to show that the drives also have other effects on the apparatuses (e.g., the effects of motivation on memory [258]); *second*, the problem of the energy supply of these apparatuses (when they are not triggered by drives) has so far not been satisfactorily solved [161, 206, and 268, particularly part 7]. Woodworth's [329] conception of "habit as drive," Buehler's [42] "pleasure in functioning," Piaget's [254] "circular reaction," Allport's [8] "functional autonomy" imply the same problem. Attempted solutions either attribute drives (or partial drives) to apparatuses, or consider apparatuses as sources of (neutral) ego energy, or assume that the energy they use is neutralized drive energy at the disposal of the ego.

Psychoanalytic theory at first considered the structures which arise in the course of ontogeny as conflict-born—i.e., defensive. Since defenses are central to psychoanalytic therapy, they are the most extensively studied structures, and this gives the impression that all structures are conflict-born and all controls are defenses. Even though the role of identifications in building ego structures was recognized early [126], a tendency persists to consider this kind of structure-building, too, as conflict-born. There is no theoretical clarity even now on this point [see, however, Erikson, 66]: certain identifications definitely arise from conflict (e.g., identification with the aggressor); others do not seem to. But it is clear that means-structures born in, or used in, the course of drive-gratification, or in the course of a defensive battle against drives, or in the course of resolving a conflict can and often do undergo "change of function"⁵¹ and become means of action and adaptation in the service of the ego. These are termed *structures of secondary autonomy* [157, 162]. They, too, are only relatively autonomous in the same sense as are the apparatuses of primary autonomy. They, too, are assumed to have

⁵¹ See Hartmann [157, 162]; for instance, rationalization is a defense mechanism which tends to undergo a "change of function" and thus to become an important means of adaptation, as a crucial ingredient of logical thought and rational action.

neutral energies at their disposal or to use neutralized energies at the disposal of the ego. The observations to which the concept of secondary autonomy refers are akin to those which led to Allport's concept of "functional autonomy": they pertain to the relation of means-structures to basic as well as higher-level motivations, attitudes, values, etc.

In conclusion:

1. The structural determiners of behavior were introduced as intervening variables to account for the observation that motivations do not determine behavior in a one-to-one fashion.
2. Structural determiners differ from motivational determiners in that they are relatively permanent: their rate of change is relatively slow.
3. There are inborn structures and acquired structures: apparatuses of primary and secondary autonomy.
4. Structure-building transforms motivations and thus gives rise to new (more neutralized) motivations.
5. Structures built, and the motivations arising from them, may become relatively autonomous determiners of behavior.

I. All Behavior Is Determined by Reality (the Adaptive Point of View)

Reality in psychoanalytic theory designates the external source of stimuli, including the subject's body, but excepting the somatic sources of drives and affects [115, pp. 60-64]. In this theory *reality* (i.e., *external reality*) is the antithesis of *psychological reality*⁵² [98, pp. 548-549].

This thesis of psychoanalytic psychology has undergone perhaps more metamorphoses than any other, and its implications are far-reaching. It implies, from the point of view of psychology, the question of the role stimuli play in behavior; from that of biology, the question of the relationship between the organism and its environment; from that of philosophy, the epistemological question (i.e., how man can know of, and act in accord with, his environment when his thoughts and actions are determined by the laws of his own nature).

In psychoanalytic theory's first conception, reality was considered the target of defense [94, ms. H; 96, 97]. More precisely, the defense was directed against *the memory of a real event*, in order to prevent the recurrence of the attendant feelings (affects) which, being socially prohibited, were incompatible with *self-respect*.⁵³ Thus symptoms (i.e.,

⁵² The corresponding adjectives are *real* and *psychological*.

⁵³ *Self-respect* and *social prohibition* did not attain conceptual status in psychoanalysis proper. They cropped up in the early superego theory [114], and returned in the Neo-Freudian schools. Only recently did ego psychology begin to work out their place in the theory [Hartmann, 161; Bibring, 27; Erikson, 66; Jacobson, 190, 192].

pathological behavior) resulting from defense (repression, etc.) were considered to be ultimately determined by reality events. But the question of reality's role in determining normal behavior was not yet raised, although it was assumed that the affect of nontraumatic experiences is "dissipated" by being distributed over a wide associative network [cf. 35, pp. 7-8; and 120] while the affect of traumatic experiences is "dammed up."

The second conception of reality [98], which dominated psychoanalytic theory from 1900 till 1923—with the exception of Freud's "Two Principles" [108], which prepared the next conception—had two aspects: these were *the drive object* and *the secondary process*.

Drive was conceived of as an internal stimulus [115, 119] which, unlike external stimuli, is continuous and inescapable through flight, a stimulus for which the organism has no intensity-reducing barrier of the sort which operates in regard to external stimuli. In turn external stimuli were accorded little significance and psychological effectiveness, and no behavior-determining role. Yet at the same time certain patterns of external stimuli, namely, drive objects, were conceived of as the precondition for drive action (drive discharge). Thus the effectiveness of drives, as the ultimate determiners of behavior, remained in part dependent on the availability of the drive object. Nor is this the only role reality plays in this theory: configurations of reality which prohibit drive action were considered to be represented intrapsychically by the censorship [98]. This is a drive-centered conception of reality: it comprises only the conditions which make drive action possible or impossible. One feature of this drive-object conception of reality has a broader significance. While the instincts of animals on lower evolutionary levels appear to be directly and more or less rigidly coordinated to specific external stimuli, the instincts of animals on higher evolutionary levels appear to be less rigidly coordinated to such specific stimuli. This difference may be characterized as a progressive internalization of the regulation of behavior.⁵⁴ The psychoanalytic theory of drives assumes that the relation of human drives to their drive objects is flexible, and that the regulation of human behavior is to a large extent internalized [see 101]. Though early psychoanalytic theory may at times have given the impression that the organism is totally autonomous from its environment, it was never so blind as to take this extreme stand. But it certainly does raise the question of the organism's *relative* autonomy from its environment [cf. pp. 95-97, above; see also Gill and Brenman, 148; and 280], and does make it clear that any explanation of behavior must come to grips with the relative autonomy of behavior from both drives and external reality.

⁵⁴ This internalization is considered coterminous with the establishment of the ego; cf. Hartmann [157, 160].

The other aspect of the conception of reality in this phase of psychoanalytic theory was the secondary process. According to this theory, the secondary thought processes tend to reflect reality "truly," i.e., not merely in terms of the drive whose object is to be reached, but in terms of the "actual" relationships between objects which obtain in reality [98, pp. 509, 533-536]. Likewise the secondary processes of action are "adaptive to" reality [108, pp. 15ff.] and do not strive blindly toward drive discharge. Delay of discharge, detour for the sake of a safe path, "full" availability of memories and their use in the experimental action of thought characterize the secondary process, which is thus not "selective" in the limiting sense that the primary process is but has a broad access to reality over which it exercises selective judgments and choices. This conception implied an "objective" reality, and secondary processes which, unlike primary processes, do not "distort" but are "veridical," even though it was clearly recognized that the secondary process cannot fully reduce these "distortions" because to do so it would have to eliminate the affects which it needs as its orienting signals [98, p. 536]. This conception remained incomplete, since it left the origin, nature, and function of the secondary process unexplained [compare, however, Freud, 129, 130; also Ferenczi, 78, 79, 80, 81].

The third conception of reality appears in Freud's ego psychology, of the 1923-1938 period, and was forecast in the "Two Principles" [108], particularly by the concepts of *reality principle* and *reality testing*. In the first conception, the defense was directed against reality and the memory of real events. In the second conception, it was directed against the drive, and reality had only a peripheral role. In the third conception, reality and drive appear to gain a more or less equal status [131]. Now the ultimate motive (determiner) of defense is real danger, and the drive is defended against because if it were acted upon it would again lead into a dangerous real situation. Thus defenses against drives come to represent reality and, as constituents of ego and superego structure, they become internalized regulations of behavior.

In the period we are now considering, the ego was still regarded as a mainly defensive organization; nevertheless its origin in identifications⁵⁵ and its most general definition⁵⁶ point to its other functions and to its intimate relation to reality. The identifications with the objects of social reality imply that reality has not only a defensive-conflictful role, but also an ego-structure-forming role. Moreover the ego, conceived as a cohesive organization with a synthetic function of its own [131, pp. 25-26; 245], gains a degree of independence from drives which permits a

⁵⁵ "The ego is a precipitate of abandoned object cathexes" [126, p. 36], i.e., of identifications with abandoned objects.

⁵⁶ "[The ego] is a coherent organization of mental processes" [126, p. 15].

relative objectivity in regard to reality. The relationship of the ego to reality brings into sharp relief the central role of reality in this phase of the theory: the ego's function is to reconcile the demands of the id, superego, and reality [126]; the relation to reality is crucial to the ego [137]; and the ego is organized around the System Perception-Conscious, i.e., around the means of contact with reality [126].

In this conception, reality shapes not only the ego, but even the drives,⁵⁷ which were previously conceived of as unchanging. Moreover, in Anna Freud's [93, pp. 96, 109–110] conception, the defense against reality itself again appears as a concept, much as it appeared in Freud's first conception of reality.

The fourth conception of reality—Hartmann's—is a radical development: the organism, as a product of evolution, is born already adapted, or potentially adapted, to reality. The ego apparatuses of primary autonomy are instruments of and guarantees of man's "preparedness for an average expectable environment." In animals of lower evolutionary levels the instincts are the guarantees of reality adaptedness; man's drives have lost much of this role, and thus inborn adaptedness is with him more a potentiality than an actuality; processes of adaptation outweigh inborn adaptedness. This potentiality for internalized regulation of behavior actualizes in the course of the development of the ego, which thus becomes man's organ of adaptation.⁵⁸

In this conception reality and adaptedness as well as adaptation to it play a much more extensive role than in Freud's third theory [cf. Kris, 205; also Hartmann, 165]: here they are the matrix of all behavior. Hartmann's concepts of *relative autonomy*, *secondary autonomy*, *automatization*, and *neutralization* for the first time provide a framework for understanding the development and the function of the secondary process as one of man's major adaptative means. But Hartmann goes even further and conceives of the reality to which man adapts as one created by him and his predecessors. Yet even this conception seems to retain an essential duality of psychological and external reality.

The fifth conception of reality, foreshadowed by both Freud's third conception and Hartmann's, is the psychosocial one developed by Erikson [61, 66]. Man is potentially preadapted, not only to one average expectable environment, but to a whole evolving series of such environments. These environments to which man adapts are not "objective," but rather social environments which meet his maturation and development half-way: social *modalities* (e.g., the socially accepted forms of "getting") foster, select, and harness his developing *modes* (e.g., the incorporative

⁵⁷ See [139], but note this point already in [117].

⁵⁸ Cf. Hartmann: *Ego Psychology and the Problem of Adaptation* [157]; see also [160].

oral mode) of behavior [62].⁵⁹ This is the genetic counterpart of Hartmann's systematic formulation; it is thus far the only attempt to conceptualize the phases of epigenesis [58, 61] through which preadaptedness becomes effective, and in which processes of adaptation inseparably unite behavior epigenesis and environmental conditions [61, particularly chap. 7]. The conceptions of an "objective" reality and of an unselective and "veridical" secondary process disappear here and even the major (time and space) coordinates of reality become "subjective" [63], in the sense that they are shown to be relative both to organismic space and time and to the particular society into whose reality man grows.⁶⁰

J. All Behavior Is Socially Determined (the Psychosocial Point of View)

To demonstrate that psychoanalysis considers real experience in general and social experience in particular to be determiners of behavior is to bang on an open door. For instance, organic psychiatry, which centered on constitutional and hereditary factors, has always regarded psychoanalysis as a pure "nurture psychology," but to do so, it had to disregard the "nature" conception of drives in psychoanalytic theory. The root of this and kindred misunderstandings seems to be that the theory did not systematically clarify its stand on the dual relationship between the organism and its environment. It is characteristic of organisms that they are dependent on their environment but also relatively independent from it. This balance between dependence and independence might be designated as a relative autonomy (of the organism from its environment) in the same sense in which the analogous relationship of the ego to the id is so designated [280].

The organic psychiatrists' view implies an *absolute autonomy* from environmental influences. From their point of view, Freud completely disregarded this autonomy, since he dealt mainly with the dependence

⁵⁹ In this conception modes develop according to genetic, inborn laws, but the social organization of the environment defines their place and form in the behavior repertory and their use in reality mastery and adaptation.

⁶⁰ This conception does not deny the "objectivity" of the common, consensually validated aspects of space and time, or the intellectual possibility of transcending the subjective coordinates of reality in order to build universally valid sciences of space, matter, etc. It does not invalidate the coordination of the organism with the "objective" environment, which is guaranteed by the apparatuses of primary and secondary autonomy (e.g., the perceptual and motor apparatuses), nor the effectiveness of the "causal texture" of the environment (Brunswik, Heider) which sets limits to all individual and social "subjectivity." For a detailed discussion of these issues, which lead far into perception theory, see G. Klein's forthcoming volume [198].

of behavior on experience, societal norms, etc. On the other hand, Freud's stress on drives, as well as his regard for constitutional factors, made his theory appear—even in the eyes of many of his followers—to imply *absolute* autonomy from the environment. From this vantage point society appeared as a factor interfering with nature, man was looked upon as a born individualist, and therefore the therapeutic aim was often regarded as the liberation of human nature from social bondage.

However, Freud considered the sexual drives and their object choices as anaclitic⁶¹ upon the drives of self-preservation and their object choices. This is a statement of the growing organism's first social relationships and implies the social determination of behavior. So does the conception of the Oedipus complex: the budding individual's social environment provides the objects of his libidinal and aggressive drives, and the structures (identifications in ego and superego development) which the relationships between the subject and these objects give rise to, codetermine his behavior in general and not merely his pathology [126]. Though these social conceptions were not generalized into an explicit psychoanalytic social psychology, the social determination of behavior is clearly not alien to classic psychoanalytic theory.

Why then the reluctance of classic psychoanalysis to accept the emphasis placed on the social determination of behavior by Adler, Sullivan, Horney, and Kardiner? It seems that a struggle between different conceptions of the *relative* autonomy of behavior from environmental reality⁶² lay behind this reluctance. To the classic analyst's mind, the "dissident" schools, upon discovering the dependence of behavior on social reality, abandoned those concepts of the psychoanalytic theory which encompassed the observations concerning the autonomy of behavior from the environment: these were the drives and the other constitutional (e.g., structural) givens. The net result seemed to be that some dissidents came to regard adaptation as "adjustment" (particularly as a therapeutic goal), to disregard the existence and nature of drives, to stress the environmental demand, and thus deliberately or unwittingly to reinforce censorship and superego. These therapists were said to have come to take "society's side" against the patient—although their task, as originally conceived, was to take neither or both. In other dissidents the result seemed to be very different: society was blamed for man's troubles and was to be so changed as to cause no more trouble to man. This was said to be a stand on the side of the individual against society. Thus, do-goodism, social rebelliousness, Philistine demands for con-

⁶¹ Anaclitic: leaning upon. The implication is that the first objects of the sexual drive are the people who take care of the infant and guarantee his survival, i.e., who are the objects of his self-preserved drives [101].

⁶² Cf., for this section, Fenichel's discussion of Fromm [76].

formity, and attitudes so approving of individual liberty as to shade into license were considered to be characteristic of the dissidents. Any of these attitudes may well be a therapist's private convictions, and as such cannot but enter therapy in some form, but none of them has a place in psychological theory. It is not our task to establish whether these are fair assessments of the dissident schools.⁶³ It is sufficient to point out that these imputed or actual attitudes are indices of a struggle centering around the ego's relative autonomy from social reality. The dissidents' contribution toward the formulation of the psychosocial point of view must not be underestimated.

Anna Freud's stress on work with the parents of her child patients, and her work with groups of children in the course of the war, is a recognition of the social determination of behavior.

Hartmann gave the first theoretical formulation of the role of social reality. His point of departure was Freud's conception of the central role in human development of the infant's prolonged helplessness and dependence on caretaking adults. Hartmann's major theoretical advance is embodied in his concept of "social compliance," which is coined on Freud's concept of "somatic compliance." The referents of this concept are the observations concerning those institutions of society which meet, foster, and mold the developing individual's inborn and acquired adaptive means. Education as a social institution is an instance of "social compliance" [157].

Erikson's epigenetic psychosocial conception parallels and fills in Hartmann's systematic and programmatic formulations. Erikson's conception of society is detailed: it is the geography and the means of survival it provides; it is the economy and other social institutions; it is the ideology, including tradition [61, chaps. 3, 4; also 60]. It involves: (1) the epigenetic conception of ego development consisting of a sequence of developmental phases, each characterized by a phase-specific crisis which is universal, while its solution varies from society to society and is individually unique [61, chap. 2; 62, 66]; (2) the caretaking people (parents, teachers, etc.) and their practices representing the society's institutions and traditions which were developed to meet each phase-specific crisis of the developing individual's life cycle; (3) the phase-specific needs of the growing individual eliciting dovetailing needs in the caretaking people, which correspond to the respective phases of their life cycles; the society's institutions and traditions making their contribution to the solution of the growing individual's phase-specific crisis by means of the so-elicited needs of the caretaking people; (4) the resulting behavior

⁶³ Fenichel's assessment [76] of Kardiner and Fromm seems as adequate as the conceptual equipment of the time permitted: Hartmann's and Erikson's concepts had not yet entered the argument.

forms having, by and large, an accepted place in the society and guaranteeing the individual's viability in it.

In Erikson's conception neither does the individual adapt to society nor does society mold him into its pattern; rather, society and individual form a *unity* within which a mutual regulation takes place. The social institutions are preconditions of individual development, and the developing individual's behavior in turn elicits that help which society gives through its adult members directed by its institutions and traditions. Society is not merely a prohibitor or provider; it is the necessary matrix of the development of all behavior. Indeed, the development and maintenance of the ego, of the superego, and perhaps of all structures are dependent on the social matrix: behavior is determined by it and is possible only within it.

Bettelheim's [21, 22, 23] and Redl's [282, 283] studies confirmed this conception and extended it. Gill and Rapaport [149] concluded, from the observations and theories here discussed, that the metapsychological triad of the dynamic, structural, and economic points of view must be extended by the addition of an adaptive point of view.

K. Discussion

This sketch of the basic propositions of the general psychoanalytic theory was presented to make a discussion of its variables possible. It centered around the three classic metapsychological points of view (dynamic, topographic, and economic) [cf. 117, p. 114], but it also included the structural point of view (which elaborated and replaced the topographic one) as well as the genetic and the adaptive points of view, which (being of the same order of significance in the theory as the classic triad) seem necessary to complete the system of psychoanalytic metapsychology [see Gill and Rapaport, 149]. The inclusion of the psychosocial point of view (like that of the topographic one) is a mark of systematic weakness, since it is merely a specific aspect of the adaptive point of view. It is as yet difficult, if not impossible, to present the theory divorced from its history. The topographic point of view, though it is satisfactorily replaced by the structural one, appears here because it is difficult to present the latter so that the role of unconscious determination will emerge as clearly as it does from the by now historical topographic point of view. Likewise, the psychosocial point of view is discussed separately because it is as yet difficult to present the adaptive point of view so that its psychosocial implications emerge clearly. Both Hartmann's and Erikson's theories are too new, their implications too little understood, and their relationship to each other too little explored [see Rapaport, 277, 278] to permit a statement disregarding all but systematic considerations.

Thus it is likely that seven of these ten points of view which we have discussed here will, in future systematic treatments, be condensed into the five (dynamic, economic, structural, genetic, and adaptive) metapsychological points of view. Indeed, they may eventually be formulated as the axioms of the system. The remaining three points of view (empirical, Gestalt, and organismic) seem to be of a different character, and lumping them together with the metapsychological points of view is another indication that the systematization here attempted is premature. By and large, they deal with the theory's observables and with their organization as units. It is possible that these three points of view will appear, in future systematic statements, as definitions of observables. If so, then why were they not segregated here from the others? One reason is that the others are not yet formulated as axioms and they, too, imply definitions. Then why not explicate all the definitions and segregate them from the points of view? At this stage of our knowledge, even if such explication and segregation were possible, the present analysis would be an indispensable preparatory step.

Tolman's [316], MacCorquodale and Meehl's [227], and others' conceptions of independent, intervening, and dependent variables cannot be discussed here in detail. It should suffice to say that their views of these variables do not seem to be "methodologically pure," but rather loaded with their respective systematic biases.^{63a} Only that conception of variables which Koch calls the "mathematical" seems relevant to psychoanalytic theory. It is in the sense of such a mathematical conception that we will speak here of variables.

I believe that the following conclusions may be derived from the sketch of the theory's "points of view":

1. The psychoanalytic concept of *overdetermination* implies that one or several determiners of a given behavior, which appear to explain it, do not necessarily give its full causal explanation. This is not *per se* alien to other sciences, though a *principle of overdetermination* did not become necessary in any of them. Psychoanalysis' need for this principle seems to be due partly to the multiplicity of the determiners of human behavior, and partly to the theory's characteristic lack of criteria for the independence and sufficiency of causes. The determiners of behavior in this theory are so defined that they apply to all behavior and thus their empirical referents must be present in any and all behavior. Since there is usually no single determiner which constantly assumes the dominant role in a given behavior, other determiners can hardly be neglected while a dominant determiner is explored. When favorable conditions make one determiner dominant, the investigator is tempted to conclude

^{63a} Frenkel-Brunswik [92, pp. 307ff.] gives a cogent discussion of some of these biases.

that he has confirmed a predicted functional relationship—as he indeed has. Regrettably, the attempt to repeat the observation or experiment in question often fails, because in the replication either the same behavior appears even though a different determiner has become dominant, or a different behavior appears even though the same determiner has remained dominant.⁶⁴ Lewin's *Gesetz und Experiment in der Psychologie* [215] is relevant here: it argues that the criterion of validity for psychological experiments [cf. Gill and Brenman, 34, 147; and Benjamin, 11] is not repeatability, but predictable systematic variation.

The implications of the concept of overdetermination for the choice of independent variables are: (a) Any motivation high in the hierarchy of psychological organization, if chosen as the independent variable of an experiment or observation, may prove to be dependent on variables closer to the base of the hierarchy. In this case, either the dependent variable will be treated as an implicit function of these more basic variables (motivations, structures, etc.), or the latter will be considered as intervening variables interposed between the independent and the dependent variable. (b) If a basic motivation is chosen as the independent variable of an experiment, then variables higher in the hierarchy will be interposed as intervening variables between the experiment's independent and dependent variable. For instance, in Klein's [197] experiment, cognitive attitudes are the intervening, thirst the independent, and cognitive behaviors the dependent variables. Though in this theory basic drives are systematically distinguished⁶⁵ independent variables, as empirical independent variables they do not seem to differ significantly from other motivational variables.

2. The psychoanalytic conception of *autonomy* puts a further limitation on the *distinguished* independent variable character of basic drives by pointing to other equally distinguished ones. The concept of autonomy implies that structures of primary (and secondary) autonomy may retain (or attain) a relative independence from drives. The function of autonomous structures, even when triggered by drives, may remain independent from them. Derivative motivations (as a rule, related to

⁶⁴ For instance, in a Zeigarnik-like experiment, on the one hand an interrupted task may be remembered either because of the undischarged tension system (Lewin's explanation) or because the task had a specific "historical" or "motivational" significance for the subject to begin with. In this instance different dominant determiners have identical effects. On the other hand, an interrupted task may be forgotten (in spite of the undischarged tension system) when the interruption is experienced as a failure. In this instance the undischarged tension remains the dominant determiner, but its behavioral effect is different.

⁶⁵ The term "distinguished" is used here to convey that though the theory's development placed restrictions on the initial conception of drives as "ultimate causal determiners," drives still retain a special position in the system of the theory (see pp. 89–91, above).

structures) may also attain such independence. The implications of this concept of autonomy, for the choice of independent variables, are contrary to those of overdetermination: since autonomy increases with the distance from the basic drives, variables (structures or motivations) high in the hierarchy also appear as systematically *distinguished* independent variables. As independent variables of an experiment, they have the advantage that the dependent variable need *not* be an implicit function of more basic ones, and that (in the ideal case) no intervening variables are interposed between them and the dependent variable.

The conflicting implications of the "overdetermination" and the "autonomy" concepts limit the advantages of the latter also, since not all structures and derivative motivations retain or attain autonomy; and moreover, autonomy is not an all-or-nothing affair, but rather a matter of degree, and thus we always deal with *relative autonomy*, the degree of which must be empirically determined.

3. Basic motivations and structures, as well as motivations of a high degree of autonomy, are systematically *distinguished* variables. Whether they should be considered to be systematic independent variables (in Koch's sense) is not clear, since they may also appear in the role of intervening and dependent variables. To illustrate this, let us survey the main classes of the theory's variables: motivations and structures (of any hierarchic level and degree of autonomy), behaviors (including thought and affect as well as observable action), and external reality.

External reality. In the reflex-arc model external reality (stimulus) appears as the *independent variable*. The model assumes that in this case unconscious impulses and ideas always enter as intervening variables, and that the dependent variable is motor action and/or conscious thought and/or affect. However, the autonomy concept implies that the functional relationship between stimulus and behavior may be of any degree of relative autonomy; i.e., the extent to which unconscious impulses and ideas intervene may vary. Thus S-R psychology appears here as a limiting case of a high degree of autonomy (automatization). [See Hartmann, 157, pp. 26, 86ff.]

External reality as an *intervening variable* is one of the implications of the adaptive point of view. When either structure or motivation is chosen as the independent, and behavior as the dependent variable, external reality appears as the intervening variable, and corresponds to the adaptive aspect of the behavior in question.⁶⁶ The concept of relative autonomy from the environment, however, implies that some of the motivation vs. behavior and structure vs. behavior relationships (like those in impulsive actions, fugues, and characterologically typical be-

⁶⁶ For instance, in studying the effect of hunger on feeding behavior, the absence of, or presence and demeanor of, an observer will enter as an intervening variable.

haviors respectively) will be, within limits, invariant in regard to changes in external reality.

How external reality can be a *dependent variable* is less obvious. If external reality were conceived "geographically" [see Koffka, 204, pp. 27ff.], then it would be meaningless (or a subject matter for physics, chemistry, etc.) to treat it as a dependent variable. If, however, we go beyond its inherent "causal texture" (Brunswik, Heider) and conceive of it psychologically, then it can become a dependent variable. A person in my life space is an external reality, yet this external reality is a variable dependent on my "feelings" toward this person. In Bruner and Goodman's experiment, the sizes of coins appear as dependent variables, though in that instance it is difficult to separate perceptual behavior and external reality as dependent variables.

Motivations. In deprivation experiments, as well as in observations made in therapy (e.g., on transference phenomena), motivations appear as *independent variables*;⁶⁷ and their hierarchic position (implying considerations of overdetermination and autonomy) defines the degree of their actual independence. In these instances defenses and other structures usually enter as intervening variables, and behavior is the most common dependent variable, though in investigations concerning processes of structure building and structural change, structure will be the dependent variable.

In the reflex-arc model, unconscious motivations appear as *intervening variables*, and external reality plays the role of the independent, and behavior that of the dependent variable. Moreover, wherever a controlled (not impulsive) motivation, or a structure close to the base of the hierarchy of the mental organization, is taken as the independent variable, higher-level motivations appear as intervening variables, provided that no automatized relationships obtain between the independent and the dependent variables.

Motivations as *dependent variables* are encountered when motivations close to the base of the hierarchy are chosen as independent variables and defensive structures appear as intervening variables; or when external reality, in the form of deprivation, is the independent variable. Clinically the presence of defensive structures is—as a rule—inferred from the appearance of derivative motivations,⁶⁸ which are in this case dependent variables. But certain motivations may also appear as dependent variables where external realities (other than deprivation) or psychological structures are the independent variables.

⁶⁷ Except where the degree of deprivation is the independent, and the drive the dependent variable.

⁶⁸ For instance, in studying a coprophilic impulse, the presence of *reaction formation* may be inferred from the appearance of a motivation for excessive cleanliness.

Structures. Structures appear as *independent variables* wherever individual differences in behavior, under (relatively) constant motivation and stimulation, are studied: for instance, in the comparative study of symptoms in various neuroses, and in the studies of individual differences in perception [Klein, Holzman, Gardner, Schlesinger, 144, 179, 180, 198, 304].

Structures as *intervening variables* are commonplace in clinical observation. They account for the lack of a one-to-one relationship between motivations and behavior. Defensive structures countermand motivations and replace them by derivative motivations (as, for instance, in reaction-formation). Controlling structures direct and channel motivations, as in delay and detour-behavior and in the choice of substitute goals. In Klein's [197] thirst experiment, the thirst motivation was the independent, perceptual behavior the dependent variable, and structures (the subjects' "cognitive attitudes") appeared as intervening variables.

It is less easy to conceive of structures as *dependent variables*, though they do appear as such in processes of structural change, including those of learning. In so far as psychoanalysis as therapy achieves its goal of changing existing structures, in at least some of the observations made in therapy, structures appear as dependent variables. Piaget's [254] schemata of the primary, secondary, and tertiary circular reactions are structures, and in his developmental studies which trace their growth and fate, structures are dependent variables.

Behavior. The role of behavior as a *dependent variable* needs no discussion. But it might be worth noting again that here behavior is broadly defined to include conscious and unconscious thought, affect, and action, which can and do substitute for one another, so that behavior is a complex dependent variable.

The role of behavior as an *intervening variable* is more difficult to conceive of, though it is commonly enough encountered. When, for example, a motivation is taken as the independent variable and the observable action facet of behavior is taken as the dependent variable, the thought and affect facets of behavior, as a rule, interpose themselves as intervening variables. This seems to be one of Hebb's [169] points in his criticism of S-R theories. Naturally, in impulsive actions and where the relation between motivation and overt action is automatized, such intervening variables are likely to be absent.

The conception of behavior as an *independent variable* is perhaps the least obvious of all. Yet, for example, under conditions of a high degree of autonomy, one facet of behavior may be taken as an independent and any other facet of it as the dependent variable. For instance, in Werner's experiments [152, 208]—in which the subject presses against motor restraint, with the consequence that the number of his movement responses in the Rorschach test increases—the motor

facet of behavior is the independent, while its cognitive facet is the dependent, variable.

4. It seems that the variables of all these major classes can be treated as empirical independent, intervening, and dependent variables; but they differ greatly in regard to manipulability, which is considered by some to be the criterion for the selection of independent variables. Genetic and structural variables, for instance, are not amenable to direct manipulation. Besides such intrinsic difficulties, manipulation of the internal and external environments as well as of action is also limited by due regard for the subjects' privacy and by the fact that social manipulation beyond a narrow range is likely to endanger the individual's rights. But manipulability is not an indispensable criterion; it may be replaced by observation (as in astronomy), or by seeking out "nature's experiments" (as in evolution theory).

5. We may conclude that psychoanalytic theory requires the exploration of all the possible functional relationships among its variables. One wonders whether or not there is an intimate connection between the rigid decisions of various schools of psychology on systematic variables (e.g., those of S-R and Gestalt psychology) and the limited range of observables acceptable to each of them. Any limitation on the choice of variables seems to result in a limited range of observables and observational methods, and it is the dearth of methods which is probably the major obstacle to bridging the gap between psychoanalysis and academic psychology [cf. Shakow and Rapaport, 309], and between the various schools of psychology.

III. THE INITIAL EVIDENTIAL GROUNDS FOR THE ASSUMPTIONS OF THE SYSTEM AND THEIR STRATEGIC CHARACTER

A. Initial Evidential Grounds

We will discuss here only the evidential grounds for the early assumptions of the system;⁶⁹ to trace those of its present assumptions would be a historical job far exceeding the scope of this essay. Thus, the propositions to be discussed in this section are not always identical with those of the present theory.

The basic assumption of psychoanalytic theory was and is *thoroughgoing psychological determinism*. Its other initial assumptions are implicit in the thesis of psychoanalytic metapsychology: a full description of any psychological phenomenon must include its dynamic, topographic,

⁶⁹ The reference here, if not otherwise indicated, is to Breuer and Freud [35].

and economic descriptions.⁷⁰ What are the assumptions implied in these three points of view?

The topographic point of view distinguishes between the Systems Unconscious, Preconscious, and Conscious, and thus implies the assumption of *unconscious psychological processes*, which is, except for psychological determinism, the earliest and most general assumption of psychoanalysis. On this assumption are built the concepts of unconscious motivation (involving dynamic assumptions) and primary process (involving economic assumptions).

The dynamic point of view implies the assumption of *psychological forces* and their *conflicts* in general and of the *drive nature of these forces* in particular [98]. On these assumptions are built the concepts of libidinal drives and censorship (ego drives or self-preservative drives—which are now dated), as well as the conception of the central role of libidinal drives [101].

The economic point of view implies the existence of *psychological energies* in general and their *drive origin* in particular. These assumptions underlie the concept of cathexis (quantity of energy). Corollaries of these assumptions are principles analogous to the physical principles of conservation of energy, entropy, and least action. *The conservation principle*: cathexis is never lost and thus is traceable in the expenditures and transformations of cathexes involved in psychological forces [117, p. 114]. *The entropy principle* (the much misconstrued pleasure principle): drive energy tends toward discharge (i.e., diminution of tension) [35, p. 143; and 98, pp. 508–509, 533–535]. *The principle of least action*: processes involving cathexes other than those of basic drives operate so as to expend the least amount of cathexis [98, pp. 533–534]. The main concepts built on these principles are wish-fulfillment vs. reality-testing which direct and the primary vs. secondary process mechanisms which subserve the transfer and transformation of cathexes [98, pp. 530–531, 535–536].

The initial evidence for these three sets of assumptions and their corollaries cannot at this time be sharply separated from the evidence for the validity of the theories built upon them. A sharp separation would require prior decisions as to which assumptions are to be treated as axioms and which are to be empirically derived from a combination of axioms, definitions, and observations.

1. The assumption of psychological determinism. The initial evidential ground for this assumption was the observation that apparently meaningless hysterical symptoms, previously attributed to a somatic

⁷⁰ This is the earliest explicit formulation of metapsychology; see Freud [117, p. 114].

etiology,⁷¹ disappeared when the patient, in hypnosis, related them to past experiences, thoughts, feelings, or fantasies, and thus endowed them with meaning and psychological "cause."⁷² This success at tying apparently arbitrary pathological behavior into a causal psychological network served as the empirical point of departure for the venture into the broader realm of dreams [98], parapraxes [99], etc. The success in "interpreting" these resulted in the further and apparently limitless generalization of this assumption, on which all the other initial assumptions of the theory rest. Clearly, the empirical evidence alone, without the background factors discussed in the introduction of this essay, might not have given rise to the assumption of psychological determinism.

2. The assumption of unconscious psychological processes. The observation that in hypnosis and in the course of free associating patients become aware of past experiences, or of relations between them, or of relations between past and present experiences, led to the assumption of the "nonconscious" survival of such experiences and the "non-conscious" existence of such relationships [35, 95]. But only the discovery that such nonconscious experiences and relationships are subject to rules (e.g., the pleasure principle and the mechanisms of the primary process) different from those of our conscious behavior and thinking made the above-mentioned memory phenomena (already observed by Charcot, as well as Bernheim) [see Breuer and Freud, 35, chap. 1] into evidence for the assumption of *unconscious psychological processes* [98]. The essence of this assumption is that it conceptualizes these observations in psychological terms, though the processes inferred from them are subject to rules different from those of the familiar, conscious psychological processes. In other words, it refuses to treat the nonconscious as somatic and the nonlogical as nonpsychological. It rejects both consciousness and logical relations as necessary criteria of psychological processes, and thus arrives at the concept of unconscious psychological processes abiding by rules other than those of conscious processes. This assumption gained powerful corroborative evidence from the study of dreams [98, p. 540].

3. The assumption of unconscious psychological forces and conflicts. The evidence for unconscious psychological processes did not, in the beginning, necessitate the assumption of unconscious psychological forces and conflicts. Breuer's hypnoid assumption and Freud's trauma

⁷¹Even the psychologically minded French school, Charcot, Janet, etc. [see 309], subscribed to this.

⁷²For the detailed reports of these observations, see [35]. The theoretical section of that volume contains a fragmentary and simplified version of the neuropsychological theory Freud developed in the Project [94, appendix] to account for these observations. These two theoretical statements are the predecessors of Freud's theory contained in the seventh chapter of *The Interpretation of Dreams* [98].

and retention assumption [35] seemed to account for the unconscious character of these processes. The evidential ground for the assumption of *unconscious psychological forces* was Freud's discovery that much of what his patients reported to him was not, as he initially believed, unconscious memories of actual experiences, but rather unconscious fantasies [94, p. 215, letter no. 69, 1897]. The assumption of unconscious forces was to account for the agent which creates these fantasies and brings them to the patient's consciousness in hypnosis and in free associations, as well as for the agent which, before and in the course of therapy, prevents them from becoming conscious. Led by the libidinal content of these fantasies, Freud assumed that the unconscious force which creates them, and makes them conscious in the course of therapy, is the sexual drive. In turn, he conceived of the forces which clash with the sexual drives, divert them into symptoms, and block the path to consciousness of the fantasies which they gave rise to, as those of the censorship: the ego drives.

Thus the initial evidential ground for this assumption comprised observations pertaining to unconscious fantasies, to their becoming conscious in therapy, to the resistance against their becoming conscious, and to the relation between these fantasies and the symptoms.

4. The assumption of psychological energies and their drive origin.

The observation that recall of traumatic experiences, when accompanied by affect, results at times in the disappearance of symptoms and anxiety, and at other times in their replacement by other symptoms and anxiety equivalents, suggested that a displaceable and transformable quantity was involved in the psychological processes underlying symptom formation. Before he developed the concept of unconscious forces, Freud assumed that this quantity was the affect, which when not expressed (i.e., "dammed up") was either transformed into anxiety or displaced into a somatic organ (conversion) or a thought process (e.g., obsession). After he developed the concept of drives, this quantity was conceived of as drive energy (cathexis).

The force concept alone could not account for the observation that blocking a drive action results in behavior different in direction and form from that expected of the drive; this observation became the evidential ground for the assumption of *psychological energies* and of a conservation principle pertaining to them. These psychological energies, analogous to those of physics, being nondirectional (scalar) could, through their displacements and transformations, account for the "work" performed by the psychological force in forms unlike, and at points not coinciding with, that expected of them. This assumption when combined with that of the instinctual origin of the unconscious psychological forces led to the assumption of the drive origin of psychological energies.

The evidence for the assumption of an entropy principle and a principle of least action lay in the observation of the difference between those two kinds of behavior characteristics which were conceptualized as manifestations of the primary and secondary processes. The prevalence of the first kind of characteristic makes a behavior preemptory and overvalent, tolerating neither delay nor detour, as though it were striving for immediate discharge of a great quantity of excitation. These characteristics of obsessional and delusional ideas, compulsive rituals, hysterical tantrums, etc., served as the evidential ground for the assumption of an entropy (pleasure-pain) principle. The prevalence of the second kind of characteristic makes a behavior amenable to interruption, delay, and detour, as though it involved potentials without significant intensities. These characteristics of goal-directed action, and of ordered logical thought, were the initial evidence for the assumption of a principle of least action.

It may be objected that this discussion has not dealt with the assumption of the *ultimate determination of all behavior by unconscious drives*, which, with its emphasis on *ultimate* and *all*, is indeed one of the assumptions of early psychoanalytic theory. We bypass it here because it is actually a combination of the assumptions we have discussed.

B. Strategic Choice of Initial Evidential Grounds

The question why the observations which served as the initial evidential ground for the assumptions of the system were considered strategic is in a sense irrelevant to the theory of psychoanalysis. The initial situation was not that the phenomena of pathology were considered strategic: they were *the* material which posed the problems to be coped with. The theory grew up on the soil of the neuroses, their pathology and their therapy. It was from there that it branched out into a relentless and ever more diversified endeavor to show that its system of conceptual relationships, though it was designed to explain pathological (apparently arbitrary and psychologically meaningless) phenomena, can also give an adequate causal account of the obviously meaningful phenomena of normal psychological life.

Pathology *was* (as Virchow recognized in biology) strategic for the study of normal processes.⁷³ It showed that the so-called normal state of

⁷³ Pathology had still another role in the development of psychoanalysis: except for love and mortal fear, only actual suffering and the hope of relief could have prompted a man to permit another that relatively unlimited access to his privacy which opened the door for psychoanalysis to the exploration of its initial evidential ground. That this opening is at the same time an obstacle to the independent verification of psychoanalytic theories is as natural as it appears paradoxical at first sight.

affairs which we take for granted is only one of many possibilities. Thus it opened the road to causal analysis, by means of which psychoanalysis consistently and successfully shattered the barriers between the normal and the pathological, the infantile and the adult, the recondite and the obvious, the exceptional and the commonplace [98, pp. 538–540]. It is not a historical accident that Freud's theory grew out of the study of pathology.

Pathology and its therapy were strategic for the discovery of the commonalities of normality and pathology, but they proved less strategic for the discovery of the differences between them. Only slowly, with the development of psychoanalytic ego psychology, has psychoanalysis begun to rediscover the differences between the pathological and the normal, the infantile and the adult, the maladaptive and the adaptive. So far, the concepts of structure, autonomy, adaptation, and reality are the main tools the theory uses in its endeavor to discover these differences. These are the very concepts which distinguish psychoanalysis from the genetic reductionist theories which see no cleavage between the normal and the pathological, the adult and the infantile, as well as from G. Allport's [8] and kindred theories which see a sharp cleavage between them.

We cannot leave this discussion without dwelling, at least briefly, on the methods by which the initial evidence was obtained.

Nowadays methodology is in vogue, and all considerations of method and technique are dignified by that name. Yet one essential methodological task—the study of the relationship between a theory and the method of observation by which the data it explains are obtained—is rarely pursued. The question is: to what extent does a theory, based on data obtained by a given method, reflect the nature of the data itself, and to what extent does it reflect the method of data-gathering and its limitations? The man who shouts into an empty room is likely to hear his own echo; likewise the investigator *may* get back little more than what he has already built into his method. For instance, we need to know to what extent the “laws of learning” are laws of human nature, and to what extent they are artifacts of the method used by associationists and conditioners to “establish” them. Likewise to what extent does psychoanalytic theory reflect human nature, and to what extent does it reflect Freud's methods for studying human nature?

Methodological study is likely to reveal that some psychoanalytic methods (for instance, the therapist-patient two-group) had a defining influence on psychoanalytic theory [see Rapaport, 260]. Although we cannot pursue this problem further, we want to suggest that methodological analysis may well lead to a distinction between a *general* psychoanalytic theory which is little dependent on these methods, and a *specific* psychoanalytic theory which is greatly dependent on them. In contrast to the

specific theory, the general theory should be readily testable by methods other than those by which the initial evidence for it was obtained. In this essay we have centered on those aspects of the theory which are not obviously dependent on these methods, and have tried to avoid the concepts which obviously are tied to them, like transference, interpretation, etc.⁷⁴

C. The Relation of the Observations to the Theory

In his outline, Dr. Koch asks us to select the chief empirical independent and dependent variables of the theory and to demonstrate their linkage to its systematic independent and dependent variables. In Section II. K., we discussed the difficulties in the way of such an attempt. To minimize repetition, we will illustrate⁷⁵ the relation of an empirical observation to the variables of the theory.

Let us take the words of a man who utters the sentence, "Now things are becoming queer," and let us provide the context from which psychoanalytic theory will adduce its explanation of this verbal behavior:

This behavior occurred in the course of a discussion in a group.—The other members responded to it with consternation.—The man was bewildered by this response.—Later he found out that he had said "queer" and not "clear" as he had intended to do, and as he thought he had done.—He was embarrassed by this discovery.—The discussion concerned a mismanagement of the group's affairs.—The subject's utterance followed an explanation by the chairman of the group.—The chairman attributed the mismanagement to a misunderstanding by the treasurer of an instruction given by him, and not to any malicious intent.—The chairman commanded the unquestioned respect of the group and also wielded considerable power otherwise.

In terms of common-sense psychology, we are dealing here with a slip of the tongue.

In descriptive terms: the subject's conscious intention was to agree with the chairman's explanation. He did not carry out this intention, but

⁷⁴ An example to highlight the relationship of method and theory: it appears that H. S. Sullivan [314], taking as his point of departure the psychoanalytic methods of the two-group and participant observation, arrived at a theory of personality which dissolves the concept of the individual and conceives of the person as one of the quasi-stable foci in the network of interpersonal relationships. In Sullivan's theory then, the method of investigation and the transference concept based on it came to play a dominant role, with the consequence that the theory overrides a crucial characteristic of the nature of the subject matter, namely, the individuality of the person. Individuality to Sullivan appears as a noxious anti-scientific myth, which he reduces to the *personification* function of the self-system.

⁷⁵ This example simplifies an actual situation by eliminating obscure points, to avoid lengthy explanations of peripheral matters.

instead expressed himself in a way that caused consternation; he was unaware both of not having carried out his intention, and of his consternation-arousing utterance. When he was told what he had said, he became embarrassed.

In terms of a data language: the independent variable (conscious intention) determined a value of the dependent variable (verbal utterance of agreement and conscious awareness of it). However, an intervening variable determined another value of the action component of the dependent variable (dissenting, consternation-arousing verbal utterance). The intervening variable left the conscious-awareness component of the dependent variable unaltered. A second intervening variable (external reality: information) altered the conscious-awareness component of the dependent variable and determined its affective aspect (embarrassment).

This formulation is not "neutral": it implies that the independent variable is a "motivation" (intention). Indeed, even the descriptive terms imply this. Before Freud, at least the common-sense term "slip of the tongue" was neutral, but it is not neutral now. Let us attempt a crude associationist formulation, to show that data languages are inseparable from construct languages and thus cannot be neutral: the chairman's explanation was associated in the subject to an approving verbal statement; the subject's actual response, however, was linked to the chairman's explanation by stronger associative bonds; the clash of the two associative complexes resulted in a compromise in which one of the complexes determined the awareness, while the other determined the verbal response of the subject.

In terms of psychoanalytic construct language: the subject's conscious intention is referred to a socially adaptive ego interest. The failure to carry out the intention is referred to an id motivation. The unawareness of the failure is referred to an unconscious ego motivation conflicting with this id motivation. The unawareness of the actual verbal expression used is referred both to the unconscious (id) motivation which was expressed, and to the unconscious ego controls (defenses) which, though they failed to prevent the use of the ego's executive apparatus by the id motivation, succeeded in preventing its access to consciousness (compromise). The acute embarrassment is referred to the affect manifestation of the clash between the unconscious motivations and the restored ego control.

Let us take a closer look at the concepts involved. The unawareness is obviously the referent of the descriptive concept unconscious. It is likewise obvious that the intent to say "clear" is a conscious motive. But it is an inference that this motive is a force and it is a further inference that saying "queer" indicates the presence of another, un-

conscious, force. It is a still further inference that a third force is also involved which prevents conscious awareness both of the failure of the conscious intention and the success of the unconscious intention. It is yet a further inference that the latter two forces conflicted and reached a compromise, indicated both by the "clang" similarity of the words "clear" and "queer," and by the fact that the unconscious force attained control of the executive apparatus but did not gain access to consciousness. Thus we see that some of the concepts involved here are close to the observations, while others are at increasing distance from them.

In clinical inferences, the distance between observations and concepts may seem even greater. The clinician may infer, for instance, that the slip pertains to the ancient triangle formed by the subject, his older brother, and father, which was reactivated by the triangular situation of the subject, treasurer, and chairman. He may even go further and infer that homoerotic and aggressive drives involved in jealousy are the unconscious forces which conflict here with the ego's defenses against them and interfere with ego interests.

No wonder psychologists gained the impression that the relation of psychoanalytic concepts and theories to observations is distant and arbitrary. But is this impression accurate? Let us suppose that our subject volunteers for a free association session, and his associations cluster around the treasurer and the chairman, rather than around the interests of the group. Will we then be justified in inferring that the agent behind the word "queer" is an unconscious force directed toward the treasurer and the chairman? Let us suppose further that the subject's associations not only corroborate that this unconscious force is an aggressive drive, but identify it as being of a jealous-suspicious variety. Let us finally assume that, in the course of these associations, the subject comes to realize that he actually has had filial feelings toward the chairman and vague, poorly understood feelings of irritation with the treasurer, akin to those he used to feel toward his brother, and thereby he specifies that the unconscious force pertains to the subject-brother-father triangle.

True, in this sequence the concepts (unconscious, unconscious impulse, unconscious hostile impulse, unconscious hostile and libidinal impulse, unconscious hostile and libidinal infantile impulse) are increasingly remote from the slip of the tongue which is the original observation. But the associations, too, are observations and the increasingly remote concepts are introduced in reference to these additional observations. Thus, the distance between observations and concepts is not as great as it seems on first sight. But there still remains a difficulty: the relationship of each more remote concept to the corresponding additional observation presupposes the less remote concepts. For instance, without assuming that the unawareness of the subject is a referent of the

descriptive concept unconscious and without assuming the unconsciously motivated character of the slip, it would make no sense to infer that the subject's associations specify the pertinence of the aggressive impulse (indicated by the slip) to the treasurer and the chairman.

This relationship between observations and concepts is common to all sciences: *observations demonstrate theoretical relationships only to those who already conceive of the observed in terms of the theory's concepts*. But the psychologist seems to overlook this truism when it comes to psychoanalysis. This oversight is so common that the lack of systematic treatments of the theory alone cannot account for it. There must be other reasons, and a few of these will be conjectured:

The psychologist is accustomed to explicit—and, indeed, operational—definitions of concepts and is wary of psychoanalysis' definitions of concepts. He suspects that the mutual implications of its concepts hide a vicious circle. In the lack of a systematic statement of the theory, we can sympathize with his wariness, but we must keep in mind that in physics nobody would think of asking for an explicit definition of energy that did not involve the concept of work (which in turn involves the concepts of path and force, which in turn involve mass and acceleration, which in turn involve time and velocity, which in turn involve space and time). We shall not dwell here on the demand for operational definitions: Feigl [71] and Frenkel-Brunswik [92] have demonstrated that in this matter psychologists have tried to be more Catholic than the Pope, and that operational definitions of *all* its concepts have never been demanded of any science.

The clinical psychoanalyst is deft and nonchalant in using concepts at a great distance from the observations. For instance, he may conjecture from the word "queer" what *might* be involved in this slip, bypassing the intervening observations (e.g., associations) and concepts. It may be a well-supported conjecture, if the patient's previous productions converge on it; or it may be a poorly supported one, if the analyst is more imaginative than careful. It may even help the patient to insight if it is conveyed to him. But a *conjecture* it remains until the patient's associations or other productions confirm it. Some such conjectures are supported by so much experience, and pertain to relationships so common, that they are *almost* certain. These are particularly prone to turn into clichés, to give the outsider the impression of arbitrariness or of an uncanny "second sight," and to oversimplify the complexity of the theoretical relationships even in the psychoanalyst's mind. Actually the psychoanalyst's use of these may not differ from an electrician's use of technical terms and repair- or construction-procedures without his referring to or even being aware of their theoretical implications. When the rules of thumb of clinical psychoanalysis are equated with the theory

of psychoanalysis, the observations and concepts which bridge the gap between the basic concepts and the initial observations are inevitably overlooked.

The psychoanalytic writer and practitioner is inclined to speak of psychoanalytic concepts and theories in terms of "content." The content of the word "queer" may serve as an example. The subject's use of this word is conducive (or seductive) to the conclusion that a homosexual impulse *may* be involved in the production of this slip. The *content* of any slip *may* suggest the nature of the unconscious motivation involved in it. Content is an important *guide* to the practitioner. Many sensitive and experienced psychoanalysts are to a great extent guided by the content of communications. Others are guided by the tone of voice or other expressions of emotion. The majority of the contributions to the literature tend to dwell on content to the neglect of other guides. What is lost sight of—and the practitioner need not necessarily keep this in focus or even in sight, but those interested in the theory must—is the functional (and thus also conceptual) relationship *to which* the content is a guide. The word "queer" is—by the circumstances of its utterance—a compromise formation between id motivations and ego controls. This is one of the functional relations involved in this slip. This slip's content suggests some of the (aggressive and homoerotic) impulses involved in this functional relation. But these suggestions make theoretical sense only if the word "queer" and the circumstances of its utterance are assumed to be referents of the descriptive concept of the unconscious, of the concept of unconscious motivation, of unconscious drive motivation, of unconscious conflict, and of resolution by compromise. No content yields its full meaning unless its formal characteristics, and those of the time, locus, and context of its appearance, are taken into consideration, that is to say, abstracted. The content of the word "queer," and of any communication, is predictive *only* in so far as it belongs to the network of the ideas which represent the drives involved. In other words, the content is a guide because it belongs to a network of formal relationships. Whether it is the Oedipus complex, or the castration complex, or an anal fixation, or a homosexual impulse to which the content refers, it does so always by virtue of a formal, conceptual relationship. The stress on content seems to be one of the main causes for overlooking the relationship between concepts and observables. Psychoanalysts are not the only ones who make a direct jump from content to unconscious motivation: Rorschach testers and other projective test "experts" do it too, often with less experience and always with less collateral information to go by.⁷⁶ Recently McClelland [226] called on psychologists to revive their interest in

⁷⁶ See, however, Schafer's [298] treatment of content as a guide to formal relationships.

content. McClelland is right, it is high time to begin the serious experimental study of contents. But a warning of the pitfalls is in place. The more familiar the contents dealt with by psychoanalytic theory become, the greater the temptation to bypass and to becloud the conceptual relations.⁷⁷

IV. CONSTRUCTION OF FUNCTION FORMS

Psychoanalysis, as a theory, did not make a formal study of the construction of its functional relationships. Thus whatever can be said about these must be inferred. The preceding sections covered this ground as much as seemed feasible. Here we can add only a discussion of the theory's "function form" in relation to the Lewinian and S-R function forms.

Lewin's [219] basic function form is $B = f(P, E)$: behavior is a function both of the environment and the person. Here B represents behavior at large and not any specified aspect of it; E represents the environment as the person's life space at large and not any specified part of it; and P represents the structural and tensional characteristics of the intrapersonal regions at the time of the behavior, rather than the person as a changing historical entity.

$B = f(P, E)$ can be made to take on extreme values which transform it into the basic S-R function form, if we make three assumptions: *First*, there are environmental situations in which behavior (response) is invariant in respect to individual differences and intrapersonal changes; then for all the E values for which this assumption holds, the function changes into $B = f(E)$. This situation is one in which the causal texture of the environment has become compelling. In terms of psychoanalytic ego psychology, this is a situation in which no autonomy from external reality obtains. *Second*, the E (life space) does not vary from subject to subject (such variations are not accounted for directly by this equation). *Third*, certain behaviors are determined not by the E in general but by

⁷⁷ Freud seems to have expressed this as follows, responding in a letter to Abraham on the latter's comments on "Mourning and Melancholia" [120]: ". . . you do not emphasize enough the essential part of my hypothesis, i.e., the topographical consideration in it, the regression of the libido and the abandoning of the unconscious cathexis, and that instead you put sadism and anal-erotism in the foreground as the final explanation. Although you are correct in that, you pass by the real explanation. Anal-erotism, castration complexes, etc. are ubiquitous sources of excitation which must have their share in every clinical picture. One time this is made from them, another time that. Naturally we have the task of ascertaining what is made from them, but the explanation of the disorder can only be found in the mechanism—considered dynamically, topographically and economically" [193, vol. 2, p. 329].

a specific element of it; that is to say, B is invariant in respect to E except for its single, specified element S . If we then use the term *response*— R —for the so specified behavior, and the term *stimulus*— S —for the so specified elements of E , we arrive at the equation $R = f(S)$.

Similarly, $B = f(P, E)$ can be transformed into the function form of that phase of psychoanalysis in which the role of reality was negligible, and the role of the person's drives and defenses paramount, in determining behavior. Embarking on such a transformation we must note that Lewin, in failing to establish in principle the relations between "geographic" and "hodological" (life) space, left the door wide open for all those who wish to transform E into a function of P and thus to turn the $B = f(P, E)$ equation into $B = f(P)$.⁷⁸ Werner and Wapner's "sensory-tonic" theory [323]—by postulating a tonic factor in perception—introduces P into the perceived E , and thus also leaves an opening (however narrow) for such a transformation. Others, particularly Brunswik, Heider, Gibson, and recently Klein [197], explicitly refused to do so. According to Piaget's studies, the "construction of reality" is an ontogenetic achievement and not a process of "imitative learning" or "conditioning." Thus in his theory, too, genetic considerations can always resolve E into $E = g(P)$, though here all such g functions are actually of the form $E_n = g_n(E_{n-1}, P)$, and P itself is subject to historical change [$P_n = h_n(P_{n-1}, E_{n-1})$]. Nevertheless, every genetic theory tends to transform $B = f(P, E)$ into $B = f(P)$: in genetic theories, genetic reductionism is always a temptation.⁷⁹

Can any dynamic psychology escape such a reduction? Allport's personalistic psychology has perhaps the most explicit safeguards against such a reduction, which turns man either into a mechanism ultimately at the mercy of its environment, or into a solipsistic creature ultimately at the mercy of his drives. Allport's [6, 8] safeguard against both these alternatives is expressed in his concept of functional autonomy, which implies that whatever the genetic (maturational or learning) history of a function, it may attain autonomy so that it can serve as an irreducible basis of behavior. In psychoanalytic theory, a similar solution was independently reached by Hartmann and expressed in his concepts of the conflict-free sphere and autonomous ego functions. However, it should be noted that his is a concept of *relative* autonomy: functions and structures have only limited autonomy from the drive or learning process from which they arose; for instance, behaviors *determined* by such structures may be *overdetermined* by drives; they may be used by drives as

⁷⁸ Brunswik and Heider point out that Lewin's environment is "encapsulated." G. S. Klein points out that Lewin disregarded the "*inherent structure*" of the environment and centered exclusively on its perceived structure.

⁷⁹ But see Erikson [61, 62, 66].

means; and under stress their autonomy does not necessarily hold and they may yield to ontogenetically earlier forms.⁸⁰ The implications of Piaget's [254] genetic theory of intelligence are similar: the new and higher-level "circular reactions"—and the "schemata" corresponding to them—attain independence from the schemata from which they arose. But the lower-order schemata and circular reactions are not *replaced* by the higher ones and may always be reactivated when the latter do not provide the means of coping with the situation encountered.

$B = f(P, E)$ implies that no broadly valid relationships of the $B = f(P)$ or the $B = f(E)$ type are possible. If the $B = f(P)$ function form is to attain validity for more than a narrowly specified range, E must be introduced into it as an intervening variable. The same consideration holds for the validity of $B = f(E)$, and demands the introduction of P as an intervening variable. But neither E nor P is a simple variable: both are functions of other variables, which can be held constant only for a narrow range of conditions. If we take it for granted that the person's experience of his life space is our basic information about his geographic environment, then E is a function of the geographic environment and P ; and if P changes—as it does—with experience, then it is a function of preceding P 's and E 's.

Let us approach the problem from another angle. The extreme values which make $B = f(P, E)$ go into either $B = f(E)$ or $B = f(P)$ imply that certain one-to-one relations between stimulus and response, drive and behavior are possible. But we know empirically that by and large this is an untenable assumption, since the single S (stimulus) is hardly ever the only effective factor in E and the single D (drive) is hardly ever the only effective factor in P . So when S or D is chosen as the independent variable, the other factors come into play as intervening variables. In other words, the remarkable thing about human behavior is that man often meets diverse stimuli by the same behavior, and identical stimuli often elicit diverse responses. Likewise with motivations: the same motive may be expressed by a wide variety of behaviors or satisfied by a variety of objects, and a great variety of motives may be expressed by the same behavior or satisfied by the same object [see Frenkel-Brunswik, 91, and Gill, 146]. Therefore, if stimuli or motives are used as independent variables, it becomes necessary to introduce intervening variables to account for the flux of the dependent variable. Thus, learning theory introduced sets, attitudes, etc., as intervening variables, to save the $R = f(S)$ function form. Where P at large is the independent variable, E will serve as the intervening one, and vice versa. In psy-

⁸⁰ This is only a possible consequence of stress, not a necessary one. Among others, Jacobson [189] and Bond [30] report observations of increased autonomy and efficiency under stress. See also [280].

chologies where the *P* is differentiated, and the motive (e.g., the drive) is the independent variable, structures (defenses, controls, etc.) will appear as intervening variables. In those psychologies where the *E* is differentiated, context and "setting" will appear as intervening variables.

In conclusion: by the very nature of psychoanalytic theory, intervening variables are indispensable in its function forms. It would seem that this holds true for all dynamic psychologies, and the range of intervening variables they will use will depend upon the degree of autonomy they assign to the structures and functions involved in the phenomena studied.⁸¹

V. THE PROBLEM OF QUANTIFICATION

It would be simplest to restate at this point that psychoanalysis as a discipline has not attempted quantification, and avoid the whole issue. One could express justified impatience with the "furor of measuring" which has gripped psychology, partly by referring to the history of evolution theory, in which precious little was measured for a long while, and partly by referring to how much is being feverishly measured in psychological laboratories without good reason. Sometimes one has the impression that the hope in such measuring is well expressed in the Hungarian proverb, "Even the blind hen does at times peck a grain."

But the issue of quantification cannot be dismissed lightly. Psychoanalysis—like all other sciences—orders, equates, compares, and distinguishes observables, and these procedures, once made precise, reveal themselves as mathematical operations [cf. Piaget, 252, vol. 1]. Thus all sciences, in striving to make their assertions precise, move toward a mathematization of the relationships they establish by their procedures.

Since mathematization may be either metric or nonmetric, quantification is only one form of it. In contrast to the customary quantification,

⁸¹ It can be argued that the use of intervening variables does not depend on the degree of autonomy the system assigns to structures and functions, but rather on the observational method by which the data the theory accounts for are obtained. For example, R. R. Holt (personal communication) comments: "Skinner does not need intervening variables because he forces behavior into a narrow compass where S and R can be directly related mathematically." It is probable, however, that Skinner's procedure, too, is rooted in a choice of autonomous (automatized) relationships. It would seem that the degree of autonomy, as well as the role assigned to intervening variables, depends on the observational method used. The psychoanalytic method alone scarcely allowed, and certainly did not require, the theory to introduce the concept of autonomy. It was introduced when data obtained by other observational methods were also considered by the theory. Conversely, Hebb [169] seems to have realized, when he considered methods of observation (e.g., Senden's) other than conditioning, that the S-R relation is not free (autonomous) from what passes between the S and the R.

Lewin [217] attempted to introduce topology and Piaget [253] to introduce group theory into psychology as nonmetric mathematizations. Nonmetric mathematization does not necessarily exclude quantification; indeed it is not clear whether Lewin's quantifications are the result of his success or failure in nonmetric mathematization. The problem of metric mathematization is what kind of quantification, if any, is appropriate to what kind of psychological relationship.

It is probable that psychoanalysis has not developed a mathematization, and that academic psychology has not gotten far with its strenuous efforts at quantification, because they are both sciences in an early phase of development. Whether it is metric or nonmetric mathematization that psychoanalysis is headed for is hard even to discuss at this point. It would certainly be premature to judge that quantification is the kind of mathematization which is appropriate to psychoanalytic theory. The following discussion of quantification implies no such judgment, but merely this attitude: since the question of quantification has been raised, and since quantification *may* prove to be the mathematization appropriate to psychoanalysis, some of the problems it involves should be explored. This section will dwell on two topics: on the quasi-quantitative concept of cathexis, which of all the concepts of the theory seems to call most urgently for quantification, and on the kind of quantification—if any—required by the theory.

A. Cathexes⁸²

The psychoanalytic theory contains quasi-quantitative concepts. The most conspicuous of these are the drives, which are conceived of as forces, and the cathexes they expend, which are conceived of as quantities of energy. Why then have these not been measured? To answer this question it is necessary to discuss the distinctions psychoanalytic theory makes between various forms of energy.

1. The muscular energy of behavior is not the psychological energy that psychoanalytic theory speaks of: the psychological forces which in their work expend psychological energy only *release* the forces that expend the biochemical energy of muscles.

2. Psychological energy (in the main) is considered as of drive origin, and to account for its major forms of manifestation, two transformation processes are postulated: binding and neutralization. Both of these result in forms of energy (*bound, neutralized*) which differ from the original (*mobile*) form of drive energy.

3. These three forms of energy, and the two major processes of transformation, may be characterized as follows:

⁸² For references, see pp. 91–93, 113–114, above.

a. Mobile energy abides by the pleasure principle: it tends toward immediate discharge by the shortest route, and brooks neither delay nor detour. These direct discharges may take the form of action, idea, or affect (e.g. impulse action, compulsive ritual, random action; delusion, obsessional idea; and uncontrolled affect storm, such as a tantrum or panic, etc.). The processes which expend mobile energy are conceptualized as primary processes. They make use of several *mechanisms* (i.e., specific transformations of energy) as, for instance, condensation, displacement, substitution, symbolization, etc. These mechanisms come into play in all facets of behavior, though they are commonly illustrated by examples taken from ideation (e.g., dreams). Mobile energy, though its function is particularly well illustrated by the behaviors mentioned above, does not refer to a class of behaviors but to a component of all behavior.

b. Bound energy is defined as energy tied up in structures. Breuer [35, pp. 140–141] compared it with the tonic innervation of muscles. The structures, the building of which amounts to a binding of energy, are conceptualized on the one hand as those controlling and defensive structures of the ego which make ordered thought as well as controlled affect and goal-directed behavior possible, and on the other hand as those which are the means (information; habits; concepts; anticipatory, grammatical, syntactic, and logical patterns, etc.) used by ordered thought, controlled affect, and goal-directed behavior. The processes made possible by these defensive and controlling structures, and by these structures of means character, are conceptualized as secondary processes. The transformation of binding changes mobile energies into bound energies. The structures thus created counteract the mobility of unbound energies, and also serve as the means (apparatuses) by which the latter are expended and controlled. Compared with the great energy expenditure in primary processes, the structures formed by binding can function (autonomously) with a minimal expenditure of psychological energy, and by controlling the discharge of mobile (great intensity) energies they create high potentials for action. Like physical mechanisms, they transform, save, and expend energy. The concepts of the binding process and of the structures which it creates account for that aspect of the psychological organization which does not reduce, but maintains or even increases, tension [see Freud, 98, pp. 533–534; and Allport, 8].

c. Neutralized energy is defined as energy whose tendency to follow the pleasure principle (direct immediate discharge) is decreased. This definition implies a spectrum of energy forms, ranging from barely neutralized to highly neutralized energies. The process of neutralization is defined as the transformation by which drive energies, the *ideal type* of which is considered nonneutralized, are transformed into energies of

various degrees of neutralization. The discharge (entropic) tendency is common to all energy (physical as well as psychological): how can we conceive of energies which abide by it only more or less? The limitations of our systematic knowledge permit only an answer by analogy:

The entropy principle certainly obtains for closed systems of physical energy, but organisms, like other open systems and like man-made mechanical structures, postpone and obstruct the operation of the entropic tendency. Organisms do this by being structured and by building further structures [cf. Schroedinger, 305]. Man-made structures do it by preventing expenditures of kinetic energy (as in dams) and thus transforming it into potential energy, and by controlling the expenditure of kinetic energy with small variations of potential energy (as on the grid of the electronic tube).

Neutralization is considered to be the result of structure building by the process of binding [see 274, and 268, particularly part 7]. These structures, by raising the discharge thresholds of drive energies and by building new controlling "dams," obstruct the tendency toward direct discharge, enforce delay and detour, and thus give rise to derivative motivations whose tendency toward immediate and direct discharge is decreased: thus, a step toward the neutralization of cathexes is made. With further structure building, further derivative motives arise, which expend cathexes of an even higher degree of neutralization.

Observations also necessitate the assumption of transformations which reverse the effects of binding and neutralization. These observations pertain on the one hand to the weakening of controls and defenses, and on the other to the so-called libidization or aggressivization of functions and structures. These transformations may be termed "mobilization" or "deneutralization."⁸³ Referents of these transformations may be observed in special normal states (e.g., dreams), in pathological conditions (e.g., compulsions and delusions), etc.

The complexity of this theory of psychological energies and of their relationship to the motor energy of behavior has far-reaching consequences for quantification. The motor energy of behavior is "controlled" and "released" by the economics of psychological energies and by the corresponding dynamics of the psychological forces which operate through psychological structures. It might be suggested that this relationship is akin to the control of large amounts of energy (muscular) by an information network operating with smaller amounts of energy (psy-

⁸³ They occur in the process of regression. Freud discusses them as the dissolution by regression of the fusion of libidinal and aggressive drives (defusion) [see 131, pp. 46-48; see also 148, chapter on "The Metapsychology of Hypnosis and Regression"].

chological). In turn, within the range of psychological energies we find a similar relationship: the expenditure of large amounts of energy (mobile drive energy) is controlled by a network operating with smaller amounts of energy (bound and neutralized energy). Moreover, since the processes of binding (structure building) and neutralization recur, creating ever new layers of the ascending hierarchy of psychological organization, we are faced with a whole array of controlling networks arranged in depth.

One of the obstacles to quantification now becomes obvious. Overt behavior is as a rule a remote representation of the psychological processes which give rise to it. Thus, even though observations strongly suggest the need for concepts like cathexis and those referring to cathectic transformations, the measurement of their referents is made difficult by the circumstance that the same set of observations necessitates the assumption of a complex hierarchical control organization regulating cathectic expenditure and transformation by means of structures operating with small amounts of cathexis. If the theory were systematically tight, its definitions explicit, and its implicative rules specific, the discontinuities, resulting from the multiple controls which cathectic expenditures are subject to, would not obstruct quantification. But the theory is far from being that tightly knit. The best index of the theory's looseness is that the volume of its experimentally verified propositions would be ample to confirm a tighter theory.

Yet the situation is not as hopeless as the complexities described suggest. The theory of cathexes does include quasi-quantitative propositions in the form of inequalities. For instance, the following inequality holds for mobile cathexes: in drive action, the quantity of cathexis is greater than that in affect charge, which in turn is greater than that in an idea. Such a series of inequalities is *per se* a primitive (intensive) quantification⁸⁴ and this kind of quantification is inherent in the theory. For instance, there is no reason why the degrees of mobility of cathexis (or conversely, the degrees of neutralization) should not be expressed in terms of such inequalities. Indeed, R. R. Holt's [178] study of primary process manifestations in the Rorschach test did just that. Such ordinal scaling, using the psychologist's rating procedures, seems *for the present* the quantification method of choice for the primitive quantitative relationships of the theory. Some of its difficulties, however, should be mentioned here:

Ordinal scaling of primary process phenomena may distract attention from the fact that the theory does *not* posit a simple continuum of neutralization. The decrease of mobility goes along with binding (struc-

⁸⁴ Cf. Piaget's [252] discussion of intensive quantification. Altogether, Piaget's discussions of the development of quantity concepts and its relation to logic and mathematics are relevant to the quantification problems of psychology.

ture-building), that is, with the establishment of new hierarchic levels which differ from each other not only in the degree of mobility of cathexis, but also in their structures and in their kinds of motivations. While the degree of mobility remains a common parameter throughout the various hierarchic levels, the qualitative differences in structures and motivations from level to level make it difficult to find that feature of behavior which, when rated and scaled, will quantify that common parameter. Whenever the behavior feature chosen for rating is not appropriate, apples and pears will be compared. R. R. Holt seems to have avoided this pitfall, but it required a considerable mastery of the theory to do so, and the measures used remained gross.

Thus, individual instances of primary process phenomena do not offer an insurmountable obstacle to quantification. But how about a behavior segment (a Rorschach or a TAT record, or a clinical interview) which contains several such instances? Each of these can be rated. But may we count them? If we do, what is our justification for doing so? If we decide to weight them, are the weights additive? These questions are not yet answered. We do not even know where the answers will come from. We may have to accept purely empirical, theoretically unsupported answers for a long while, in the hope that these will show us that the theory has (or can be expanded to have) the answers. It is also possible that the empirical answers will radically change the theory. This problem is not specific to Holt's study. In food deprivation studies using TAT stories, we find individual differences in the stories of a group of equally deprived subjects: some stories contain much material *distantly* related to food, others contain little food-related material, but what there is, is *closely* related. Can the ratings of these individually differing products be added up? The relationship between drive intensity (amount of cathexis) on the one hand and the frequency and intensity of its indicators on the other is a significant unsolved problem of quantification.

B. Dimensional Quantification⁸⁵

What are the general prospects for the quantification of the variables of this theory?

Before attempting an answer to this question, let us state that the urgent tasks of this theory are in the relationships it posits which require systematization, and in the areas which require new observations. Without stressing that, among other things, much of ego psychology is still uncharted territory, and that our knowledge of affects is in urgent need of systematization, etc., the very discussion of quantification may

⁸⁵ Since the completion of this manuscript, A. Menkes and J. Menkes have published a paper [231] which contains an example of this kind of quantification and goes a considerable way in demonstrating the necessity for such.

misrepresent the actual situation: mathematization in general and quantification in particular require a systematized and tightly knit theory.

When the physicist measures, he knows the dimensions of his observables as expressed in terms of the CGS (centimeter, gram, second) system, and when he establishes a constant he knows that its dimension is such as to make his equation not only quantitatively but also dimensionally true. In $s = (g/2)t^2$ the dimension of s is C , of t is S , and of g is C/S^2 ; thus, substituting these dimensions, we get $C = (C/S^2)S^2$, indicating that the equation is dimensionally true. The classic scale of *hardness* is a means of quantification too. But instead of a dimensional measure, it provides only an *ad hoc* quantification. Most—if not all—measurements (e.g., IQ's) of present-day psychology are *ad hoc* quantifications. Without a systematized theory, no dimensional quantification is possible. In physics, nobody would try to test a theory by a measurement without first ascertaining the relevance of what he measures and how he measures it. The dimensions are the criteria of relevance. Psychologists, however, “test” psychoanalytic propositions without studying and systematizing the theory which gives meaning to these propositions. Theories can be tested only when they are taken seriously. To test is to mathematize and to mathematize is to discover, in the relationships posited by the theory, relationships of a higher order of abstraction. Such abstractions cannot be derived from isolated propositions, but only from the system of relationships which link these to each other.

So far we do not know how to achieve a dimensional quantification of psychoanalytic variables; and yet we cannot sit with folded hands, since additional observations are needed for the systematization of the theory and for dimensional quantification. Thus in gathering new observations we must be satisfied with *ad hoc* quantifications, but we must not lose sight of the goal of dimensional quantification. To achieve that, we will have to learn to consider the locus of our variables in the motivational and structural hierarchy and to play variables against each other so as to arrive at equations which represent actual balances of forces, or balances between structures and forces, etc. Progress toward dimensional quantification will at every step require long series of experiments which vary the experimental conditions systematically. The currently fashionable one-shot experiments (probably fostered both by the premium put on publication and by the publication policy of psychological journals) militate against progress toward dimensional quantification. One-shot experiments, naturally enough, use *ad hoc* quantifications, and only rarely cogwheel into the *ad hoc* quantifications of other experiments. Lewinian experiments in affect and action psychology avoided this pitfall to some extent and showed how *ad hoc* relationships can be avoided by systematic variation of experimental conditions directed by a cohesive

theory. But the reports of these experiments are in German and thus have been little read, except in Ellis's [52] excerpts or Lewin's [216] summary, neither of which conveys the method.

One of the banes of *ad hoc* quantification is that even when it yields statistically reliable results, these may be due to sheer luck in choosing the experimental tasks and subjects. Even the apparently precise replication of an experiment may bring different results. The crucial dimensions not being known, unnoticed "minor" variations of the setup affect the results. In other words, without knowing the dimensions involved it is impossible to predict what changes will make for hierarchic differences and for what types of subjects will the objectively "precise" replication amount to a radically different setup. George Klein [197, 200] has shown that something of this sort was involved in the "now you see it, now you don't" character of the Bruner-Goodman [37] effect.

Now, as to the possibility and prerequisites of dimensional quantification: *First*, dimensional quantification in psychology may *not* be feasible. We would be reluctant to entertain this possibility, partly because it would require negative proof, which is difficult if not impossible to obtain, and partly because it would discourage further research. *Second*, the quest for dimensional quantification may lead to a nonmetric mathematization. *Third*, a dimensional quantification may develop.

The avenue by which we may be able to arrive at a choice among these three possibilities will—to my mind—be paved by a new start on the problem of learning:

The physical dimensions are mass, space, and time. Physics expresses both the movement of mass (i.e., its changes of position in space and time) and the changes in the *structure* of mass, as well as the gradients and causes (force, energy) of such changes, in terms of these dimensions. If we were to have psychological dimensions, they too would have to be able to express psychological processes as well as psychological structures and their changes. In psychoanalytic theory, structures play such a crucial role that as long as the propensities and changes of psychological structure cannot be expressed in the same dimensions as psychological processes, dimensional quantification is but a pious hope. In other words, the study of the process of psychological structure formation seems to be the prime requisite for progress toward dimensional quantification. We must establish how processes turn into structures, how a structure, once formed, changes, and how it gives rise to and influences processes. This could be achieved, for instance, by studying the processes by which Hebb's hypothetical structures (assemblies and phase sequences) are formed and changed, as well as the processes by which these structures change new ongoing processes. Likewise, this could be achieved by the study of those structures whose genesis

and function psychoanalysis conceives of as follows: when drives encounter an obstacle to the discharge of their cathexes, structures are formed and these structures thereafter serve both as obstacles to (defenses against) and controls and means of discharge. These examples refer to changes wrought by experience. Whether or not all structure formation (in that broad sense which takes account of the epigenetic-maturational matrix)⁸⁶ should be considered learning (i.e., abiding change wrought by experience) is both an empirical and a conceptual problem. But it seems that all learning may be looked upon as a process of structure formation. The processes of verbal learning and habit formation may well be considered subordinate to this broader category, though their study may or may not be revealing of the relationship between process and structure.

What study will reveal this relationship? Thirty years ago, Adams [2] suggested that the main obstacle in the way of the study of the learning process is its slowness. Hebb [169] pointed to the slow rate of early learning processes. The burden of Piaget's [254, 255, 256, etc.] developmental studies in and since *The Origins of Intelligence* is the same. It is possible that only longitudinal studies can clarify the relationship between process and structure. But since the methodology of longitudinal studies is still obscure, this is a dim hope. Perhaps the answer will come from a new attack on learning as structure formation, which will take account of Hebb's assumption that late learning operates by recombining already established "phase sequences" and will thus center on the changes in, rather than on the origin of, such phase sequences.

The immediate outlook for an early clarification of the process of structure formation seems none too rosy. Yet this clarification appears to be the prerequisite for dimensional quantification in psychoanalysis in particular, and perhaps even in psychology at large.

But the quest for dimensional quantification must not amount to a disdain for *ad hoc* quantification. The latter seems to be a step toward the former, provided it is clearly understood that *ad hoc* quantification itself does not locate hierarchically the structures and functions which it crudely quantifies. The possibility of arriving at a dimensional quantification can be kept open by matching the care and ingenuity expended on *ad hoc* quantifications with an unremitting alertness for the hierarchic locus of the relationships so quantified.

All this discussion of quantification is, however, in a sense abstract and sterile. A proper discussion would have to start out with an analysis of the experimental literature pertaining to Freudian propositions. We have several surveys of this literature [e.g., 308, 173], but their concern is: what psychoanalytic propositions are confirmed by "objective studies?"

⁸⁶ Cf. pp. 86-88, above.

A survey which could advance the solution of the mathematization or quantification problem would have to center not on the *results* of these studies but on *their method*, on the variables which were *the targets* of quantification in them, and on *the technique* of quantification used by them. Short of a breakthrough by means of experimental ingenuity coupled with thorough theoretical grounding, such a survey seems to hold the best promise of progress toward the solution of the quantification problem.

VI. THE FORMAL ORGANIZATION OF THE SYSTEM

A. The Present Status of the System

The theory of psychoanalysis grew by successive spurts in the fifty years of Freud's work. Additions and revisions make it appear more like a patchwork than an architectonic design, since their consequences for the structure of the system have often remained a matter of a passing comment by Freud or isolated papers by other psychoanalysts. This in itself bespeaks a looseness of the theory and its lack of an explicit canon according to which revisions and additions are to be fitted into its system. Yet psychoanalytic theory does have an impressive structural unity, though it is hidden under the layers of progressive additions and modifications, and has not been disentangled and independently stated.

The "revisors" of Freud's theory further obscured its structural unity. Jung and Adler, who created relatively independent theories, failed to give these a systematic form which could have sharply distinguished them from Freudian theory. The situation is even worse with the "revisions" of Stekel, Rank, Sullivan, Horney, M. Klein, Kardiner, Alexander, French, Reik, Fromm, Rado [see Munroe, 240]. While each attacked and denied certain Freudian propositions, and replaced them by others (which often contained a valid core), none of these authors stated how their revisions affect the theory as a whole. Some of them (Stekel, Kardiner, Alexander, French, Reik) have asserted that their revisions do not affect the rest of the system, although they made no attempt to demonstrate this. Others (Rank, Horney, Sullivan, Rado) have implied that Freud's system has been replaced by their own, although they never presented a full elaboration of their systems. No Neo-Freudian has taken cognizance of, and has integrated his own contribution with, the whole of psychoanalytic theory. Nor is there a single attempt to replace it with a whole system that demonstrably accounts for all the phenomena psychoanalytic theory claims to explain. Such an attempt could obviously include a demonstration that some of the problems psychoanalytic theory dealt with are pseudoproblems which can be ignored. The lack of an explicit

statement of the theory is as much responsible for all this as are the "revisors" themselves, who may have felt that they were not obliged to disentangle the system before they revised it. Study of Neo-Freudian writing often makes one wonder whether the authors were aware of the existence and nature of the implicit system of psychoanalytic theory.

There are three outstanding rudimentary statements of the theory's system.

First, Freud's seventh chapter of *The Interpretation of Dreams* [98] and his "Papers on Metapsychology" [108, 110, 114, 115, 116, 117, 119, 120] are attempts to present the system. One of the most puzzling problems of the history of psychoanalysis is why they were so little noticed. The fact that the form of these attempts is not systematic does not seem to explain this fully. The formulations of the present essay derive from these writings, and so do the other attempts at systematization to be mentioned here.

Second, Fenichel's *The Psychoanalytic Theory of Neurosis* [73], while it is focused on the clinical theory of psychoanalysis, does persistently invoke the general theory and thus gives a sense of its system. Yet the latter remains implicit, and the experimenter who wishes to start from Fenichel's formulations must first disentangle them from their clinical matrix. With Fenichel's death, psychoanalysis lost one of its few systematizers. His essay on the theory of technique [72] is a beginning of the systematization of the theory of therapy. His posthumously published *Collected Papers* [77] contains systematic discussions of M. Klein, Kaiser, Fromm, and other "revisionists." These discussions, as well as his paper on Freud's theory of the death instinct, show that the psychoanalytic theory is sufficiently cohesive to permit systematic exclusion and inclusion of new contributions.

Third, the development of ego psychology is perhaps the clearest demonstration of the systematic nature of psychoanalytic theory. In Anna Freud's [93] work the clinical theory of defenses begins to take a systematic form; in Erikson's [61] work the development of the ego and the psychosocial theory of psychoanalysis takes shape; and in Hartmann's [157] work [complemented by Kris's and Loewenstein's, 167, 168, 206] the theory of the ego develops hand in hand with a progressive crystallization of the general theory of psychoanalysis. All these contributions show that psychoanalytic theory can grow organically so as to include the valid observations and formulations of the Neo-Freudians, without becoming an incoherent patchwork and without the necessity of discarding any of its major segments. They demonstrate that the theory has sufficient systematic coherence not only to reject incompatible solutions, but also to develop compatible theories of the ego, of reality, of interpersonal relationships, and of social psychology.

B. The Desirable Level of Formalization

The desirable level of formalization is, in a sense, an empirical question. Since everybody wants to be on the side of the angels, we may assume that reaching maximal explicitness is the ideal and only the limitations of our knowledge stand in the way. Newton's axiomatization was explicit and its heuristic value shows that it was desirable. But the systematic and heuristic value of Einstein's last formalizations is much questioned. Present-day physics has no unified axiomatic system. All in all, probably only experience can decide when and how far axiomatization can be *meaningfully* pushed in psychology or in any other science.

Yet it may be worth while to raise the question: why are psychologists so concerned with axiomatization? Actually, axiomatization has always been a late product in every science. Centuries of Egyptian geometry preceded Euclid. Newton had not only Galileo and Kepler, but thousands of years of physics behind him. Sciences do not arise from, but culminate in, axiomatics. Axiomatic systems do not reveal the tracks of a science's development: they conceal them. They do what so many psychologists do who arrive at their results with great difficulty (like the rest of us), but from reading their published papers one would never guess that; they seem to reveal a foresight which puts to shame all others who deal with human beings or govern human affairs.

Does the yearning for axiomatization mean that psychologists believe psychology can arrive at its future by lifting itself by its own bootstraps? Are we really to believe that we can guess our way through to axiomatics and bypass the long road other sciences have had to travel toward it? Beat the other sciences at the game? Or just simply profit by their experience? But what if our guesses lead to a disregard of the empirical evidence we already have and to a lack of concern for the evidence that is not yet in? What if the attempts at short-cutting the arduous path of development lead only to endless detours—much longer than the “long and hard” empirical route, and futile, to boot? Is it possible that psychologists ignore what the natural scientist [24] and the historian of science [46] have come to recognize: that scientific discovery starts from intuition and not from deduction?

This is not to question that psychology can profit by the experience of other sciences, nor to make light of axiomatics as an ideal, nor to minimize its importance in the development of sciences, nor to contend that theory making (including axiomatics) is not as essential to science as “measurement”: intuition or hunch *is* theory. The point is that in present-day psychology the *measuring furor* seems to have made an unholy alliance with an *axiomatic furor*, and between the two of them they may well doom psychology to stagnation.

Piaget in his *Epistemologie Genetique* [252] examined both the history of mathematics and the ontogenetic development of mathematical reasoning in an attempt to explain how mathematics can be simultaneously deductively rigorous and yet fertile. The study of his investigations is a good antidote to premature axiomatization.

Psychoanalysis is in sore need of systematization, because without it the experimenter is likely to continue to test isolated and misconstrued propositions, unaware of their actual theoretical context. But systematization is a long way from formalization and axiomatization. Much additional knowledge will have to accumulate before we can even begin to work on the latter tasks.

VII. THE RANGE OF THE SYSTEM'S APPLICATIONS

The theory, though it originated in the study of pathology, has always claimed to explain normal behavior and development also—*Psychopathology of Everyday Life* [99], *Wit and Its Relation to the Unconscious* [100], "Humour" [134], *Three Essays on the Theory of Sexuality* [101].

Moreover, Freud demonstrated that the theory and its methods can be fruitfully applied to anthropology and prehistory [*Totem and Taboo*, 109]; to the study of literature [*Delusion and Dream*, 102, "The Relation of the Poet to Day-dreaming," 103, "Dostoevsky and Parricide," 133, "The Theme of the Three Caskets," 112]; to the study of art [*Leonardo da Vinci*, 105, "The Moses of Michelangelo," 113]; to the study of mythology, folklore, and legend [*Totem and Taboo*, 109, "A Mythological Parallel to a Visual Obsession," 118, "Medusa's Head," 125, "The Occurrence in Dreams of Material from Fairy-tales," 111]; to the study of language ["The Antithetical Sense of Primal Words," 104]; to the study of religion [*Totem and Taboo*, 109, "A Religious Experience," 135, *The Future of an Illusion*, 132, *Moses and Monotheism*, 141]; to the study of history [*Moses and Monotheism*, 141]; and to the study of society [*Totem and Taboo*, 109, *Group Psychology and the Analysis of the Ego*, 124, *Civilization and Its Discontents*, 136, "Why War?" 138]. Finally, Freud at various times asserted the applicability of his method and theory to those phenomena which we subsume under the term psychosomatics.

Indeed, Freud considered all human behavior and endeavor to be within the purview of psychoanalysis. Psychoanalysts followed his lead and their literature abounds in papers and books dealing with the fields listed. Even though these contributions aroused heated and often acrimonious debate, and even though their cogency and their scholarship in the field in question have been criticized sharply and often rightly,

the present situation in all of these fields seems to bear out Freud's early claim.

In summary, psychoanalytic theory has asserted an all-inclusive applicability to the study of man. Psychoanalysts have acted to make this claim good. Investigators in the various fields approached by psychoanalysis have adopted some of its methods, concepts, theories, and outlook. There is sufficient evidence that the claim has a substantially valid core.

Now we come to the applications of psychoanalysis to psychology proper. Here we can give only a brief sketch of the complex problems involved [see Shakow and Rapaport, 309].

Though Freud conceived of psychoanalysis as a general psychology, little in his theory pertained directly to psychophysics, learning, and perception, the areas central to academic psychology, and he did not attempt to apply his theories or methods to psychology at large.

At first only a few psychoanalysts showed an interest in psychology: for example, Schilder [299, 300, 301, 302], Bernfeld [13], de Saussure [297]. But through developmental psychology [Piaget, 247, 248, 249, 250, 251, and Werner, 322], through early clinical-experimental psychology [Murray, 242, 243, and Rosenzweig, 293, 294], through the influence of projective techniques on clinical psychology [Rorschach, 292, Morgan and Murray, 238, and others], through learning theory [Dollard and Miller, 48; Hull, 183; Mowrer, 239],⁸⁷ and through psychologists' growing interest in psychotherapy, psychoanalysis came to exert a powerful influence on psychology proper. Most of this influence did not stem from psychoanalysts' applying their theory and methods to psychology (Jung, Rorschach, and Murray may be considered exceptions) but rather from psychologists' attempting to use the conceptions (rather than the concepts)⁸⁸ of psychoanalysis.

Only with the development of psychoanalytic ego psychology did psychoanalysis begin to acquire means for dealing with the usual problems of psychology. Hartmann [157] then made it explicit that psychoanalysis *is* a general psychology, that its interest and application extend to the field of academic psychology, and proceeded to link psychoanalytic and psychological propositions to each other. Subsequently, several psychoanalysts and psychoanalytically trained psychologists continued to relate psychological and psychoanalytic conceptions, theories, concepts,

⁸⁷ For earlier psychoanalytic influence on learning theory, see E. B. Holt [174, 175], Kempf [196], Humphrey [184, 185], Troland [318], and others.

⁸⁸ Concepts are terms defined within the framework of a theory, conceptions are terms and formulations which either precede the definition of the concepts in the history of a theory or disregard them. Thus statements of conceptions use the terms of a theory in an imprecise or arbitrary "common-sense" fashion.

and methods to each other. It is to be hoped that, as a consequence of this work, the haphazard "experimental testing" of psychoanalytic theories and their untested application by clinical psychologists may eventually give way to their systematic application to psychology, within the framework which psychoanalytic ego psychology has begun to build. To advance this work of mutual application, the theory of psychoanalysis must face two major tasks besides systematization: coming to terms with Piaget's theory and developing a learning theory.

If Piaget's [252, 254, 255, 256] findings are confirmed, psychoanalysis will have to come to terms with his developmental theory as an indispensable segment of the theory of ego development. The problems to be solved before this can be accomplished cannot be sketched here.⁸⁹

Our discussion of learning (see Section V. B.) suggested that dimensional quantification *may* not be possible without a prior clarification of the process of structure formation and learning. But the solution of the problem of structure formation may also be one of the prerequisites for a unified theory of cognition (including perception), for the clarification of the methodology of developmental studies, and perhaps for the solution of still other issues crucial both for the systematic development of psychoanalysis and for the mutual and fertile application of psychological and psychoanalytic methods and theories.

VIII. HISTORY OF THE SYSTEM'S RESEARCH MEDIATION

It would take volumes to sketch and critically appraise all the research that has been "mediated" by psychoanalytic theory, by hunches derived from it, questions raised by it, and methods originating in it. It is not feasible to list even the highlights of such research in the fields mentioned in the previous section. Therefore, we will restrict ourselves to a cursory survey of its research mediation in psychodynamics and psychology.

There are, first of all, the clinical studies which fill the psychoanalytic as well as the Neo-Freudian literature. Moreover, it has been a long time since any clinical psychiatric case study could be made without drawing on psychoanalytic theory, which, to a greater or lesser extent, has mediated much of what passes today for clinical psychiatric research. Finally, the psychosomatic investigations of the last two decades arose, in the main, from psychoanalytic studies of organ neuroses, were nursed to a more or less general acceptance by the work of psychoanalysts like F. Deutsch, Alexander and French, Dunbar, and Binger, and have been turned into everyday clinical research by the efforts of Kubie, Kaufman, M. Lewin, Romano, and many others.

⁸⁹ See P. H. Wolff's [328] study of Piaget's theory and his discussion of its relation to psychoanalysis.

A selection from one area of more or less systematic studies which issued from psychoanalysis proper was collected and reviewed in *Organization and Pathology of Thought* [268], but a full survey of all such studies has not yet been made.

Projective techniques, which have come to play an increasing role both as subject matter and as tools of psychological research, had their origins in psychoanalytic theory. Rorschach and Murray were steeped in psychoanalysis and their tests are informed by psychoanalytic conceptions. In fact, these tests came into clinical use carried by, and carrying, the impact of psychoanalytic theory; they used segments of that theory for their rationale and interpretation [176, 281]; and they were used to "test" psychoanalytic propositions. Moreover, both these and play tests (deriving from the play techniques of therapy) bred a vast array of new projective tests, founded on and "testing" further psychoanalytic conceptions. How valid their connection to and their "testing" of psychoanalytic assumptions were, need not concern us here: "research" was mediated.

Throughout the last forty years, psychoanalytic theory has led to an extensive array of experimental studies on the effect of emotions and motivations on memory [see 258]. Most of these intended to test the psychoanalytic theory of repression, but many failed to distinguish this from hedonistic pleasure-pain theories or from the law of effect, and few if any were really conversant with it.

A related area of research mediated by psychoanalysis is that of motivated perception. Murray [242], N. Sanford [295, 296] pioneered, and Murphy and his pupils [see survey in 241, chap. 15] continued this line of investigation, which led to the "new look in perception," beginning with Bruner's [37, 38, 39, 40, 41] work and reflected in Blake and Ramsey's [29] volume. Among these, from the point of view of this essay, the work of Klein and his associates^{89a} stands out. While all these studies bear the imprint of the interest in motivation aroused in psychology by psychoanalysis [cf. Boring, 31, pp. 693, 713], the Freudian influence is not always as obvious in them as is the influence of Freud's motivation theory in the studies of Murray and Sanford, and that of Freudian motivation and ego theory in the work of Klein and his associates.

Psychoanalytic theory was also responsible for the reawakening of interest, during the last twenty years, in the nature of hypnosis and in the use of hypnosis as an experimental method. M. Erickson's [53, 54, 55], Farber and Fisher's [68], and Gill and Brenman's [33, 148] hypnotic work, as well as Fisher's [83] work with waking suggestion, represent efforts to apply psychoanalytic theory to hypnosis or to use hypnosis as a means of psychoanalytic exploration.

^{89a} A survey of these will be found in Klein's [198] volume soon to be published.

Psychoanalysis stimulated and guided more or less directly many longitudinal and cross-sectional developmental studies (Benjamin, Escalona, Spitz, K. Wolf, and others). This field is so broad that neither a further listing of investigators nor a bibliography can be given here. But a reference to Piaget must be made. Piaget's early work (up until *The Origins of Intelligence*, 254, in 1935) on autistic thinking and its socialization in children was strongly influenced by the psychoanalytic theory.⁹⁰ Piaget's later work is critical of psychoanalysis, but still appears to show its influence.

Finally, psychoanalysis—for better or for worse—has also mediated much research along the lines of the learning theories which originated at Yale. Whatever view one takes of their ultimate pertinence to psychoanalytic theory, Miller's experimental work [234, 235, 236], Miller and Dollard's studies [237], and Mowrer's experiments [239], as well as those of their many students, certainly arose under psychoanalytic influence.

But this enumeration of major areas of research mediation by psychoanalysis in psychology disregards, among other things, social psychological research (e.g., on authoritarianism) and does not do justice to the pioneering work of D. Levy [212, 213], Halverson [155, 156], Murray [243], J. McV. Hunt [186, 187], and many others. Regrettably, the existing surveys—Sears [307, 308], Rapaport [258], Hilgard [173]—are either specialized or incomplete. A careful analytic survey of the pertinent experimental literature would be a formidable undertaking: the amount of literature on research purporting to be related and on research actually related to psychoanalytic theory is immense. Yet such a survey is urgently needed. It would be of most use if it were to center neither on the design of the experiments nor on their results, but rather on the relation of the methods used to the theory.

IX. THE EVIDENCE FOR THE SYSTEM

A. Current Status of Positive Evidence

The major body of positive evidence for the theory lies in the field of accumulated clinical observations. The first achievement of the system was a phenomenological one: it called attention to a vast array of phenomena and to the relations between them, and for the first time made these appear meaningful and amenable to rational consideration. In regard to these phenomena and relationships, the accumulated clinical evidence is positive and decisive. The situation is different, however, in regard to the theoretical propositions of the system. While the evidence

⁹⁰ See the introductions to these volumes and their other references to Freud.

in respect to these also seems massive and imposing, the lack of clarification as to what constitutes a valid clinical research method leaves undetermined the positive evidential weight of the confirming clinical material. In spite of the various discussions [e.g., Brenman and Gill, 34, 147; Kubie, 209, 210; Benjamin, 11; Escalona, 67, etc.] on the nature of clinical research, and in spite of French's [89] extensive attempt to exemplify the method, its principles have not yet been expressed in the form of a canon. Indeed, many psychologists would question whether there is or can be any other canon of research than the experimental. Since it is questionable whether there exists such a thing as *the* experimental canon, these views need not worry us. Because a canon of clinical investigation is lacking, much of the evidence for the theory remains phenomenological and anecdotal, even if its obviousness and bulk tend to lend it a semblance of objective validity. This makes it urgent to re-investigate Freud's case studies with the aim of clarifying whether or not they can yield a canon of clinical research at the present stage of our knowledge.

In the lack of a canon for clinical research, it is difficult to accept as positive evidence observations which must first be interpreted before it becomes clear whether or not they confirm the predictions of the theory: we must be wary lest we smuggle in the confirmation through the interpretation. Axiomatization and/or a canon of investigation protect other sciences from such circularity. The lack of such safeguards is a real handicap for this theory, since by the very nature of the relation between observations and theory, only observations pertaining to basic concepts and theorems can be free of interpretation (cf. pp. 116-121). For instance, one of the major propositions of the psychoanalytic theory, confirmed by observations, is that there are two kinds of mental processes: primary and secondary. Little or no interpretation of the observations is needed to demonstrate that pathological, dream, or drug states bring to the fore mental processes which do not abide by the laws of ordered logical thought. But only on this low level of abstraction is the evidence conclusive without interpretation. As soon as the evidence for the *mechanisms* of the primary process is tackled, observation and interpretation begin to shade into each other. *Per se*, that should not invalidate the evidence, since no science can get along without interpreting its findings. Yet in psychoanalysis the difficulty is that the canon of interpretation itself is in question—or at least not beyond question—and it is likely to remain so until the nature of the clinical method has been clarified, or until experimental methods have been found which provide an independent base for the theory. As things stand, there is no canon whereby valid interpretation can be distinguished from speculation, though *post facto* the experienced clinician can distinguish them rather well.

We have a few experiments which are free of this difficulty. The experiments on dream symbolism [306, 288] and the related observations of Silberer [310; see also Rapaport, 276] are the outstanding ones. But these experiments remain phenomenological in that they demonstrate the *existence* of symbolization, rather than the *specific conditions* of its occurrence. The Poetzl [257] experiment and Fisher's [84] replication of it, as impressive as they are, involve interpretation.⁹¹

Most of the experimental evidence for the theory is questionable, even if Sears' survey [308], which was loaded with negative bias, was inclined to accept some of it as positive evidence. The overwhelming majority of experiments designed to test psychoanalytic propositions display a blatant lack of interest in the meaning, within the theory of psychoanalysis, of the propositions tested. Thus most of them certainly did not measure what they purported to; as for the rest, it is unclear whether or not they did. Even where the findings appear to confirm a relationship posited by psychoanalysis, the experiments usually tested only an analogous relationship on a high level of the hierarchy of psychological organization. It is not that all these experiments are useless as confirming evidence, but rather that at this stage of our knowledge it is not clear what—if anything—they confirm. It is hard to share Hilgard's [173] enthusiasm for most of the experiments he considers relevant and confirming. It is likely that some of the experimental findings will fall into place when ego psychology has clarified the hierarchic relationships which obtain in psychological organization. Command of the theory should help toward making the results of future experiments unequivocal, but it is not as much of a guarantee of success as ignorance of the theory is of failure. The experimental psychologist who enters the precincts of psychodynamics meets the same complexities which the clinical observer has been struggling with for over six decades. There are no "easy pickings" and the "experimental method" has no magic here.

In conclusion: the extensive experimental evidence for the system, which would seem to confirm it in terms of the usual criteria of psychological experiments, cannot be considered conclusive in terms of the psychoanalytic theory, since most of the experiments disregard the theory's definitions. The extensive clinical evidence, which would seem conclusive in terms of the system's internal consistency, fails to be conclusive in terms of the usual criteria of science, because there is no estab-

⁹¹ I am not listing here D. Levy's and J. McV. Hunt's experiments, nor others akin to them, because they are animal experiments, and represent conditions of a simplicity which does not obtain in man.—By the time of proofreading I had an opportunity to read the manuscript of the joint study by C. Fisher and I. Paul presented at the 1958 meetings of the American Psychological Association. It goes a long way toward meeting the difficulties discussed here.

lished *canon for the interpretation*⁹² of clinical observations. Thus, only a few observations and experiments (themselves in need of replication) offer evidence acceptable both in terms of the theory and in terms of psychology at large.

B. Major Sources of Incompatible Data

It is often assumed that the data and theories of the “dissident” schools of psychoanalysis [cf. Munroe, 240] are incompatible with and an embarrassment to psychoanalytic theory. This does not seem to be the case. Recent developments [for instance, Zetzel, 330, Erikson, 66] in psychoanalytic theory in general, and in ego psychology in particular, seem to show that this theory has the foundations for concepts and propositions which can account for the observations made and the valid relationships posited by the dissident schools. Thus we find no source of embarrassment here, but rather a task to be accomplished.

It is at times assumed—particularly by psychologists—that the findings and therapeutic results of Rogers’ client-centered therapy are a source of embarrassment for the psychoanalytic theory. But this is hardly the case. Rogers’ counseling procedure, at least to begin with, had no general psychological theory, nor even a theory of personality. The vague outlines of a theory of personality, which it has developed since then, seem to form a segment of an ego psychology. Thus the possibility of contradiction and embarrassment is limited to begin with, and is further minimized by two other aspects of nondirective counseling. *First*, the very idea of nondirectiveness is one of the implications of psychoanalytic therapy. The method of free association and the analyst’s “evenly hovering” [106] attention imply it. They both demand that the patient’s problems not be prejudged and that reliance be placed on his ability to meet his problems spontaneously. Rogers’ criticism of psychoanalysis is well founded in so far as it implies that with the accumulation of psychoanalytic knowledge, and with experience in wielding the tools of interpretation, often little room was left for the patient’s spontaneity, and too often the therapist came to be always right and the patient always wrong. Indeed, psychoanalytic ego psychology, too, may be considered to be a reaction to this danger, and the emphasis on the activity of the ego as a crux of therapy seems to have a central place in its therapeutic and general theory.⁹³ But the roots of this danger are in the practice rather than in the theory even of “classical” psychoanalysis. *Second*, nondirectiveness is but one aspect of the technique of psychoanalysis, and can be no more

⁹² The nonexistent *scientific canon of interpretation* is not to be mistaken for the well-established *clinical techniques of interpretation*.

⁹³ Cf. Sullivan, Horney; also P. Bergman [12], Gill [145], and Rapaport [266, 275, 280].

than one aspect of any other therapy. Experience has confronted non-directive counselors with the problems of transference and resistance familiar to open-eyed therapists of any persuasion. When the "nondirective approach" faces these problems, it will meet the eternal struggle of man's spontaneity, goodness, readiness and ability to help himself, against man's inertia, fear of his own spontaneity, need for help, etc. While it is true that treating man as a helpless, inert, and needful creature is prone to demobilize his spontaneity and ability to help himself, it is also true that man's helplessness, inertia, and need for support will not be eliminated by denying that they exist. Therapies or therapists who practice either sort of denial end up by establishing their own McCarran Act: sooner or later they announce that this or that kind of patient is not the right kind for their kind of therapy. Not rarely they go further and announce that this or that kind of patient is "not treatable." In the long run, psychological theories of therapy must come to a point where they will make it possible to select the therapy which is good for a patient and not the patient who is good for a therapy.⁹⁴ Yet Rogers' successes, limited though they may be, clearly show how little we know about the ego, its activity and passivity, its sources of energy, etc. Reider's [285] report of "spontaneous cures" likewise shows up our ignorance. In this sense, though not "embarrassing" or "inconsistent," Rogers seems to provide data which prod psychoanalysis toward further exploration of familiar as well as barely charted areas of ego psychology.

Many psychologists and even psychoanalysts (particularly, but not only, Neo-Freudians) have assumed that Dollard and Miller's [48] study and Mowrer's [239] experiments and their theoretical combination of psychoanalysis with learning theory have cut across the "theoretical jungle" of psychoanalysis, replacing much of it by learning theory. The powerful position occupied by learning theory until recently (and perhaps still) on the American scene "reinforced" this assumption. But the fate of psychoanalytic theory, or for that matter, the fate of any theory, cannot be settled by popular vote; if it could be, psychoanalysis would be in a bad way. Learning theory seems to be the (academic) theoretical backbone of the majority of recent, mass-produced clinical psychologists. But since this theory cannot guide their clinical work, they rely there increasingly upon psychoanalytic propositions, whose theory they have not studied. Thus the "marriage of convenience" that Dollard, Miller, and Mowrer recommend between psychoanalysis and learning theory must indeed seem to be convenient to them, since it seems to justify clinical practices, while at the same time it provides a salve to the academic conscience. Does the work of these authors provide data in-

⁹⁴ See Knight [202, 203] and Gill [145] concerning the bearing of psychoanalytic ego psychology on these issues.

consistent with and embarrassing to psychoanalytic theory? Since they present themselves primarily as protagonists of psychoanalysis and purport to provide it with a solid experimental and conceptual foundation this question is not easily answered. Still, we must ask: while these authors (excepting in some respects Mowrer) do not intend to embarrass psychoanalysis, have they nevertheless produced data incompatible with psychoanalytic findings and theories? Only a brief sketch of the theoretical situation can be attempted here [see Rapaport, 271, 272].

These investigators have produced, by the method of conditioning experimental analogues of "Freudian mechanisms" in animals [Masserman, 229]. These analogues would be neither embarrassing to nor incompatible with psychoanalytic theory if no claim were made that in man, too, the mechanisms of the primary process and of the defenses are products of conditioning. Dollard, Miller, Mowrer, and Masserman imply—to say the least—such a claim, and thereby elevate the conditioning theory of learning to the status of *the* learning theory of psychoanalysis. This *is* incompatible with psychoanalytic theory, since it makes the economic and genetic points of view superfluous and thus clashes with the observational data which made these points of view necessary parts of the theory (cf. pp. 110–114). Psychoanalytic theory at present cannot escape this embarrassment, since it has no learning theory of its own to pit against conditioning. This lack is not palliated by the demonstration that the conditioning theory of learning does not meet the empirical requirements (e.g., automatization problems, structure formation, distinction between primary and secondary processes) which a psychoanalytic learning theory will have to meet. Psychoanalysis will be totally free of embarrassment from this quarter only when it has a learning theory which not only fulfills its own empirical and theoretical requirements, but is also broad enough to account for conditioning phenomena—including the conditioned analogues of "unconscious mechanisms"—as special cases.

The work of these investigators has come up against the problem of persisting psychic formations, which has beset and embarrassed all motivational (need-gratification) theories. G. Allport's criticism of motivational theories and his ego psychology start from this problem, and psychoanalytic ego psychology faces it squarely. The *method* of conditioning used by Miller, Dollard, etc., determined the form in which they encountered this problem: conditioned responses are in general subject to extinction; thus, abiding psychological formations require explanation. Why are they exempt from this rule? or, How are they so reinforced as to avoid extinction? This is indeed one of the central difficulties of all conditioning theories of learning [cf. 82]. The theory of neuroses brings these questions into sharp relief, since symptoms are apparently non-

rewarding and should thus be subject to extinction.⁹⁵ Dollard and Miller as well as Mowrer tried to meet the problem by assuming that reinforcement through "learned" (conditioned) drives can account for non-extinction. This solution brings with it the same difficulties which raised the problem to begin with—namely, that drives, whether learned or not, cannot account for persisting structures. But this fact does not seem to have deterred Dollard and Miller, though they were aware that the problem is one of ego psychology.

Mowrer, however, was apparently not satisfied with postulating—and demonstrating by analogues—"acquired drives," but asserted that they are acquired by contiguity and not by reinforcement learning. This assertion and the observations it is based on—though questioned by learning theorists—are a source of embarrassment for psychoanalytic theory, and will continue to be as long as psychoanalytic theory accounts for derivative drives by differentiation of basic drives, and infers that this occurs parallel with structure development, but cannot specify either the process of structure formation or that of drive differentiation.

But what is more often considered a source of embarrassment for psychoanalytic theory in Mowrer's system—his formulation that neurosis is due to "underlearning" and not to "overlearning"—is actually no source of inconsistent data. Mowrer apparently saw that a conditioning theory (whether monistic or dualistic) can hardly explain the persistence of "learned drives" and nonrewarded symptoms by "overlearning." Therefore, he reasoned, if the drives and the neurotic drive manifestations cannot be proved to be "overlearned," then that which is supposed to control them must be "underlearned." Thus he equated the repressing forces (censorship, superego) which—according to him—are weak in neuroses, with underlearned social prohibitions. This sounds logical, but it is not psychological, and is doubly incompatible with psychoanalytic theory and observations. *First*, it implies what is to be proved, namely that the intrapsychic structures and forces in question are *learned* (conditioned). *Second*, it implies that these structures and forces are ineffective because of their weakness or absence ("underlearning"), though the concept of the unconscious in general, and the observations concerning the unconscious sense of guilt in particular provide a different explanation so far not contested by any *evidence*. Having replaced the "overlearning theory" which, *according to him*, is the core of the psychoanalytic "drive-repression theory" of neurosis, he assumes that he has demolished the latter. His "underlearning theory" of neurosis, translated into clinical language, says that the trouble with neurotics is not that their censorship (repressive forces, conscience) is too strong, but rather that it is too weak, having been repressed by the id and ego combination. However,

⁹⁵N. Maier [228] took this issue so to heart that he propounded a dualistic theory of motivation learning vs. frustration learning.

he does not specify the process and mechanism of this "repression." For Mowrer the neurotic is a person who "did not learn the lesson." He takes us back to moralistic and religious precepts and to the pre-Freudian conception of neurosis. What seems to have happened is that Mowrer rediscovered the unconscious sense of guilt, long since recognized in psychoanalysis. Not realizing the place of his "discovery" in psychoanalytic theory, he explained it in terms of learning theory and put it in the center of psychodynamics, unconcerned with the consequences of this recentering for psychodynamics at large.

Piaget's observations and theories, if confirmed, may—though they need not—prove to be sources of incompatible data. They seem to demonstrate that structure (schema) formation arises from disturbances in the equilibrium of existing structures (schemata), and that such disturbances always act as motivations (desirability). In Piaget's terms, function always brings about structural change (disequilibrium) which, in turn, provides motivation (desirability) for a repetition of the function (circular reaction) which consolidates the structural change, i.e., builds new structure (schema). Now it may prove possible to treat the observations on which this theory is based in terms of what psychoanalysis calls processes of autonomous ego development. If so, Piaget's theory would shed new light on the nature of many ego motivations and would corroborate Hartmann's assumption that the ego has sources of energy other than bound and neutralized drive cathexes. It would also force us to rethink the theory of id-ego relationships. But if Piaget's theory and the observations it is based on should lead to the conclusion that the *only* source of motivations is the one discovered by him, they would become incompatible with psychoanalytic theory. In either case nothing more desirable could happen to psychoanalytic theory than a corroboration of Piaget's findings. Psychoanalysis would find itself for the first time confronted with a genetic theory of broad scope, using a method of observation which is in some ways akin to (if not derived from) its own. The mutual stimulation of this confrontation could not but prove productive.⁹⁶

Psychologists, particularly experimental psychologists, seem to assume that experimental tests of psychoanalytic theories, if negative in outcome, provide data inconsistent with and embarrassing to the theory. Sears [308] and the many who have quoted him and relied on him seem to have assumed something like this. Would that it were so. It is not. Most of the studies Sears surveyed took a psychoanalytic statement out of its

⁹⁶Harlow's, Christie's, and others' observations concerning "activity drives" may also pose a problem akin to that posed by Piaget's studies. But these observations are too new to be assessed.

context and tested the statement, rather than the theory, which they usually knew little about. Moreover, they used methods of testing alien to the observations from which the statement and its terms derived. It is doubtful that any of the currently available experimental results can be proved clearly incompatible with the theory. Here the very difficulty of obtaining data inconsistent with and embarrassing to the system becomes an embarrassment to it. Psychoanalytic theory, which is adequate for clinical purposes, will have to become much more systematic before experiments can be designed which will not simply confirm or refute its propositions, but rather specify and modify them. Thus the experimental psychologist who approaches it must assume the responsibility of clarifying and specifying theoretically the propositions which he undertakes to test. For the time being this is the only way to arrive at experimental findings relevant to and incompatible with the theory.

C. "Critical" Tests of Principal Assumptions

Those difficulties in testing psychoanalytic propositions which we have discussed naturally apply also to the so-called critical tests. In addition, the latter usually require the existence of alternative theories or alternative possibilities within the theory. There are few, if any, specific psychoanalytic propositions for which other theories have an alternative to offer, and since the psychoanalytic theory itself is not geared to experimental tests, it does not usually envisage alternatives in the sense implied by the conception of crucial tests, but rather in that implied by alternative interpretations. While the alternatives in the former sense call for a decision between two possibilities, one of which is incompatible with the theory, the alternatives in the latter sense are both consistent with the theory, but only one is realized in the phenomenon studied, whereas the other is not. The former pertains to systematic possibilities, the latter either to a single instance, or to a specific genetic sequence, or to an individual person. Thus critical tests are hardly possible for the propositions of the special (clinical) theory of psychoanalysis.⁹⁷ The opportunities—if any—for such tests must, then, be sought in the general (psychological) theory of psychoanalysis. But the primitive state of the systematization of this general theory militates against the possibility of critical tests, and so does the nonexistence of other theories of comparable scope.

For these reasons it is difficult to envisage "critical" tests of this

⁹⁷ Clinical predictions are always fraught with the fact that all motivations have multiple, equivalent, alternative means and goals. Thus, such predictions usually cannot specify which of these equivalent alternatives are to be expected, and therefore, the results of experimental tests of predictions must first be interpreted before their bearing on the theory can be established.

theory. Yet it is probable that the sources of actually or potentially incompatible data—i.e., learning theories and Piaget's work—must serve as points of departure for critical tests.

Critical studies centering around Piaget's theory must first corroborate his observations and extend them to behavior which involves affects and motivations, in the psychoanalytic sense of these terms. The aim of studies revolving around learning theory would be to demonstrate processes of structure building and learning compatible with psychoanalytic theory, but incompatible with existing learning theories, or vice versa. Any quantitative method *may* lead to a critical test if it can trace qualitatively the process of structure consolidation, that is to say, if it can show that, once a certain set of qualitative changes has occurred in a process of acquisition, a nonextinguishing structure arises. It *will* turn into a critical test when it can also show that existing learning theories are incompatible with the process of structure formation traced by it. None of the well-known methods can at present be regarded as the royal road to such a critical test. But the following may serve as examples of techniques which might be tried: tracing quantitatively the qualitative changes in the acquisition of skills which are not simply compounds of other skills; tracing the qualitative changes in the course of learning meaningful verbal material; tracing how subjects *spontaneously* discover a meaning or a pattern embedded in material which they handle in the *deliberate* pursuit of a different goal. Altogether, any quantitative technique which makes it possible to follow the qualitative (and not just the quantitative) course of the development of any behavior, which is on its way to becoming a part of the person's quasi-abiding behavior equipment, might conceivably become the method of choice for a critical test.

This lengthy discussion of "critical" tests is warranted neither by the actual state of psychoanalytic theory nor by my knowledge of these matters. Its purpose is to stress that crucial tests—if they are to come—will not necessarily center on motivations. Indeed, my intention here has been to make it plausible that the crucial experimental contribution toward the consolidation of psychoanalytic theory may well be made at an apparent distance from what is commonly considered its home ground. It may well come on the battlefield of learning theory, or on that of perception.

X. METHODS, CONCEPTS, AND PRINCIPLES OF BROAD APPLICATION

A. The Range of Application

Unlike most psychological theories, whose application outside their initial ground is a matter of future possibility or probability, the applica-

tion of psychoanalysis to nearly all human endeavors and products has been envisaged and actualized from the very beginning.

The questions here are not what applications are possible, but rather, how valid and effective are they? How can they be made more appropriate and effective? Since these applications have rarely been systematic, the need for and the possibility of studies aimed at systematizing the existing applications are practically unlimited. Such studies might well increase the effectiveness of the applications by bringing them in line with the present state of development and systematization of psychoanalytic theory. The development of ego psychology provided psychoanalysis with new tools which bid fair to increase the appropriateness of its application in all fields. Particularly the application of psychoanalysis to sociology and anthropology has gained and stands to gain further from Hartmann's [159] and Erikson's [60, 61, 66] contributions, likewise, its applications to art from the contributions of Kris [207]. The change that ego psychology has wrought in the relationship between psychoanalysis and psychology has been discussed above.

B. Methods, Concepts, and Principles of Long-term Significance

In his outline, Dr. Koch defines "long-term significance" as the ability to survive independently from "the over-all structure or detailed assumptional content of the system." Freud repeatedly stated that any therapy which takes into account the unconscious, transference, and resistances is psychoanalysis. Thus the concepts which he considered to be of broadest significance are the dynamic unconscious, transference and resistance.

But perhaps we can go beyond Freud's view if we consider *first* that the methods, principles, and concepts of greatest independence in any system are on the one hand those closest to observations and, on the other, those of greatest generality; *second*, that some of the methods, principles, and concepts of all major theories sooner or later become so general that they enter the public domain and can no longer be considered specific to the theory. Psychoanalysis has developed methods, concepts, and principles which are now in the public domain: for example, the method of interview;⁹⁸ the concepts of the "descriptive unconscious," motivation, and defense; and the principle of psychological determinism. It could be justly argued that all of these antedate psychoanalysis. But psychoanalysis changed their character and gained for them an acceptance in the public domain, where they are now independent of the theory and not subject to its changes.

⁹⁸ The method of interview implies only that the past is relevant to the understanding of the present, but no other specifically psychoanalytic assumption concept [cf. 166].

1. Methods. What are the methods of psychoanalysis? It may be useful to distinguish here between methods and techniques. Let us define techniques as the specific ways and means by which methods are applied and note that in psychoanalysis they have the additional connotation of ways and means which are not only exploratory, but also effective therapeutically. The techniques of psychoanalysis have been studied [106, 72, 151] but its methods have scarcely been given systematic thought [see, however, Bernfeld, 14]. Thus, what follows can be no more than a preliminary sketch.

It would seem that the basic method of psychoanalysis⁹⁹ is the method of interpersonal relation [314, 260]; more specifically, it is the participant observation variant of the method of interpersonal relation; in particular, it applies the nondirective (free association), the interpretive-genetic, and the defense-analysis techniques of participant observation [see Gross, 153; also Rapaport, 263, 273]. These methods and techniques, unlike the interview method, are linked to the theory of psychoanalysis in that the phenomena they are based on are the observational referents of the transference concept. Human beings in dealing with each other repeat the patterns they have developed in their relations to "significant others," and these patterns of relationships *ultimately* go back to those which the individual has developed toward the earliest "significant others": father, mother, siblings, nurses, etc. Such repetitions of relationship patterns are the empirical referents of the transference concept. Transferences are ubiquitous in everyday life, but so far the psychoanalytic methods are the only ones for observing them systematically and for tracing their genetic roots. The aim of the psychoanalytic method of interpersonal relation is to bring about such transferences. The aim of the method of participant observation is to make these transferred patterns conscious. The free association, interpretive-genetic, and defense-analysis techniques are specific interventions facilitating insight into these transferences.

In so far as these methods and techniques are tied to the concept of transference they are specifically psychoanalytic.¹⁰⁰ But they are so closely related to a broad and crucial range of observations that it is hard to conceive of changes in the structure of psychoanalytic theory which would alter them or dispense with them. What has changed repeatedly, and is likely to change again, is the relative emphasis in the theory and in practice on any one of these methods and on the patient's gaining insight into his transference patterns. Interview and therapy methods which

⁹⁹ Here we are concerned only with the methods specific to psychoanalysis and disregard others like suggestion, support, etc. [cf. Bibring, 28].

¹⁰⁰ They vary in this respect; of the three, the free-association technique seems to be the one least closely tied to the concept of transference.

do not aim at gaining information about and insight into transference patterns may well achieve their limited or different goals, but none so far has succeeded in replacing the psychoanalytic methods of systematically observing transference patterns. Projective techniques do obtain some such data, but the recent emphasis on the significance of the interpersonal relation between patient and tester [see Schafer, 298] points to their limitations. Whatever the fate of those more specific methods described as "techniques," and whatever the ultimate judgment on the therapeutic effectiveness of these basic psychoanalytic methods, the latter are likely to stay with us as unique methods of observation for a very long while.

2. Principles. The "points of view" seem to be the equivalents of "principles" in psychoanalytic theory. Yet their form shows that the time to examine them one by one, for their long-range significance, has not yet arrived. Instead of formal principles we will present here a few general conceptions, which compound the various points of view, and which seem likely to survive whatever the fate of the more specific ingredients of the psychoanalytic theory should prove to be.

a. Human behavior is neither merely learned (imprinted by repeated experience), nor preformed and merely unfolded in the course of a "maturation" process.

b. Human behavior develops according to the "ground plan" (Erikson) of an epigenetic process (of which libido development and ego development are specific aspects) through a sequence of developmental crises, whose solution depends as much on the solutions of previous crises¹⁰¹ as on the environmental (social) provisions which meet it (Freud, Hartmann, Erikson, Kardiner, Sullivan).

c. The laws of epigenesis, whose expression in the full perspective of the individual life cycle is the epigenetic "ground plan," find their shorter-range expressions in the regulation of all behavior and experience by intrapsychic motivations and structures. The crucial regulations are unconscious.

d. The regulation of behavior and experience by motivations and structures implies: (1) basic tensions (motivations) within the organism, which strive toward reduction and organize experience and behavior to that end; (2) basic structures, given by evolution, which on the one hand serve as guarantees of the organism's adaptedness and adaptation to the environment (Hartmann, Erikson), and on the other serve as the means of maintaining, increasing, and discharging the tension which exists in the organism; they organize experience and behavior to these ends; (3) differentiation both of the tensions (motivations) and of the

¹⁰¹ Not success or failure but the *kind* of solution reached is crucial here (Hartmann, Erikson).

structures, in such a manner that the matrix of the differentiation survives side by side with its products, though its manifestations are always amplified by these differentiation products; (4) this differentiation is determined both by the epigenetic laws and by the environmental (social) provisions designed to meet the epigenetic crisis in which the differentiation in question comes about; the differentiation products become further guarantees of the organism's adaptedness and adaptation to the environment.

It is not implied that no other psychoanalytic propositions have this degree of generality, nor that other psychoanalytic propositions of equal or lesser generality may not also have a long-range survival potential. These four propositions summarize that cohesive core of the most general conceptions of psychoanalysis (stripped of their specific content) which has remained constant throughout the changes of the theory and bids fair to continue to do so. It could be argued that these points are shared with other psychologies and are not specific to psychoanalysis. This argument does not hold, though it is a clear indication that psychoanalytic conceptions have been gradually assimilated by psychology at large. No other psychology contains this assembly of general conceptions, methods, concepts, and theories; nor has any other psychology supported any of these by as broad an array of observations as has psychoanalysis.

3. Concepts. The major concepts of a high survival potential pertaining to each of the metapsychological points of view are:

a. Dynamic point of view. The concepts of unconscious forces and conflicts are close to observations and yet of sufficient generality to have a high survival potential. The concepts of drive, drive-fusion, specific drives (sex, aggression, life and death instincts, etc.) are of a lesser generality, and may well change or be replaced as the theory changes.

b. Economic point of view. The concepts of primary process, secondary process, and pleasure principle (wish-fulfillment) are so directly related to observation and so general that they are likely to survive. The concepts of cathexis, binding, and neutralization, however, are both more inferential and more specific, and while the observations do seem to demand some set of quasi-quantitative concepts like these, it is uncertain whether they will survive in their present form.

c. Structural point of view. The concepts of structure and relative autonomy (Hartmann) are indispensable to the theory, and at present it is not possible to foresee changes in the theory which could eliminate them. But the concepts of id, ego, superego, and the differentiation of the ego into defense-, control-, and means-structures are neither as indispensable to nor as independent from the theory. However, a variety of subordinate structural concepts (e.g., specific primary-process and defense mechanisms, like displacement, condensation, substitution, sym-

bolization, repression, isolation, reaction formation, projection)¹⁰² which are more directly related to observations and of a lesser generality, are likely to survive. It is not implied, however, that this holds for all the specific defense mechanisms.

d. Genetic point of view. We discussed above the high survival potential of the epigenetic principle. This holds also for the conception of the crucial role of early experiences, as well as for the concepts of fixation and regression. The specific concepts related to libido development, such as orality and anality, are also likely to survive [cf. Kardiner, 194, 195; Sullivan, 313; Erikson, 56, 61, 62], since they are closely related to observations. However, the classic conception of libido development itself may well undergo radical change, as it becomes one aspect of the integral process of epigenesis. The conception of the *special role* of psychosexuality, even though it has good empirical anchorage, does not seem to have that degree of generality which would make it a theoretical necessity (cf. pp. 89–90, above).

e. Adaptive point of view. The conceptions of the organism's preparedness for an average expectable environment (Hartmann), apparatuses of primary and secondary autonomy (Hartmann), mutuality (Erikson), relative autonomy from the environment (Gill-Rapaport), i.e., the dependence of the secondary process on external stimulation [98, p. 515], modes and modalities (Erikson), though too new to be properly evaluated, do seem likely to survive.

Now, in brief, about the concepts of the special (clinical) theory. Let us take the transference concept as an example. We have encountered it as the foundation of the long-range significance of psychoanalytic methods. Yet its own survival potential might be characterized as borrowed. The referent of the transference concept is not a single process but a congeries of processes. The patterns which are transferred may be broad or fragmentary, and the processes by which transference is accomplished are many and varied: wish-fulfillments, displacements, projections, etc. The transference concept refers to an end result: it is an achievement concept. In the clinical theory of psychoanalysis it is indispensable, but the general theory of psychoanalysis resolves it into process concepts. It is probably not far off the mark to suggest that this is the case for most clinical concepts. A case in point is the very definition of resistance as the manifestation of defense. It is not implied, however, that clinical concepts do not have a survival potential: they do, but only when they are close to observations and when the process-concepts which underlie them are themselves likely to survive. A study of the concepts of the clinical theory from this point of view would be rewarding, but so far we

¹⁰² Objections might be raised against discussing these primary-process mechanisms as structures, but I cannot attempt to justify this here.

do not have even a workable separation of the special clinical and the general psychological concepts of psychoanalysis.

XI. THE THEORY'S ACHIEVEMENTS AND ITS CONVERGENCE WITH OTHER THEORIES

A. Achievements

Freud's earliest program [94, appendix] was to develop a general psychology on neuropsychological lines. This attempt failed and Freud concluded [94, 64] that the theory of behavior must be a psychological theory. But he never gave up the belief that once psychoanalysis had developed far enough, its link to physico-chemico-biological processes would be found. Apparently the time for this has not yet arrived and the recurrent popularity of neurological models has so far not brought it any closer. It seems that until psychology has progressed much further, attempts at neurological or biological explanations of behavior are bound to be of little avail. Freud's program to develop a general psychology receded into the background for a while, but was revived with the development of ego psychology.

To solve the problem of conversion (somatic compliance) was also part of the earliest program. The nature of the hysterical conversion symptom—the psychological conflict's "leap into the somatic"—was and has remained a haunting riddle, though Freud began early to question that psychoanalytic methods and theory could solve it. Psychoanalysts, instead of solving the problem, generalized it, first into the conception of "organ neurosis" [F. Deutsch, 45; Meng, 230], and then into "psychosomatic medicine" [Alexander and French, 4, 5, 86, 90; Dunbar, 49, 50; Weiss and English, 321]. The number of investigators and investigations in this field is great, broad areas of observation have been scouted and mapped, and the effect on medicine proper is considerable, but it is not clear just how much—if any—theoretical advance has been made. Psychosomatic studies remain fraught with the problem of "specificity," which so far has defied solution. It is worth noting, however, that relatively recently a possible clue to the conversion riddle has appeared [see Travell, 317].

The programs so far discussed belong to that phase of Freud's work which was preparatory to psychoanalysis. The main program Freud set for psychoanalysis proper (1900) was to explore the unconscious; later (1923) this changed into the exploration of the id and the unconscious ego. Discoveries are still being made in both areas and much of the "unconscious ego" is still uncharted territory. Yet considering that successful exploration always breeds new problems, the work on this program

can be considered well advanced. This, however, is a judgment within the frame of reference of psychoanalysis: it refers to the program of exploration and not to a program of *testing* and *developing* the theory by means of experiments.

A related program was to apply the theory to myth, legend, literature, art, ethnology, etc., in order to demonstrate its pertinence to all human behavior and products and thus to obtain a broad base of supporting evidence. The achievements in this direction have already been mentioned, and the importance of the new means provided by ego psychology for the further pursuit of this program has been indicated.

For a long while the exploration of the ego seemed to be only a contemplated program. Freud expected the information about ego functions to come from the study of "narcissistic neuroses" (i.e., psychoses), but delayed this study because he considered the exploration of the unconscious to be the primary task. Yet this program was indirectly pursued throughout the history of psychoanalysis in the study of the defenses, censorship, secondary process, and reality relationships. However, Freud did embark (1921, 1923) on an explicit conceptualization of the ego without studying psychoses anew, apparently prompted by the problem of "the negative therapeutic reaction" and "the unconscious sense of guilt" [124, 126]. Later he carried the study of the ego further (1926) by re-evaluating the problem of anxiety [131]. Other psychoanalysts followed his lead [77, 245, 284, 320] and the achievements of this phase of the program were capped (1936) by A. Freud's [93] work. The ego-psychological program was then dramatically broadened by Hartmann [157] and by Erikson [56, 59]. The ego was explored slowly but so successfully that a broad and still uncharted area was opened up.

The program of superego exploration was already implicit in the study of censorship (1900). But only the study of narcissism [114] brought it into focus in the term "ego ideal" (1914). Though the concept of the superego was formalized simultaneously with the ego [124, 126], and in spite of significant advances [see 73], the work on this program has hardly passed the beginning stages.

While psychoanalysis as a therapy is primarily the subject matter of the special (clinical) theory, the theory of therapeutic technique is part of the general theoretical program of psychoanalysis. It was so treated by Freud in the prehistory of psychoanalysis [35, chap. 3], in some of the "Papers on Technique" [106] and in "Analysis Terminable and Interminable" [139]. Nevertheless, this program is still far from fulfillment. Even the most systematic [72] of the few extensive [151, 224] treatments of technique contributes little toward the theoretical program. E. Bibring [28] has penetrated into these problems further than most others. Recently Eissler [51] and Gill [145] have also made rele-

vant contributions. The importance of this program and the difficulties in its way are equally great. Progress may require not only the study of the techniques of psychoanalysis and those of other schools of therapy, but also the development of a psychoanalytic theory of communication.

The theoretical explanation of neuroses was an outstanding part of the program. This is where the work on the special (clinical) theory of psychoanalysis had its greatest achievements and also brought a considerable general theoretical yield, to which Fenichel's [73, 77] systematic survey of the special theory refers continuously. Yet we still do not have a systematic treatment of neuroses, from the point of view of the general theory, comparable to that which Freud gave of dreams in chapter 7 of *The Interpretation of Dreams* [98].

The theoretical explanation of psychoses was also a part of the program. Beginning with his early (1895) study of a case of paranoia [94] and his analysis of the Schreber case [107], Freud dwelt on it repeatedly [114, 117, 119, 126, 127, 128]. Yet despite the contributions of Abraham [1], Federn [70], B. Lewin [214], Fromm-Reichmann [142, 143], M. Wexler [324, 325], Hartmann [163], and others, and in spite of the studies by Putnam, Mahler, Bettelheim, and other psychoanalysts on juvenile schizophrenia, the fulfillment of this program has barely begun.

The situation is only slightly better in that part of the program which comprises the general theory of character disorders, addictions, delinquency, criminality, and borderline problems [22, 23, 202, 203, 282, 283].

B. Convergence with Other Theories

It is difficult to differentiate the applications of the theory to other fields, its influence on other sciences, and its convergence with other theories and sciences. The distinction might be drawn, perhaps, as follows: *application* is the work of psychoanalysts in other fields; *influence* is the adoption of psychoanalytic assumptions, methods, findings, and/or theories by workers in other fields; *convergence* is mutual influence.

In this sense, in anthropology the days of application [Freud, 109; Roheim, 289, 291, etc.] and influence (e.g., Kluckhohn) are past, and convergence can be observed on one side in Erikson's work, and on the other in the work of cultural anthropologists (however the opinions may vary about this work otherwise). [See also *Psychoanalysis and the Social Sciences*, 290.]

The same holds for sociology, where the days of application and influence (Freud, W. Reich, Fenichel, Lasswell, and the early Fromm) are past and convergence can be observed, for instance, in Parsons', Riesman's, and N. Foote's work on the one hand (however the opinions

may vary about this work otherwise), and in Hartmann's and Erikson's on the other.

The convergence of psychoanalysis with medicine in general and psychiatry in particular, though only too obvious, is practical rather than theoretical.

It is questionable whether one can speak of a convergence of psychoanalysis with the other fields, its applications to which were mentioned earlier: in art, literature, history, etc., we find influence, but no more.

Now, to the convergence of psychoanalysis with psychology. We have already mentioned that psychoanalytic ego psychology seems to be responsible for a considerable part of this. The convergence with developmental psychology is of long standing (Werner and the early Piaget) and is reinforced on the one hand by the recent longitudinal and cross-sectional studies of psychoanalysts like M. Fries, Spitz, Escalona and Leitch, Benjamin, Kris, Mittelmann; and on the other by the studies of Piaget, Werner and his associates, and others. The work of K. Lewin and his associates on the one hand, and that of T. French on the other are outstanding indications of convergence. The studies in learning theory by Dollard, Miller, Mowrer, Sears, etc. (however the opinions may vary about this work otherwise), represent a convergence of psychology with psychoanalysis. Murray's early work, his and his associates' and their successors' work in the assessment of personality are also indications of this convergence; so is much of the recent work in experimental clinical psychology. Two other important indications of convergence, the studies on motivations and memory and on motivations and perception, have already been discussed.

The future of this convergence may hinge on the solution of the problem of structure formation and learning. If that solution should arise from the matrix of psychoanalytic theory, the latter may become the core of psychology proper. If the solution should prove relatively independent of psychoanalysis, then the latter is likely to become a relatively subordinate part of the general theory of psychology as the core of its clinical and motivational theories, but its concepts and theories will be reducible to more fundamental ones. The existing learning theories have not accomplished this reduction and it seems unlikely that they ever will.

Finally, coming closest to the home base of psychoanalysis, the development of psychoanalytic ego psychology has begun to extract the valid contributions from the theories of the Neo-Freudian schools, and thus to initiate the convergence of these offshoots with psychoanalytic theory proper. There is still much to be done here and the convergence pertains only to the theories, not to the "schools" as organizations of vested interest.

XII. TASKS FOR THE FUTURE DEVELOPMENT OF THE THEORY

A. Empirical Evidence Needed

Psychoanalytic theory does not need additional data *per se* for its development: the amount of data is already embarrassingly large. It does not need the Blacky Test type of data which, though amenable to statistical treatment, are simply masked clinical data: the clinical data are better. It does not need experimental data which replicate clinical relationships. What it needs are *methods to obtain data which can lead beyond the clinical relationships to theoretical relationships of the type discussed in this essay.*

It is for this reason that experimental data on structure formation and learning, and data corroborating or negating Piaget's observations and theories are needed.

In Hartmann's theory, we have for the first time a concept of autonomy and we have (cf. pp. 95ff. and p. 98, above) also an elaboration of it into a threefold conception of relative autonomy (Erikson, Gill). The methods used in the McGill University and Bethesda studies on sensory deprivation, as well as the hypnotic methods, seem to be appropriate means to alter the balance of these relative autonomies [see 280]. The theory needs data obtained by these or other relevant methods (e.g., drugs like mescaline), but just *any* data obtainable by these methods will not do: the need is for data obtained in controlled experiments *guided by the theory of autonomy.*

In Hartmann's studies we have for the first time a concept of conflict-free ego functions. G. S. Klein's work contains a variety of methods for the study of those ego structures which, unlike defenses, are conflict free and serve to control and channel motivations. Data concerning such structures are needed. But an indiscriminate proliferation of such data would provide mainly a catalogue of "cognitive attitudes" (Klein), just as French psychiatry has provided us with a term for more or less every possible form of phobia. The data needed are those which will elucidate the relation of these style structures to *other* ego structures (e.g., defenses), to motivations, and to each other.

In Erikson's work we have for the first time a theory and an epigenetic ground plan of ego development. Additional data concerning each epigenetic phase are needed. But again, just any data pertaining to a given phase of development will not do. The data should pertain to Erikson's observations and should corroborate, elaborate, modify, or negate them. To obtain such data, the investigators will have to adopt Erikson's frame of reference, at least to begin with.

We suggested above that a prerequisite of the theory of therapeutic

technique may be a theory of communication. Data for building such a theory are needed. The data and concepts of the existing attempts at a communication theory do not seem to be relevant. The focus of such a communication theory must be the laws which govern the tendency of communication to engender or to prevent reciprocal communication. Moreover, it should be a theory in which the communicants' becoming conscious of something is equivalent to (latent) verbal or nonverbal communication [see 263, 273]. The methods by which data relevant to such a theory can be obtained have yet to be worked out.

In Hartmann's "self" [161, 162] and in Erikson's "identity" [66] we have in psychoanalytic theory for the first time concepts to account for the historical continuity of the individual and for his self-experience, and conceptual tools to distinguish them from the referents of the ego concept. Data pertaining to and permitting the elaboration of these concepts are needed. But just any data of "self-experience," "self-evaluation," or "ego-involvement" will not do. They must be data concerning the relation of the "self" or of "identity" to the psychoanalytic theory of psychological functions in general and of ego functions in particular.

The less than satisfactory progress in the theoretical understanding of schizophrenia and other psychoses has been mentioned. Here again data are in abundance. What to do with them is the question. They have not been selected to reveal the relation of the phenomena of schizophrenia to the existing theory. There is no need for more data showing that the content of psychotic products can be interpreted like dreams or unconscious fantasies. Nor are data needed on oral or anal wishes underlying the manifest content of psychotic products; these are ubiquitous in man, and only their role, intensity, and frequency might conceivably be specific to a given psychosis. It is the formal characteristics of psychotic behavior (action, affect, and thought) which seem to be specific, and what is needed are data to connect them with the psychoanalytic theory.

Data are needed to reveal the similarities and differences between analogous structures (and motivations) on different hierarchic levels of the psychological organization. J. F. Brown [36] obtained some data of this kind [cf. also 276].

Last but not least, though no data replicating clinical relationships are needed, any replication whose purpose is to quantify these relationships so as to pave the way toward "dimensional quantification" should be welcome.

This enumeration has no systematic pretensions, nor does its sequence imply an order of importance. The examples were chosen to show that, more than data, we need methods which promise to yield data relevant to the theory and its unsolved problems.

B. Obstacles to the Development of the Theory

Here we will dwell on theoretical obstacles, leaving the discussion of practical obstacles for Section C.

The days of the resigned belief that complex psychological phenomena cannot be studied in the laboratory are past. So is the overenthusiasm of K. Lewin's early days [220], when there seemed to be no doubt that all psychological phenomena could be relevantly studied on the laboratory scale. While we all hope that every psychological phenomenon is amenable to scientific study, to find the ways and means for this has become our gravest concern. The main obstacles to the development of psychoanalytic theory center around these ways and means.

First, due regard for the individual's rights sets limits to the manipulation of behavior outside and even inside the laboratory; and due regard for the privacy of the individual sets limits even to observation. This is one of the major empirical obstacles. The problem is not only the ethical one of trespassing on rights and privacy but also, and perhaps primarily, what such trespassing does to the subject, to the observer, and to the observation.

Second, the hierarchic problem, so heavily stressed in these pages, implies that reduction to laboratory size more often than not changes the hierarchic position of the phenomenon or relationship in question, so that not the phenomenon or relationship itself, but a high-level hierarchic equivalent of it is studied. This is not simply an obstacle. It indicates that laboratory research can attack all psychological problems, provided it centers its attention on the laws of hierarchic transformations. Once such laws begin to take shape, psychologists will be able to dispense with the arbitrary claim that the laboratory findings obtain for life situations and will use these laws as the rules by which inferences from the laboratory findings to life situations can be drawn. This theoretical complexity then is not *per se* an obstacle, though there is long and arduous experimentation ahead before these laws of hierarchic relations are discovered and brought to a point where they can serve as rules of inference.

Third, laboratory methods cannot get around the troublesome fact that there are many psychological phenomena which occur, as a rule, only in the contact of one person with another (or others). The study of such phenomena led to the method of participant observation in therapy, in everyday life, and in laboratory situations. This method has scarcely been explored theoretically; in it the investigator enters into the privacy of the subject, but he does so at the price of becoming a participant, shouldering all those implicit and explicit commitments which participation involves. Psychoanalysts and other therapists know a great deal

about these commitments and their effect on the observer and on the observed. But the implications of this knowledge for the method have not yet been theoretically formulated [see, however, Bernfeld, 14, Gross, 153], and the lack of such systematization is an obstacle in the way of the theory's development.

We have discussed the possibility that experimental study will discover rules of inference, by means of which conclusions can be drawn from laboratory-sized to life-sized phenomena. What about the rules of inference for relating data obtained by direct observation to data obtained from participant observation? For instance, the psychoanalytic theory of development is built from reconstructions based on data obtained by the method of interpersonal participant observation in the therapeutic two-group situation, while Piaget's theory of development is built on data obtained in direct observation.¹⁰³ Now it is possible that the theories of development of psychoanalysis and of Piaget will prove compatible, and rules of inference will be found to link their concepts. Indeed, it is possible that their mutual influence will lead to a redefinition of their concepts so that there will arise a single conceptual system which subsumes both theories or subsumes one under the other. But there are two other possibilities. First, the two theories might prove incompatible and thus one of them untenable. Second, it might just happen that the two theories, like the observations they are based on, will prove not to overlap, and not to be incompatible. If so, the two methods will have arrived at theories pertaining to two different aspects of the same subject matter. We might, then, have to conclude that these two aspects of the subject matter are complementary [cf. Niels Bohr's complementarity concept in atomic physics, and the complementarity in the study of the living cell envisaged by the biophysicist Delbrueck, 44]. The uncertainty whether the yield of the participant observation method and the yield of other methods can be related to each other by conjunctive rules of inference, or must be related by a disjunctive rule of complementarity, is a major hurdle in the way of the development of psycho-

¹⁰³ All observations on human beings are in a sense participant observations: one-way screens, movies, and sound tracks obscure but do not circumvent this fact. Yet there is a difference between *being* a participant observer and *using* the method of interpersonal participant observation, and there is also a difference between *constructing* and *reconstructing* developmental relationships from participant observations. Piaget [254, 255, 256], too, *was* a participant observer: by his actions, he modified the situations and the tasks his children faced. Yet he did not *use* the method of participant observation, in that he did not systematically study the changes in the children's relation to him (their father) consequent to his "participation," nor the changes in their sensorimotor behavior as the latter depended on the children's relation to him.

analytic theory. It is possible that an inkling of this difficulty accounted for Freud's lack of interest in the attempts to verify psychoanalytic propositions by methods other than psychoanalytic.

Fourth, the last of the obstacles to be mentioned here is the problem of mathematization, including quantification, already discussed. It is both an empirical and a theoretical obstacle to the development of the theory.

C. The Practical Obstacles to Theoretical Advance in Psychology

Let us first take the obstacles in the way of psychoanalytic theory. Here the lack of systematic theoretical literature, the nature of psychoanalytic training, and the character of psychoanalytic practice stand out.

As a rule, the observer and experimenter is guided in his contribution to theory development by systematic theoretical literature. In the lack of such, the investigator has to master the primary sources and do the systematization for himself. This is a time-consuming pursuit to which the habits of psychoanalytic practice are not conducive. It is often said that psychoanalytic theory is a rigid and unchangeable doctrine. Although there is such dogmatism and orthodoxy in the Societies and Institutes (whether they are Freudian or Neo-Freudian or in between) in regard to the clinical theory, I have rarely found dogmatism in regard to the general theory. The attitudes range from enthusiasm, through lack of interest, to total lack of information. The general theory, far from being well-ingrained dogma, is a waif unknown to many, noticed by some, and closely familiar to few. Not the alleged rigidity of the theory, but rather unfamiliarity with it is the obstacle to theoretical progress. The lack of systematic theoretical literature is certainly not the sole cause of this situation (the original sources *are* available) but it is a major handicap to advancement.

The training given by psychoanalytic Institutes is primarily designed for future practitioners, and limited to physicians. The scope of this training is defined by several factors: (*a*) its "night school" character, (*b*) the *average* medical training, which prepares the students neither for psychology, psychiatry, and psychoanalysis, nor for theoretical and research pursuits, (*c*) the fact that both teachers and students are, as a rule, full-time practitioners. Two additional facts about this training: *first*, it is postdoctoral, time-consuming, and costly, and thus pushes the graduate to seek more lucrative and less leisurely pursuits than research; *second*, though the rules limit it to physicians, some psychologists and other scientists can obtain "research training" in psychoanalysis, but this includes only training analysis and course work, not supervision (control)

and often not even clinical seminars,¹⁰⁴ although the theoretician and research man needs full training no less than does the future practitioner. Thus the "medical closed shop" works doubly against progress in psychoanalytic theory. It is small wonder that divergences of observation and thinking among psychoanalysts tend to be resolved not by theoretical or empirical decision but by orthodoxy and secessions.

The nature of psychoanalytic practice does not foster theoretical development. The long workdays, spent closeted with patients, provide neither the necessary time and leisure nor the detachment. The solitary character of the practice minimizes that kind of collegial interchange which is the fertile soil of theory making. The grants available and the institutions which relieve some psychoanalysts from the burden of full-time practice and provide opportunities for such interchange are for the privileged few. While no science has more than a few theoreticians at a time, those few always emerge from the many who try. Where only a few can try, the prospects remain dim, however well or poorly the few may be chosen.

The effect on the psychoanalyst of the limitation on the number of patients he can see is enhanced by the limited range of people considered treatable by psychoanalysis and by the limited number who can afford it. Moreover, the outstanding psychoanalysts sooner or later become training analysts, and then part of their time is occupied with an even more limited group: the kind of people who want to become psychoanalysts and pass through the sieve of the training committees. These limitations are particularly crippling to the development of the psychosocial aspects of the theory, but they also leave psychoanalysis centered on its clinical aspects, to the neglect of its general theory. True, the clinical theory needs further development and its methods are so far indispensable for the study of a wide range of phenomena; but there is another wide range of phenomena, crucial for the development of the general theory, that is not amenable to study in the therapeutic situation where the patient's interest is—and should be—the guide. The development of ego psychology is particularly affected by this limitation.

It seems that without more scholarly and academic training, and without the admission of nonmedical students to such training, the main obstacles in the way of the development of psychoanalytic theory are bound to persist. It is unlikely that medical schools or psychology departments would do better than the psychoanalytic Institutes: neither their traditions, nor their chances of recruiting training staffs, nor the complexity of the training problem to be met seems to bode well for such "simple" solutions.

¹⁰⁴ Since this was written, initial steps have been taken by the American Psychoanalytic Association to explore ways to change this situation.

Now, the obstacles to theoretical progress in psychology: the "scientific method," the addiction to a single method (or limited set of methods), and the measuring rage stand out.

Theory making, i.e., theoretical progress, begins in familiarity with phenomena and in thinking about them (or about the theories pertaining to them). It continues in hunches and speculations, some of which are amenable to empirical test; others, which spin relations between concepts and theories, or restructure and systematize them, are not and need not be, though they may well lead to conclusions which again can and must be subjected to empirical test.

The "scientific method" is the canon by which that record is made which we call science the codified, interconnected body of accepted knowledge. But it is not the canon for making discoveries, nor the canon for making theories. Nor is the canon, by which the scientific record is made, unique and static: it changes with the change in the methods, subject matter, and aims of research. Dingle, the British historian of science, had harsher words about the "scientific method, or methodology as it is often called now":

. . . a discipline conducted for the most part by logicians unacquainted with the practice of science, and it consists mainly of a set of principles by which accepted conclusions can best be reached by those who already know them. When we compare these principles with the steps by which the discoveries were actually made we find scarcely a single instance in which there is the slightest resemblance. If experience is to be any guide to us at all—and what scientist can think otherwise—we must conclude that there is only one scientific method: produce a genius and let him do what he likes . . . the best we can do is to learn to spot natural genius . . . and protect it, by fiery dragons if need be, from the god of planning [46, pp. 38–39].

Beveridge [24] described scientific investigation as an art. Theory making may be described as a work of imagination; the "scientific method" comes into play only in testing the theory and in making the record. But even there, however much the scientific method can help to design economic and valid tests, the essential ingredient is still the ingenuity in inventing a method which connects the phenomena and the theory.

The stress on the "scientific method" becomes an obstacle to theoretical advance in several ways. *First*, the stress on teaching the scientific method and the design of experiment diverts attention from training in observation. *Second*, it discourages the budding investigator's interest and trust in his own hunches and speculations. *Third*, it makes the "scientific method" and the "design of experiment" appear as a sure-fire way to produce "research findings." The findings thus produced clutter our literature and crowd out the interest in *methods* of experimenting

and observing. *Fourth*, it leads to a publication policy (and, through it, to a training by precept) such that the publications conform to the "scientific method" and cover up the actual tracks of the investigator even when by chance his tracks would be worth knowing. The publications read as though investigation consists of nothing but the application of the scientific method. Thus to the novice, our (and what is more important, his own) actual disorderly ways of productive thinking appear as an inadequacy. His self-observations, which show him that his thinking does not follow the "scientific method," become the sources of a gnawing self-doubt, which in turn only too often leads to a sterilizing discipline of thought. No wonder that in our literature few authors are surprised, few things are surprising, and a deadly boredom prevails, aided and abetted by what the given journal considers to be *the* form of scientific reporting.

The bane of the "single theory and single method" is in part synonymous with the plague called "schools of psychology." The investigator uses a method and becomes its captive. So do his students. He develops a theory which can only predict phenomena elicited by that method or a closely related one. What is not amenable to study by those methods ceases to influence the theory. In turn, all theories whose methods do not apply to the realm of phenomena in question are somehow considered "wrong," and if they are tested at all, it is by methods alien to them, and so they are obviously *found* wrong. Usually, however, they are ignored altogether. As a result, certain methods become "canonized," the study of a limited range of phenomena becomes the only "proper study of man," and those who try to reunite the field of psychology, so fragmented by a few methods, are regarded as "philosophers" in the pejorative sense of the word. To be a theorist becomes an opprobrium: this is the particular form of anti-intellectualism which is endemic in present-day psychology. No new methods (i.e., ways of experimenting, in contradistinction to designs of experiment) are sought to break the splendid isolation of the self-encapsulated realms of phenomena thus created. Methodological thinking, which deals with the relation of method and theory, and attempts to establish what is an artifact of the investigative method and what is "the nature of the beast," remains mostly beyond the ken of the psychologist.¹⁰⁵

The "measuring rage," already discussed, is particularly characteristic of the experimental work in clinical and personality psychology. It expresses and fosters a disregard for theory, and is thus a major obstacle

¹⁰⁵ This methodological implication of Brunswik's "representative sampling of design" is often overlooked. For brevity and emphasis, I deliberately overstate these points: for instance, the "artifact" issue is by no means as simple as the above statement suggests.

to theoretical advancement. But it also distracts attention from the general problem of mathematization and the specific problem of dimensional quantification. We may not be too far off the mark in suggesting that the malaise of psychology which is manifested in the "measuring rage" is the same as the one responsible for the epidemic-like popularity in psychology of "information theory," "open systems," "stress syndrome," and other extrapsychological achievements. Conceptions and methods can be borrowed from other sciences: all that is useful should be used. But the epidemic of grasping at every likely new achievement of other sciences seems to be a symptomatic giveaway: salvation is expected from the outside and not from results achieved by the sweat of our own brows. At the root of it is a lack of self-confidence: the lack of assurance that psychology knows where it has come from and where it is going.

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A THEORY OF THERAPY, PERSONALITY, AND INTERPERSONAL RELATIONSHIPS, AS DEVELOPED IN THE CLIENT-CENTERED FRAMEWORK

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INTRODUCTION

Being one who has deprecated the use of compulsion as a means of altering personality and behavior, it is no doubt singularly appropriate that I should be forced to acknowledge the value of the gentle compulsion of a formal request. For some time I had recognized the need of a more adequate and more up-to-date statement of the theories which have been developing in the group associated with client-centered therapy. This might well have remained in the realm of good intentions, had it not been for the formal request from the American Psychological Association, in connection with its Study of the Status and Development of Psychology in the United States, to prepare a systematic statement of this developing theory. To join with others who were endeavoring to formulate their own theories and to use, so far as possible, a common outline—this seemed to be both an obligation and an opportunity which could not be refused. It is this softly voiced but insistent pressure from my colleagues which has caused me to write the following pages now, rather than at some later date. For this pressure I am grateful.

The soil of the theory. No theory can be adequately understood without some knowledge of the cultural and personal soil from which it springs. Consequently I am pleased that the first item of the suggested outline requests a thorough discussion of background factors. This means, I fear, that I must take the reader through some autobiographical material since, although the client-centered orientation has become very much of a group enterprise in every respect, I, as an individual, carry a considerable responsibility for its initiation and for the beginning formulation of its theories. I shall, therefore, mention briefly some cultural influences and personal experiences which may or may not

have relevance to the theory itself. I shall not attempt to evaluate these influences, since I am probably a poor judge of the part they have played.

I lived my childhood as a middle child in a large, close-knit family, where hard work and a highly conservative (almost fundamentalist) Protestant Christianity were about equally revered. When the family moved to a farm at the time I was twelve, I became deeply interested and involved in scientific agriculture. The heavy research volumes I read on my own initiative in the next few years regarding feeds and feeding, soils, animal husbandry, and the like, instilled in me a deep and abiding respect for the scientific method as a means of solving problems and creating new advances in knowledge. This respect was reinforced by my first years in college, where I was fond of the physical and biological sciences. In my work in history I also realized something of the satisfactions of scholarly work.

Having rejected the family views of religion, I became interested in a more modern religious viewpoint and spent two profitable years in Union Theological Seminary, which at that time was deeply committed to a freedom of philosophical thought which respected any honest attempt to resolve significant problems, whether this led into or away from the church. My own thinking led me in the latter direction, and I moved "across the street" to Teachers College, Columbia University. Here I was exposed to the views of John Dewey, not directly, but through William H. Kilpatrick. I also had my first introduction to clinical psychology in the warmly human and common-sense approach of Leta Stetter Hollingworth. There followed a year of internship at the Institute for Child Guidance, then in its chaotic but dynamic first year of existence. Here I gained much from the highly Freudian orientation of most of its psychiatric staff, which included David Levy and Lawson Lowrey. My first attempts at therapy were carried on at the Institute. Because I was still completing my doctorate at Teachers College, the sharp incompatibility of the highly speculative Freudian thinking of the Institute with the highly statistical and Thorndikean views at Teachers College was keenly felt.

There followed twelve years in what was essentially a community child guidance clinic in Rochester, New York. This was a period of comparative isolation from the thinking of others. The psychology department of the University of Rochester was uninterested in what we were doing because our work was not, in its opinion, in the field of psychology. Our colleagues in the social agencies, schools, and courts knew little and cared less about psychological ideologies. The only element which carried weight with them was the ability to get results in working with maladjusted individuals. The staff was eclectic, of diverse

background, and our frequent and continuing discussion of treatment methods was based on our practical everyday working experience with the children, adolescents, and adults who were our clients. It was the beginning of an effort, which has had meaning for me ever since, to discover the order which exists in our experience of working with people. The volume on the *Clinical Treatment of the Problem Child* was one outcome of this effort.

During the second half of this period there were several individuals who brought into our group the controversial therapeutic views of Otto Rank and the Philadelphia group of social workers and psychiatrists whom he had influenced. Personal contact with Rank was limited to a three-day institute we arranged; nevertheless his thinking had a very decided impact on our staff and helped me to crystallize some of the therapeutic methods we were groping toward. For by this time I was becoming more competent as a therapist, and beginning to sense a discoverable orderliness in this experience, an orderliness which was inherent *in* the experience, and (unlike some of the Freudian theories which had grown so far from their original soil) did not have to be imposed *on* the experience.

Though I had carried on some part-time university teaching throughout the Rochester years, the shift to a faculty position at Ohio State University was a sharp one. I found that the emerging principles of therapy, which I had experienced largely on an implicit basis, were by no means clear to well-trained, critically minded graduate students. I began to sense that what I was doing and thinking in the clinical field was perhaps more of a new pathway than I had recognized. The paper I presented to the Minnesota chapter of Psi Chi in December, 1940, (later chapter 2 of *Counseling and Psychotherapy*) was the first conscious attempt to develop a relatively new line of thought. Up to that time I had felt that my writings were essentially attempts to distill out more clearly the principles which "all clinicians" were using.

The new influence at Ohio State, which continued to be felt in my years at Chicago, was the impact of young men and women—intellectually curious, often theoretically oriented, eager to learn from experience and to contribute through research and theory to the development of a field of knowledge. Through their mistakes as well as their successes in therapy, through their research studies, their critical contributions, and through our shared thinking, have come many of the recent developments in this orientation.

In the past decade at the University of Chicago the new elements which stand out most sharply are the opportunity for and the encouragement of research, the inclusion of graduate students from education, theology, human development, sociology, industrial relations, as well as

psychology, in the ramified activities of the Counseling Center, and the creative thinking of my faculty colleagues, especially those connected with the Center.

The persistent influence which might not be fully recognized, because it is largely implicit in the preceding paragraphs, is the continuing clinical experience with individuals who perceive themselves, or are perceived by others to be, in need of personal help. Since 1928, for a period now approaching thirty years, I have spent probably an average of 15 to 20 hr per week, except during vacation periods, in endeavoring to understand and be of therapeutic help to these individuals. To me, they seem to be the major stimulus to my psychological thinking. From these hours, and from my relationships with these people, I have drawn most of whatever insight I possess into the meaning of therapy, the dynamics of interpersonal relationships, and the structure and functioning of personality.

Some basic attitudes. Out of this cultural and personal soil have grown certain basic convictions and attitudes which have undoubtedly influenced the theoretical formulation which will be presented. I will endeavor to list some of these views which seem to me relevant:

1. I have come to see both research and theory as being aimed toward the inward ordering of significant experience. Thus research is not something esoteric, nor an activity in which one engages to gain professional kudos. It is the persistent, disciplined effort to make sense and order out of the phenomena of subjective experience. Such effort is justified because it is satisfying to perceive the world as having order and because rewarding results often ensue when one understands the orderly relationships which appear to exist in nature. One of these rewarding results is that the ordering of one segment of experience in a theory immediately opens up new vistas of inquiry, research, and thought, thus leading one continually forward.

Thus the primary reason for research and systematic theory in the field of therapy is that it is personally dissatisfying to permit the cumulating experiences of therapeutic hours to remain as a conglomeration of more or less isolated events. It feels as though there is an order in these events. What could it be? And of any hunch regarding the inherent order, it is necessary to ask the question, is this really true, or am I deceiving myself? Thus slowly there is assembled a body of facts, and systematic constructs to explain those facts, which have as their basic function the satisfaction of a need for order which exists in me.

(I have, at times, carried on research for purposes other than the above to satisfy others, to convince opponents and sceptics, to gain prestige, and for other unsavory reasons. These errors in judgment and activity have only deepened the above positive conviction.)

2. It is my opinion that the type of understanding which we call science can begin anywhere, at any level of sophistication. To observe acutely, to think carefully and creatively—these activities, not the accumulation of laboratory instruments, are the beginnings of science. To observe that a given crop grows better on the rocky hill than in the lush bottom land, and to think about this observation, is the start of science. To notice that most sailors get scurvy but not those who have stopped at islands to pick up fresh fruit is a similar start. To recognize that, when a person's views of himself change, his behavior changes accordingly, and to puzzle over this, is again the beginning of both theory and science. I voice this conviction in protest against the attitude, which seems too common in American psychology, that science starts in the laboratory or at the calculating machine.

3. A closely related belief is that there is a natural history of science—that science, in any given field, goes through a patterned course of growth and development. For example, it seems to me right and natural that in any new field of scientific endeavor the observations are gross, the hypotheses speculative and full of errors, the measurements crude. More important, I hold the opinion that this is just as truly science as the use of the most refined hypotheses and measurements in a more fully developed field of study. The crucial question in either case is not the degree of refinement but the direction of movement. If in either instance the movement is toward more exact measurement, toward more clear-cut and rigorous theory and hypotheses, toward findings which have greater validity and generality, then this is a healthy and growing science. If not, then it is a sterile pseudo science, no matter how exact its methods. Science is a *developing* mode of inquiry, or it is of no particular importance.

4. In the invitation to participate in the APA study, I have been asked to cast our theoretical thinking in the terminology of the independent-intervening-dependent variable, in so far as this is feasible. I regret that I find this terminology somehow uncongenial. I cannot justify my negative reaction very adequately, and perhaps it is an irrational one, for the logic behind these terms seems unassailable. But to me the terms seem static—they seem to deny the restless, dynamic, searching, changing aspects of scientific movement. There is a tendency to suppose that a variable thus labeled, remains so, which is certainly not true. The terms also seem to me to smack too much of the laboratory, where one undertakes an experiment *de novo*, with everything under control, rather than of a science which is endeavoring to wrest from the phenomena of experience the inherent order which they contain. Such terms seem to be more applicable to the advanced stages of scientific endeavor than to the beginning stages.

Please do not misunderstand. I quite realize that *after the fact*, any research investigation, or any theory constructed to relate the discovered facts, should be translatable into the language of independent and dependent variables or there is something wrong with the research or theory. But the terms seem to me better adapted to such autopsies than to the living physiology of scientific work in a new field.

5. It should be quite clear from the foregoing that the model of science which I find most helpful is not taken from the advanced stages of theoretical physics. In a field such as psychotherapy or personality the model which seems more congenial to me would be taken from the much earlier stages of the physical sciences. I like to think of the discovery of radioactivity by the Curies. They had left some pitchblende ore, which they were using for some purpose or other, in a room where they stored photographic plates. They discovered that the plates had been spoiled. In other words, first there was the observation of a dynamic event. This event might have been due to a multitude of causes. It might have been a flaw in the manufacture of the plates. It might have been the humidity, the temperature, or any one of a dozen other things. But acute observation and creative thinking fastened on a hunch regarding the pitchblende, and this became a tentative hypothesis. Crude experiments began to confirm the hypothesis. Only slowly was it discovered that it was *not* the pitchblende, but a strange element *in* the pitchblende which was related to the observed effect. Meanwhile a theory had to be constructed to bring this strange phenomenon into orderly relationship with other knowledge. And although the theory in its most modest form had to do with the effect of radium on photographic plates, in its wider and more speculative reaches it was concerned with the nature of matter and the composition of the universe. By present-day standards in the physical sciences, this is an example of a primitive stage of investigation and theory construction. But in the fields in which I am most deeply interested I can only hope that we are approaching such a stage. I feel sure that we are not beyond it.

6. Another deep-seated opinion has to do with theory. I believe that there is only one statement which can accurately apply to all theories—from the phlogiston theory to the theory of relativity, from the theory I will present to the one which I hope will replace it in a decade—and that is that at the time of its formulation every theory contains an unknown (and perhaps at that point an unknowable) amount of error and mistaken inference. The degree of error may be very great, as in the phlogiston theory, or small, as I imagine it may be in the theory of relativity, but unless we regard the discovery of truth as a closed and finished book, then there will be new discoveries which will contradict the best theories which we can now construct.

To me this attitude is very important, for I am distressed at the manner in which small-caliber minds immediately accept a theory—almost any theory—as a dogma of truth. If theory could be seen for what it is—a fallible, changing attempt to construct a network of gossamer threads which will contain the solid facts—then a theory would serve as it should, as a stimulus to further creative thinking.

I am sure that the stress I place on this grows in part out of my regret at the history of Freudian theory. For Freud, it seems quite clear that his highly creative theories were never more than that. He kept changing, altering, revising, giving new meaning to old terms—always with more respect for the facts he observed than for the theories he had built. But at the hands of insecure disciples (so it seems to me), the gossamer threads became iron chains of dogma from which dynamic psychology is only recently beginning to free itself. I feel that every formulation of a theory contains this same risk and that, at the time a theory is constructed, some precautions should be taken to prevent it from becoming dogma.

7. I share with many others the belief that truth is unitary, even though we will never be able to know this unity. Hence any theory, derived from almost any segment of experience, if it were complete and completely accurate, could be extended indefinitely to provide meaning for other very remote areas of experience. Tennyson expressed this in sentimental fashion in his "Flower in the Crannied Wall." I too believe that a complete theory of the individual plant would show us "what God and man is."

The corollary, however, is of equal importance and is not so often stated. A slight error in a theory may make little difference in providing an explanation of the observed facts out of which the theory grew. But when the theory is projected to explain more remote phenomena, the error may be magnified, and the inferences from the theory may be completely false. A very slight error in the understanding of Tennyson's flower may give a grossly false understanding of man. Thus every theory deserves the greatest respect in the area from which it was drawn from the facts and a decreasing degree of respect as it makes predictions in areas more and more remote from its origin. This is true of the theories developed by our own group.

8. There is one other attitude which I hold, which I believe has relevance for the proper evaluation of any theory I might present. It is my belief in the fundamental predominance of the subjective. Man lives essentially in his own personal and subjective world, and even his most objective functioning, in science, mathematics, and the like, is the result of subjective purpose and subjective choice. In relation to research and theory, for example, it is my subjective perception that the machinery of

science as we know it—operational definitions, experimental method, mathematical proof—is the best way of avoiding self-deception. But I cannot escape the fact that this is the way it appears to me, and that had I lived two centuries ago, or if I were to live two centuries in the future, some other pathway to truth might seem equally or more valid. To put it more briefly, it appears to me that though there may be such a thing as objective truth, I can never know it; all I can know is that some statements appear to me subjectively to have the qualifications of objective truth. Thus there is no such thing as Scientific Knowledge; there are only individual perceptions of what appears to each person to be such knowledge.

Since this is a large and philosophical issue, not too closely related to what follows, I shall not endeavor to state it more fully here but refer any who are interested to an article in which I have tried to expound this view somewhat more fully [67]. I mention it here only because it is a part of the context in which my theoretical thinking has developed.

THE GENERAL STRUCTURE OF OUR SYSTEMATIC THINKING

Before proceeding to the detailed statement of some of our theoretical views, I believe it may be helpful to describe some of the interrelationships between various portions of our theoretical formulations.

The earliest portion, most closely related to observed fact, most heavily supported by evidence, is the theory of psychotherapy and personality change which was constructed to give order to the phenomena of therapy as we experienced it.

In this theory there were certain hypotheses regarding the nature of personality and the dynamics of behavior. Some of these were explicit, some implicit. These have been developed more fully into a theory of personality. The purpose has been to provide ourselves with a tentative understanding of the human organism and its developing dynamics—an attempt to make sense of this person who comes to us in therapy.

Implicit in the theories of therapy and of personality are certain hypotheses regarding the outcomes of therapy—hence, hypotheses regarding a more socially constructive or creative individual. In the last few years we have endeavored to spell out the picture of the theoretical end point of therapy, the maximally creative, self-actualizing, or fully functioning person.

In another direction, our understanding of the therapeutic relationship has led us to formulate theoretical statements regarding all interpersonal relationships, seeing the therapeutic relationship simply

as one special case. This is a very new and tentative development, which we believe has promise.

Finally, it has seemed that if our views of therapy have any validity they have application in all those fields of human experience and endeavor which involve (a) interpersonal relationships and (b) the aim or potentiality of development or change in personality and behavior.

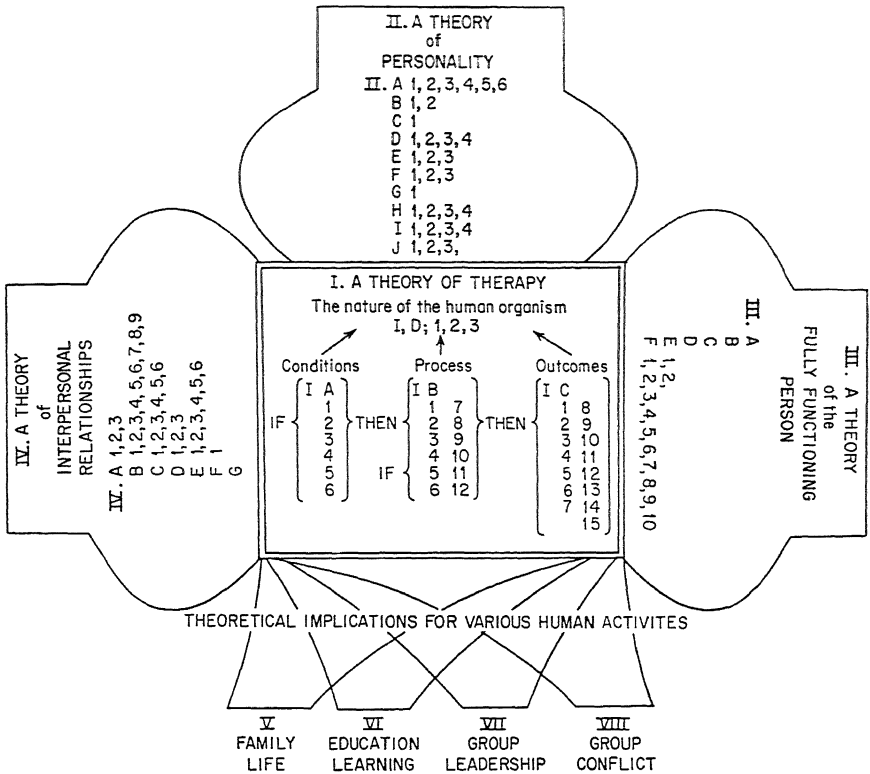


FIG. 1

Consequently a cluster of partially developed theories exists in relation to such fields as family life, education, group leadership, and situations of group tension and conflict.

The accompanying chart may help the reader to see and understand these relationships between different aspects of our theories. It should be clear that the chart reads from the center, and that the developments have taken place in the four directions indicated. It should also be remembered that the possibility of magnification of error in the theory increases as one goes out from the center. By and large, there is less

evidence available in these peripheral areas than in the center. Entered in the chart are the identifying numbers of the various propositions which follow, so that in reading any specific portion of the theory the reader may refer back to see its organic relationship to other parts of the theoretical structure.

Before proceeding to set forth something of the theories themselves, I should like gratefully to stress the extent to which this is basically a group enterprise. I have drawn upon specific written contributions to theory made by Victor Raimy, Richard Hogan, Stanley Standal, John Butler, and Thomas Gordon. Many others have contributed to my thinking in ways known and unknown, but I would particularly like to mention the valuable influence of Oliver Bown, Desmond Cartwright, Arthur Combs, Eugene Gendlin, A. H. Maslow, Julius Seeman, John Shlien, and Donald Snygg on the theories which I am about to present. Yet these individuals are by no means to be held responsible for what follows, for their own attempts to order experience have often led them into somewhat different channels of thinking.

Definitions of constructs. In the development of our theories various systematic constructs have emerged, gradually acquiring sharper and more specific meaning. Also terms in common usage have gradually acquired somewhat specialized meanings in our theoretical statements. In this section I have endeavored to define, as rigorously as I am able, these constructs and terms. These definitions supply the means by which the theory may be more accurately understood.

In this section one will find first a numbered list of all of the constructs defined, grouped in related clusters. There are eleven of these clusters, each with a focal concept. If these focal concepts are understood, the understanding of each of the related terms should not be difficult, since each of the constructs within a group has a close and meaningful relationship to the others.

Following the list one will find each of the constructs in the order numbered. Each is defined, and explanatory comment is often added.

In connection with one cluster of concepts, those having to do with the self, there is a long digression giving the "case history" of the development of that construct. This is intended to illustrate the way in which most of the constructs in this theoretical system have been developed, not as armchair constructs but out of a continuing interplay between therapeutic experience, abstract conceptualizing, and research using operationally defined terms.

It is quite possible that such a section, devoted entirely to definitions, will prove dull reading. The reader may prefer to go at once to the theory of therapy in the following section, where he will find each defined term printed in italics. He may then refer back to this section for the exact meaning of each such term.

Grouping of Definitions

- Actualizing tendency and related construct
 - 1. Actualizing tendency
 - 2. Tendency toward self-actualization
- Experience and related constructs
 - 3. Experience (noun)
 - 4. Experience (verb)
 - 5. Feeling, Experiencing a feeling
- Awareness and related constructs
 - 6. Awareness, Symbolization, Consciousness
 - 7. Availability to awareness
 - 8. Accurate symbolization
 - 9. Perceive, Perception
 - 10. Subceive, Subception
- Self and related constructs
 - 11. Self-experience
 - 12. Self, Concept of self, Self-structure
 - 13. Ideal self
- Incongruence and related constructs
 - 14. Incongruence between self and experience
 - 15. Vulnerability
 - 16. Anxiety
 - 17. Threat
 - 18. Psychological maladjustment
- The response to threat
 - 19. Defense, Defensiveness
 - 20. Distortion in awareness, Denial to awareness
 - 21. Intensionality
- Congruence and related constructs
 - 22. Congruence of self and experience
 - 23. Openness to experience
 - 24. Psychological adjustment
 - 25. Extensionality
 - 26. Mature, Maturity
- Unconditional positive regard and related constructs
 - 27. Contact
 - 28. Positive regard
 - 29. Need for positive regard
 - 30. Unconditional positive regard
 - 31. Regard complex
 - 32. Positive self-regard
 - 33. Need for self-regard
 - 34. Unconditional self-regard

Conditions of worth

35. Conditions of worth

Constructs related to valuing

36. Locus of evaluation

37. Organismic valuing process

Constructs related to source of knowledge

38. Internal frame of reference

39. Empathy

40. External frame of reference

1. *Actualizing tendency*. This is the inherent tendency of the organism to develop all its capacities in ways which serve to maintain or enhance the organism. It involves not only the tendency to meet what Maslow [45] terms "deficiency needs" for air, food, water, and the like, but also more generalized activities. It involves development toward the differentiation of organs and of functions, expansion in terms of growth, expansion of effectiveness through the use of tools, expansion and enhancement through reproduction. It is development toward autonomy and away from heteronomy, or control by external forces. Angyal's statement [2] could be used as a synonym for this term: "Life is an autonomous event which takes place between the organism and the environment. Life processes do not merely tend to preserve life but transcend the momentary status quo of the organism, expanding itself continually and imposing its autonomous determination upon an ever increasing realm of events."

It should be noted that this basic actualizing tendency is the only motive which is postulated in this theoretical system. It should also be noted that it is the organism as a whole, and only the organism as a whole, which exhibits this tendency. There are no homunculi, no other sources of energy or action in the system. The self, for example, is an important construct in our theory, but the self does not "do" anything. It is only one expression of the general tendency of the organism to behave in those ways which maintain and enhance itself.

It might also be mentioned that such concepts of motivation as are termed need-reduction, tension-reduction, drive-reduction, are included in this concept. It also includes, however, the growth motivations which appear to go beyond these terms: the seeking of pleasurable tensions, the tendency to be creative, the tendency to learn painfully to walk when crawling would meet the same needs more comfortably.

2. *Tendency toward self-actualization*. Following the development of the self-structure, this general tendency toward actualization expresses itself also in the actualization of that portion of the experience of the organism which is symbolized in the self. If the self and the total

experience of the organism are relatively congruent, then the actualizing tendency remains relatively unified. If self and experience are incongruent, then the general tendency to actualize the organism may work at cross purposes with the subsystem of that motive, the tendency to actualize the self.

This definition will be better understood when various of its terms—self, incongruence, etc.—are defined. It is given here because it is a sub-aspect of motivation. It should perhaps be reread after the other terms are more accurately understood.

3. *Experience* (noun). This term is used to include all that is going on within the envelope of the organism at any given moment which is potentially available to awareness. It includes events of which the individual is unaware, as well as all the phenomena which are in consciousness. Thus it includes the psychological aspects of hunger, even though the individual may be so fascinated by his work or play that he is completely unaware of the hunger; it includes the impact of sights and sounds and smells on the organism, even though these are not in the focus of attention. It includes the influence of memory and past experience, as these are active in the moment, in restricting or broadening the meaning given to various stimuli. It also includes all that is present in immediate awareness or consciousness. It does not include such events as neuron discharges or changes in blood sugar, because these are not directly available to awareness. It is thus a psychological, not a physiological definition.

Synonyms are “experiential field,” or the term “phenomenal field” as used by Snygg and Combs, which also covers more than the phenomena of consciousness. I have in the past used such phrases as “sensory and visceral experiences” and “organic experiences” in the attempt to convey something of the total quality of this concept.

It is to be noted that experience refers to the given moment, not to some accumulation of past experience. It is believed that this makes the operational definition of experience, or of *an* experience, which is a given segment of the field, more possible.

4. *Experience* (verb). To experience means simply to receive in the organism the impact of the sensory or physiological events which are happening at the moment.

Often this process term is used in the phrase “to experience in awareness” which means to symbolize in some accurate form at the conscious level the above sensory or visceral events. Since there are varying degrees of completeness in symbolization, the phrase is often “to experience more fully in awareness,” thus indicating that it is the extension of this

process toward more complete and accurate symbolization to which reference is being made.

5. *Feeling, Experiencing a feeling.* This is a term which has been heavily used in writings on client-centered therapy and theory. It denotes an emotionally tinged experience, together with its personal meaning. Thus it includes the emotion but also the cognitive content of the meaning of that emotion in its experiential context. It thus refers to the unity of emotion and cognition as they are experienced inseparably in the moment. It is perhaps best thought of as a brief theme of experience, carrying with it the emotional coloring and the perceived meaning to the individual. Examples would include "I feel angry at myself," "I feel ashamed of my desires when I am with her," "For the first time, right now, I feel that you like me." This last is an example of another phenomenon which is relevant to our theory, and which has been called *experiencing a feeling fully*, in the immediate present. The individual is then congruent in his experience (of the feeling), his awareness (of it), and his expression (of it).

6. *Awareness, Symbolization, Consciousness.* These three terms are defined as synonymous. To use Angyal's expression, consciousness (or awareness) is the symbolization of some of our experience. Awareness is thus seen as the symbolic representation (not necessarily in verbal symbols) of some portion of our experience. This representation may have varying degrees of sharpness or vividness, from a dim awareness of something existing as ground, to a sharp awareness of something which is in focus as figure.

7. *Availability to awareness.* When an experience can be symbolized freely, without defensive denial and distortion, then it is available to awareness.

8. *Accurate symbolization.* The symbols which constitute our awareness do not necessarily match, or correspond to, the "real" experience, or to "reality." Thus the psychotic is aware of (symbolizes) electrical impulses in his body which do not seem in actuality to exist. I glance up quickly and perceive a plane in the distance, but it turns out to be a gnat close to my eye. It seems important to distinguish between those awarenesses which, in common-sense terms, are real or accurate and those which are not. But how can this be conceptualized if we are trying to think rigorously?

The most adequate way of handling this predicament seems to me to be to take the position of those who recognize that all perception (and I would add, all awareness) is transactional in nature, that it is a construction from our past experience and a hypothesis or prognosis for the future. Thus the examples given are both hypotheses which

can be checked. If I brush at the gnat and it disappears, it increases the probability that what I was aware of *was* a gnat and not a plane. If the psychotic were able to permit himself to check the electric currents in his body, and to see whether they have the same characteristics as other electric currents, he would be checking the hypothesis implicit in his awareness. Hence when we speak of accurate symbolization in awareness, we mean that the hypotheses implicit in the awareness will be borne out if tested by acting on them.

We are, however, well over the border line of simple awareness and into the realm which is usually classified as perception, so let us proceed to a consideration of that concept.

9. *Perceive, Perception.* So much has the meaning of this term changed that one definition has been given as follows: "Perception is that which comes into consciousness when stimuli, principally light or sound, impinge on the organism from the outside" [40, p. 250]. Although this seems a bit too general, it does take account of the work of Hebb, Riesen, and others, which indicates that the impingement of the stimuli and the meaning given to the stimuli are inseparable parts of a single experience.

For our own definition we might say that a perception is a hypothesis or prognosis for action which comes into being in awareness when stimuli impinge on the organism. When we perceive "this is a triangle," "that is a tree," "this person is my mother," it means that we are making a prediction that the objects from which the stimuli are received would, if checked in other ways, exhibit properties we have come to regard, from our past experience, as being characteristic of triangles, trees, mother.

Thus we might say that perception and awareness are synonymous, perception being the narrower term, usually used when we wish to emphasize the importance of the stimulus in the process, and awareness the broader term, covering symbolizations and meanings which arise from such purely internal stimuli as memory traces, visceral changes, and the like, as well as from external stimuli.

To define perception in this purely psychological fashion is not meant to deny that it can be defined in physiological fashion by referring to the impact of a pattern of light rays upon certain nerve cells, for example. For our purpose, however, the psychological definition seems more fruitful, and it is in this sense that the term will be used in our formulations.

10. *Subceive, Subception.* McCleary and Lazarus [46] formulated this construct to signify discrimination without awareness. They state that "even when a subject is unable to report a visual discrimination he is still able to make a stimulus discrimination at some level below that

required for conscious recognition." Thus it appears that the organism can discriminate a stimulus and its meaning for the organism without utilizing the higher nerve centers involved in awareness. It is this capacity which, in our theory, permits the individual to discriminate an experience as threatening, without symbolization in awareness of this threat.

11. *Self-experience*. This is a term coined by Standal [80], and defined as being any event or entity in the phenomenal field discriminated by the individual which is also discriminated as "self," "me," "I," or related thereto. In general self-experiences are the raw material of which the organized self-concept is formed.

12. *Self, Concept of self, Self-structure*. These terms refer to the organized, consistent conceptual gestalt composed of perceptions of the characteristics of the "I" or "me" and the perceptions of the relationships of the "I" or "me" to others and to various aspects of life, together with the values attached to these perceptions. It is a gestalt which is available to awareness though not necessarily in awareness. It is a fluid and changing gestalt, a process, but at any given moment it is a specific entity which is at least partially definable in operational terms by means of a *Q* sort or other instrument or measure. The term self or self-concept is more likely to be used when we are talking of the person's view of himself, self-structure when we are looking at this gestalt from an external frame of reference.

13. *Ideal self*. Ideal self (or self-ideal) is the term used to denote the self-concept which the individual would most like to possess, upon which he places the highest value for himself. In all other respects it is defined in the same way as the self-concept.

A digression on the case history of a construct. Since the abstraction which we term the self is one of the central constructs in our theory, it may be helpful to interpose a somewhat lengthy digression at this point in our list of definitions in order to relate something of the development of this construct. In so doing we will also be illustrating the manner in which most of these defined constructs have come into being in our theory.

Speaking personally, I began my work with the settled notion that the "self" was a vague, ambiguous, scientifically meaningless term which had gone out of the psychologist's vocabulary with the departure of the introspectionists. Consequently I was slow in recognizing that when clients were given the opportunity to express their problems and their attitudes in their own terms, without any guidance or interpreta-

tion, they tended to talk in terms of the self. Characteristic expressions were attitudes such as these: "I feel I'm not being my real self." "I wonder who I am, really." "I wouldn't want anyone to know the real me." "I never had a chance to be myself." "It feels good to let myself go and just *be* myself here." "I think if I chip off all the plaster facade I've got a pretty solid self—a good substantial brick building, underneath." It seemed clear from such expressions that the self was an important element in the experience of the client, and that in some odd sense his goal was to become his "real self."

Raimy [54] produced a careful and searching definition of the self-concept which was helpful in our thinking. There seemed to be no operational way of defining it at that point. Attitudes toward the self could be measured, however, and Raimy and a number of others began such research. Self-attitudes were determined, operationally, by the categorizing of all self-referent terms in interviews preserved in verbatim form by electrical recording. The categories used had a satisfactory degree of interjudge reliability, thus making them suitable scientific constructs for our work. We were encouraged to find that these self-referent attitudes altered significantly in therapy as we had hypothesized they would.

As we focused more upon the concept of the self, clinical experience again gave us further clues as to its nature. For example, in the process of change which appeared to occur in therapy, it was not at all uncommon to find violent fluctuation in the concept of the self. A client, during a given interview, would come to experience himself quite positively. He felt he was worthwhile, that he could meet life with the capacities he possessed, and that he was experiencing a quiet confidence. Three days later he might return with a completely reversed conception of himself. The same evidence now proved an opposite point. The positive new choice he had made now was an instance of silly immaturity; the valid feelings courageously expressed to his colleagues now were clearly inadequate. Often such a client could date, to the moment, the point at which, following some very minor incident, the balance was upset, and his picture of himself had undergone a complete flip-flop. During the interview it might as suddenly reverse itself again.

Consideration of this phenomenon made it clear that we were not dealing with an entity of slow accretion, of step-by-step learning, of thousands of unidirectional conditionings. These might all be involved, but the product was clearly a gestalt, a configuration in which the alteration of one minor aspect could completely alter the whole pattern. One was forcibly reminded of the favorite textbook illustration of a gestalt, the double picture of the old hag and the young woman. Looked at with one mind set, the picture is clearly that of an ugly old woman. The

slightest change, and the whole becomes a portrait of an attractive girl. So with our clients. The self-concept was clearly configurational in nature.

Our clinical experience gave us another clue to the manner in which the self functioned. The conventional concept of repression as having to do with forbidden or socially taboo impulses had been recognized as inadequate to fit the facts. Often the most deeply denied impulses and feelings were positive feelings of love, or tenderness, or confidence in self. How could one explain the puzzling conglomeration of experience which seemingly could not be permitted in awareness? Gradually it was recognized that the important principle was one of consistency with the self. Experiences which were incongruent with the individual's concept of himself tended to be denied to awareness, whatever their social character. We began to see the self as a criterion by which the organism screened out experiences which could not comfortably be permitted in consciousness. Lecky's little posthumous book [43] reinforced this line of thought. We also began to understand other functions of the self in its regulatory influence on behavior, and the like.

At about this juncture Stephenson's Q technique [81] opened up the possibility of an operational definition of the self-concept. Immediately, research burgeoned. Though we feel it has barely made a start in exploiting the possible testing of hypotheses, there have already been measurements and predictions regarding the self as of this moment, the self in the past, "myself as I am with my mother," "the self I would like to be," etc. Probably the most sophisticated and significant of these studies is that completed by Chodorkoff [10], in which his hypothesis, stated informally, is as follows: that the greater the agreement between the individual's self-description and an objective description of him, the less perceptual defensiveness he will show, and the more adequate will be his personal adjustment. This hypothesis is upheld and tends to confirm some important aspects of our theory. In general the various investigations have agreed in indicating that the self-concept is an important variable in personality dynamics and that change in the self is one of the most marked and significant changes occurring in therapy.

It should be recognized that any construct is a more or less arbitrary abstraction from experience. Thus the self could be defined in many different ways. Hilgard, for example [34], has proposed that it be defined in such a way as to include unconscious material, not available to awareness, as well as conscious material. Although we recognize that this is certainly a legitimate way of abstracting from the phenomena, we believe it is not a useful way because it produces a concept which cannot at this point be given operational definition. One cannot obtain sufficient agreement as to the content of the individual's unconscious to make

research possible. Hence we believe that it is more fruitful to define the self-concept as a gestalt which is available to awareness. This has permitted and encouraged a flood of important research.

At all times, however, we endeavor to keep in the forefront of our thinking the fact that each definition is no more than an abstraction and that the same phenomena might be abstracted in a different fashion. One of our group is working on a definition of self which would give more emphasis to its process nature. Others have felt that a plural definition, indicating many specific selves in each of various life contexts, would be more fruitful, and this way of thinking has been embodied in, for example, Nunnally's [50] research. So the search continues for a more adequate conceptualization of this area of our therapeutic experience and for more adequate technical means of providing operational definitions for the concepts which are formulated.

This concludes our interruption of the list of definitions. It is hoped that this one example will give an indication of the way in which many of our basic constructs have developed—not only the self-concept but the constructs of congruence, incongruence, defensiveness, unconditional positive regard, locus of evaluation, and the like. Although the process has been irregular, it has tended to include clinical observation, initial conceptualization, initial crude research to test some of the hypotheses involved, further clinical observation, more rigorous formulation of the construct and its functional relationships, more refined operational definitions of the construct, more conclusive research.

14. *Incongruence between self and experience.* In a manner which will be described in the theory of personality a discrepancy frequently develops between the self as perceived, and the actual experience of the organism. Thus the individual may perceive himself as having characteristics *a*, *b*, and *c*, and experiencing feelings *x*, *y*, and *z*. An accurate symbolization of his experience would, however, indicate characteristics *c*, *d*, and *e*, and feelings *v*, *w*, *x*. When such a discrepancy exists, the state is one of incongruence between self and experience. This state is one of tension and internal confusion, since in some respects the individual's behavior will be regulated by the actualizing tendency, and in other respects by the self-actualizing tendency, thus producing discordant or incomprehensible behaviors. What is commonly called neurotic behavior is one example, the neurotic behavior being the product of the actualizing tendency, whereas in other respects the individual is actualizing the self. Thus the neurotic behavior is incomprehensible to the individual himself, since it is at variance with what he consciously "wants" to do, which is to actualize a self no longer congruent with experience.

15. *Vulnerability.* Vulnerability is the term used to refer to the

state of incongruence between self and experience, when it is desired to emphasize the potentialities of this state for creating psychological disorganization. When incongruence exists, and the individual is unaware of it, then he is potentially vulnerable to anxiety, threat, and disorganization. If a significant new experience demonstrates the discrepancy so clearly that it must be consciously perceived, then the individual will be threatened, and his concept of self disorganized by this contradictory and unassimilable experience.

16. *Anxiety*. Anxiety is phenomenologically a state of uneasiness or tension whose cause is unknown. From an external frame of reference, anxiety is a state in which the incongruence between the concept of self and the total experience of the individual is approaching symbolization in awareness. When experience is *obviously* discrepant from the self-concept, a defensive response to threat becomes increasingly difficult. Anxiety is the response of the organism to the "subception" that such discrepancy may enter awareness, thus forcing a change in the self-concept.

17. *Threat*. Threat is the state which exists when an experience is perceived or anticipated (subceived) as incongruent with the structure of the self. It may be regarded as an external view of the same phenomenon which, from the internal frame of reference, is anxiety.

18. *Psychological maladjustment*. Psychological maladjustment exists when the organism denies to awareness, or distorts in awareness, significant experiences, which consequently are not accurately symbolized and organized into the gestalt of the self-structure, thus creating an incongruence between self and experience.

It may help to clarify this basic concept of incongruence if we recognize that several of the terms we are defining are simply different vantage points for viewing this phenomenon. If an individual is in a state of incongruence between self and experience and we are looking at him from an external point of view we see him as vulnerable (if he is unaware of the discrepancy), or threatened (if he has some awareness of it). If we are viewing him from a social point of view, then this incongruence is psychological maladjustment. If the individual is viewing himself, he may even see himself as adjusted (if he has no awareness of the discrepancy) or anxious (if he dimly subceives it) or threatened or disorganized (if the discrepancy has forced itself upon his awareness).

19. *Defense, Defensiveness*. Defense is the behavioral response of the organism to threat, the goal of which is the maintenance of the current structure of the self. This goal is achieved by the perceptual distortion of the experience in awareness, in such a way as to reduce the incongruity between the experience and the structure of the self, or by the denial

to awareness of an experience, thus denying any threat to the self. Defensiveness is the term denoting a state in which the behaviors are of the sort described.

20. *Distortion in awareness, Denial to awareness.* It is an observed phenomenon that material which is significantly inconsistent with the concept of self cannot be directly and freely admitted to awareness. To explain this the construct of denial or distortion has been developed. When an experience is dimly perceived (or "subceived" is perhaps the better term) as being incongruent with the self-structure, the organism appears to react with a distortion of the meaning of the experience, (making it consistent with the self) or with a denial of the existence of the experience, in order to preserve the self-structure from threat. It is perhaps most vividly illustrated in those occasional moments in therapy when the therapist's response, correctly heard and understood, would mean that the client would necessarily perceive openly a serious inconsistency between his self-concept and a given experience. In such a case, the client may respond, "I can hear the words you say, and I know I should understand them, but I just can't make them convey any meaning to me." Here the relationship is too good for the meaning to be distorted by rationalization, the meaning too threatening to be received. Hence the organism denies that there is meaning in the communication. Such outright denial of experience is much less common than the phenomenon of distortion. Thus if the concept of self includes the characteristic "I am a poor student" the experience of receiving a high grade can be easily be distorted to make it congruent with the self by perceiving in it such meanings as, "That professor is a fool"; "It was just luck"; etc.

21. *Intensionality.* This term is taken from general semantics. If the person is reacting or perceiving in an intensional fashion he tends to see experience in absolute and unconditional terms, to overgeneralize, to be dominated by concept or belief, to fail to anchor his reactions in space and time, to confuse fact and evaluation, to rely upon abstractions rather than upon reality-testing. This term covers the frequently used concept of rigidity but includes perhaps a wider variety of behaviors than are generally thought of as constituting rigidity.

It will perhaps be evident that this cluster of definitions all have to do with the organism's response to threat. Defense is the most general term: distortion and denial are the mechanisms of defense; intensionality is a term which covers the characteristics of the behavior of the individual who is in a defensive state.

22. *Congruence, Congruence of self and experience.* This is a basic concept which has grown out of therapeutic experience, in which the

individual appears to be revising his concept of self to bring it into congruence with his experience, accurately symbolized. Thus he discovers that one aspect of his experience if accurately symbolized, would be hatred for his father; another would be strong homosexual desires. He reorganizes the concept he holds of himself to include these characteristics, which would previously have been inconsistent with self.

Thus when self-experiences are accurately symbolized, and are included in the self-concept in this accurately symbolized form, then the state is one of congruence of self and experience. If this were completely true of all self-experiences, the individual would be a fully functioning person, as will be made more clear in the section devoted to this aspect of our theory. If it is true of some specific aspect of experience, such as the individual's experience in a given relationship or in a given moment of time, then we can say that the individual is to this degree in a state of congruence. Other terms which are in a general way synonymous are these: integrated, whole, genuine.

23. *Openness to experience.* When the individual is in no way threatened, then he is open to his experience. To be open to experience is the polar opposite of defensiveness. The term may be used in regard to some area of experience or in regard to the total experience of the organism. It signifies that every stimulus, whether originating within the organism or in the environment, is freely relayed through the nervous system without being distorted or channeled off by any defensive mechanism. There is no need of the mechanism of "subception" whereby the organism is forewarned of experiences threatening to the self. On the contrary, whether the stimulus is the impact of a configuration of form, color, or sound in the environment on the sensory nerves, or a memory trace from the past, or a visceral sensation of fear, pleasure, or disgust, it is completely available to the individual's awareness. In the hypothetical person who is completely open to his experience, his concept of self would be a symbolization in awareness which would be completely congruent with his experience. There would, therefore, be no possibility of threat.

24. *Psychological adjustment.* Optimal psychological adjustment exists when the concept of the self is such that all experiences are or may be assimilated on a symbolic level into the gestalt of the self-structure. Optimal psychological adjustment is thus synonymous with complete congruence of self and experience, or complete openness to experience. On the practical level, improvement in psychological adjustment is equivalent to progress toward this end point.

25. *Extensionality.* This term is taken from general semantics. If the person is reacting or perceiving in an extensional manner he tends to see experience in limited, differentiated terms, to be aware of the

space-time anchorage of facts, to be dominated by facts, not by concepts, to evaluate in multiple ways, to be aware of different levels of abstraction, to test his inferences and abstractions against reality.

26. *Mature, Maturity.* The individual exhibits mature behavior when he perceives realistically and in an extensional manner, is not defensive, accepts the responsibility of being different from others, accepts responsibility for his own behavior, evaluates experience in terms of the evidence coming from his own senses, changes his evaluation of experience only on the basis of new evidence, accepts others as unique individuals different from himself, prizes himself, and prizes others. (If his behavior has these characteristics, then there will automatically follow all the types of behavior which are more popularly thought of as constituting psychological maturity.)

These last five definitions form a cluster which grows out of the concept of congruence. Congruence is the term which defines the state. Openness to experience is the way an internally congruent individual meets new experience. Psychological adjustment is congruence as viewed from a social point of view. Extensional is the term which describes the specific types of behavior of a congruent individual. Maturity is a broader term describing the personality characteristics and behavior of a person who is, in general, congruent.

The concepts in the group of definitions which follow have all been developed and formulated by Standal [80], and have taken the place of a number of less satisfactory and less rigorously defined constructs. Essentially this group has to do with the concept of positive regard, but since all transactions relative to this construct take place in relationships, a definition of psychological contact, or minimal relationship, is set down first.

27. *Contact.* Two persons are in psychological contact, or have the minimum essential of a relationship, when each makes a perceived or subceived difference in the experiential field of the other.

This construct was first given the label of "relationship" but it was found that this led to much misunderstanding, for it was often understood to represent the depth and quality of a good relationship, or a therapeutic relationship. The present term has been chosen to signify more clearly that this is the *least* or minimum experience which could be called a relationship. If more than this simple contact between two persons is intended, then the additional characteristics of that contact are specified in the theory.

28. *Positive regard.* If the perception by me of some self-experience in another makes a positive difference in my experiential field, then I

am experiencing positive regard for that individual. In general, positive regard is defined as including such attitudes as warmth, liking, respect, sympathy, acceptance. To perceive oneself as receiving positive regard is to experience oneself as making a positive difference in the experiential field of another.

29. *Need for positive regard.* It is postulated by Standal that a basic need for positive regard, as defined above, is a secondary or learned need, commonly developed in early infancy. Some writers have looked upon the infant's need for love and affection as an inherent or instinctive need. Standal is probably on safer ground in regarding it as a learned need. By terming it the need for positive regard, he has, it is believed, selected out the significant psychological variable from the broader terms usually used.

30. *Unconditional positive regard.* Here is one of the key constructs of the theory, which may be defined in these terms: if the self-experiences of another are perceived by me in such a way that no self-experience can be discriminated as more or less worthy of positive regard than any other, then I am experiencing unconditional positive regard for this individual. To perceive oneself as receiving unconditional positive regard is to perceive that of one's self-experiences none can be discriminated by the other individual as more or less worthy of positive regard.

Putting this in simpler terms, to feel unconditional positive regard toward another is to "prize" him (to use Dewey's term, recently used in this sense by Butler). This means to value the person, irrespective of the differential values which one might place on his specific behaviors. A parent "prizes" his child, though he may not value equally all of his behaviors. Acceptance is another term which has been frequently used to convey this meaning, but it perhaps carries more misleading connotations than the phrase which Standal has coined. In general, however, acceptance and prizing are synonymous with unconditional positive regard.

This construct has been developed out of the experiences of therapy, where it appears that one of the potent elements in the relationship is that the therapist "prizes" the whole person of the client. It is the fact that he feels and shows an unconditional positive regard toward the experiences of which the client is frightened or ashamed, as well as toward the experiences with which the client is pleased or satisfied, that seems effective in bringing about change. Gradually the client can feel more acceptance of all of his own experiences, and this makes him again more of a whole or congruent person, able to function effectively. This clinical explanation will, it is hoped, help to illuminate the meaning contained in the rigorous definition.

31. *Regard complex.* The regard complex is a construct defined by

Standal as all those self-experiences, together with their interrelationships, which the individual discriminates as being related to the positive regard of a particular social other.

This construct is intended to emphasize the gestalt nature of transactions involving positive or negative regard, and their potency. Thus, for example, if a parent shows positive regard to a child in relationship to a specific behavior, this tends to strengthen the whole pattern of positive regard which has previously been experienced as coming from that parent. Likewise specific negative regard from this parent tends to weaken the whole configuration of positive regard.

32. *Positive self-regard.* This term is used to denote a positive regard satisfaction which has become associated with a particular self-experience or a group of self-experiences, in which this satisfaction is independent of positive regard transactions with social others. Though it appears that positive regard must first be experienced from others, this results in a positive attitude toward self which is no longer directly dependent on the attitudes of others. The individual, in effect, becomes his own significant social other.

33. *Need for self-regard.* It is postulated that a need for positive self-regard is a secondary or learned need, related to the satisfaction of the need for positive regard by others.

34. *Unconditional self-regard.* When the individual perceives himself in such a way that no self-experience can be discriminated as more or less worthy of positive regard than any other, then he is experiencing unconditional positive self-regard.

35. *Conditions of worth.* The self-structure is characterized by a condition of worth when a self-experience or set of related self-experiences is either avoided or sought solely because the individual discriminates it as being less or more worthy of self-regard.

This important construct has been developed by Standal to take the place of "introjected value," which was a less exact concept used in earlier formulations. A condition of worth arises when the positive regard of a significant other is conditional, when the individual feels that in some respects he is prized and in others not. Gradually this same attitude is assimilated into his own self-regard complex, and he values an experience positively or negatively solely because of these conditions of worth which he has taken over from others, not because the experience enhances or fails to enhance his organism.

It is this last phrase which deserves special note. When the individual has experienced unconditional positive regard, then a new experience is valued or not, depending on its effectiveness in maintaining or en-

hancing the organism. But if a value is "introjected" from a significant other, then this condition of worth is applied to an experience quite without reference to the extent to which it maintains or enhances the organism. It is an important specific instance of inaccurate symbolization, the individual valuing an experience positively or negatively, *as if* in relation to the criterion of the actualizing tendency, but not actually in relation to it. An experience may be perceived as organismically satisfying, when in fact this is not true. Thus a condition of worth, because it disturbs the valuing process, prevents the individual from functioning freely and with maximum effectiveness.

36. *Locus of evaluation.* This term is used to indicate the source of evidence as to values. Thus an internal locus of evaluation, within the individual himself, means that he is the center of the valuing process, the evidence being supplied by his own senses. When the locus of evaluation resides in others, their judgment as to the value of an object or experience becomes the criterion of value for the individual.

37. *Organismic valuing process.* This concept describes an ongoing process in which values are never fixed or rigid, but experiences are being accurately symbolized and continually and freshly valued in terms of the satisfactions organismically experienced; the organism experiences satisfaction in those stimuli or behaviors which maintain and enhance the organism and the self, both in the immediate present and in the long range. The actualizing tendency is thus the criterion. The simplest example is the infant who at one moment values food, and when satiated, is disgusted with it; at one moment values stimulation, and soon after, values only rest; who finds satisfying that diet which in the long run most enhances his development.

38. *Internal frame of reference.* This is all of the realm of experience which is available to the awareness of the individual at a given moment. It includes the full range of sensations, perceptions, meanings, and memories, which are available to consciousness.

The internal frame of reference is the subjective world of the individual. Only he knows it fully. It can never be known to another except through empathic inference and then can never be perfectly known.

39. *Empathy.* The state of empathy, or being empathic, is to perceive the internal frame of reference of another with accuracy, and with the emotional components and meanings which pertain thereto, as if one were the other person, but without ever losing the "as if" condition. Thus it means to sense the hurt or the pleasure of another as he senses it, and to perceive the causes thereof as he perceives them, but without

ever losing the recognition that it is *as if* I were hurt or pleased, etc. If this "as if" quality is lost, then the state is one of identification.

40. *External frame of reference.* To perceive solely from one's own subjective internal frame of reference without empathizing with the observed person or object, is to perceive from an external frame of reference. The "empty organism" school of thought in psychology is an example of this. Thus the observer says that an animal has been stimulated when the animal has been exposed to a condition which, in the observer's subjective frame of reference, is a stimulus. There is no attempt to understand, empathically, whether this is a stimulus in the animal's experiential field. Likewise the observer reports that the animal emits a response when a phenomenon occurs which, in the observer's subjective field, is a response.

We generally regard all "objects" (stones, trees, or abstractions) from this external frame of reference since we assume that they have no "experience" with which we can empathize. The other side of this coin is that anything perceived from an external frame of reference (whether an inanimate thing, an animal, or a person) becomes for us an "object" because no empathic inferences are made.

This cluster of three ways of knowing deserves some further comment. In so far as we are considering knowledge of human beings we might say that these ways of knowing exist on a continuum. They range from one's own complete subjectivity in one's own internal frame of reference to one's own complete subjectivity about another (the external frame of reference). In between lies the range of empathic inference regarding the subjective field of another.

Each of these ways of knowing is essentially a formulation of hypotheses. The differences lie in the way the hypotheses are checked. In my own internal frame of reference if I experience love or hate, enjoyment or dislike, interest or boredom, belief or disbelief, the only way I can check these hypotheses of experience is by further focusing on my experience. Do I really love him? Am I really enjoying this? Do I really believe this? are questions which can only be answered by checking with my own organism. (If I try to find out whether I really love him by checking with others, then I am observing myself as an object, am viewing myself from an external frame of reference.)

Although in the last analysis each individual lives in and by his own subjective knowledge, this is not regarded socially as "knowledge" and certainly not as scientific knowledge.

Knowledge which has any "certainty," in the social sense, involves the use of empathic inference as a means of checking, but the direction of that empathy differs. When the experience of empathic understanding is used as a source of knowledge, one checks one's empathic inferences

with the subject, thus verifying or disproving the inferences and hypotheses implicit in such empathy. It is this way of knowing which we have found so fruitful in therapy. Utilizing empathic inference to the fullest, the knowledge thus gained of the client's subjective world has led to understanding the basis of his behavior and the process of personality change.

In knowing a person or an object from the external frame of reference, our implicit hypotheses are checked with other people, but *not* with the subject of our concern. Thus a rigorous behaviorist believes that S is a stimulus for his experimental animal and R is a response, because his colleagues and even the man in the street agree with him and regard S and R in the same way. His empathic inferences are made in regard to the internal frame of reference of his colleagues, rather than in regard to the internal frame of reference of the animal.

Science involves taking an external frame of reference, in which we check our hypotheses basically through empathic inferences as to the internal frame of reference of our colleagues. They perform the same operations we have (either actually or through symbolic representation), and if they perceive the same events and meanings, then we regard our hypotheses as confirmed.

The reason for thus elaborating the different ways of knowing is that it seems to us that all ways of knowing have their usefulness, and that confusion arises only when one is not clear as to the type of knowledge which is being specified. Thus in the theory of therapy which follows one will find certain conditions of therapy specified as subjective experiencing states, another as an empathic knowledge of the client, and yet the scientific checking of the hypotheses of the theory can only be done from an external frame of reference.

I. A THEORY OF THERAPY AND PERSONALITY CHANGE

This theory is of the if-then variety. If certain conditions exist (independent variables), then a process (dependent variable) will occur which includes certain characteristic elements. If this process (now the independent variable) occurs, then certain personality and behavioral changes (dependent variables) will occur. This will be made specific.

In this and the following sections the formal statement of the theory is given briefly, in smaller type. The italicized terms or phrases in these formal statements have been defined in the previous section and are to be understood as defined. The remaining paragraphs are explanatory and do not follow the rigorous pattern of the formal statements.

A. Conditions of the Therapeutic Process

For therapy to occur it is necessary that these conditions exist.

1. That two persons are in *contact*.
2. That the first person, whom we shall term the client, is in a state of *incongruence*, being *vulnerable*, or *anxious*.
3. That the second person, whom we shall term the therapist, is *congruent* in the *relationship*.
4. That the therapist is *experiencing unconditional positive regard* toward the client.
5. That the therapist is *experiencing an empathic* understanding of the client's *internal frame of reference*.
6. That the client *perceives*, at least to a minimal degree, conditions 4 and 5, the *unconditional positive regard* of the therapist for him, and the *empathic* understanding of the therapist.

Comment. These seem to be the necessary conditions of therapy, though other elements are often or usually present. The process is more likely to get under way if the client is anxious, rather than merely vulnerable. Often it is necessary for the contact or relationship to be of some duration before the therapeutic process begins. Usually the empathic understanding is to some degree expressed verbally, as well as experienced. But the process often commences with only these minimal conditions, and it is hypothesized that it never commences *without* these conditions being met.

The point which is most likely to be misunderstood is the omission of any statement that the therapist *communicates* his empathic understanding and his unconditional positive regard to the client. Such a statement has been omitted only after much consideration, for these reasons. It is not enough for the therapist to communicate, since the communication must be received, as pointed out in condition 6, to be effective. It is not essential that the therapist *intend* such communication, since often it is by some casual remark, or involuntary facial expression, that the communication is actually achieved. However, if one wishes to stress the communicative aspect which is certainly a vital part of the living experience, then condition 6 might be worded in this fashion:

6. That the communication to the client of the therapist's empathic understanding and unconditional positive regard is, at least to a minimal degree, achieved.

The element which will be most surprising to conventional therapists is that the same conditions are regarded as sufficient for therapy, regardless of the particular characteristics of the client. It has been our experience to date that although the therapeutic relationship is used dif-

ferently by different clients, it is not necessary nor helpful to manipulate the relationship in specific ways for specific kinds of clients. To do this damages, it seems to us, the most helpful and significant aspect of the experience, that it is a genuine relationship between two persons, each of whom is endeavoring, to the best of his ability, to be himself in the interaction.¹

The "growing edge" of this portion of the theory has to do with point 3, the congruence or genuineness of the therapist in the relationship. This means that the therapist's symbolization of his own experience in the relationship must be accurate, if therapy is to be most effective. Thus if he is experiencing threat and discomfort in the relationship, and is aware only of an acceptance and understanding, then he is not congruent in the relationship and therapy will suffer. It seems important that he should accurately "be himself" in the relationship, whatever the self of that moment may be.

Should he also express or communicate to the client the accurate symbolization of his own experience? The answer to this question is still in an uncertain state. At present we would say that such feelings should be expressed, if the therapist finds himself persistently focused on his own feelings rather than those of the client, thus greatly reducing or eliminating any experience of empathic understanding, or if he finds himself persistently experiencing some feeling other than unconditional positive regard. To know whether this answer is correct demands further testing of the hypothesis it contains, and this is not simple since the courage to do this is often lacking, even in experienced therapists. When the therapist's real feelings are of this order: "I find myself fearful that you are slipping into a psychosis," or "I find myself frightened because you are touching on feelings I have never been able to resolve," then it is difficult to test the hypothesis, for it is very difficult for the therapist to express such feelings.

Another question which arises is this: is it the congruence, the wholeness, the integration of the therapist in the relationship which is important, or are the specific attitudes of empathic understanding and un-

¹This paragraph may have to be rewritten if a recent study of Kirtner [42] is confirmed. Kirtner has found, in a group of 26 cases from the Counseling Center at the University of Chicago, that there are sharp differences in the client's mode of approach to the resolution of life difficulties and that these differences are related to success in therapy. Briefly, the client who sees his problem as involving his relationships, and who feels that he contributes to this problem and wants to change it, is likely to be successful. The client who externalizes his problem and feels little self-responsibility is much more likely to be a failure. Thus the implication is that different conditions of therapy may be necessary to make personality change possible in this latter group. If this is verified, then the theory will have to be revised accordingly.

conditional positive regard vital? Again the final answer is unknown, but a conservative answer, the one we have embodied in the theory, is that for therapy to occur the wholeness of the therapist in the relationship is primary, but a part of the congruence of the therapist must be the experience of unconditional positive regard and the experience of empathic understanding.

Another point worth noting is that the stress is upon the experience *in the relationship*. It is not to be expected that the therapist is a completely congruent person at all times. Indeed if this were a necessary condition there would be no therapy. But it is enough if in this particular moment of this immediate relationship with this specific person he is completely and fully himself, with his experience of the moment being accurately symbolized and integrated into the picture he holds of himself. Thus it is that imperfect human beings can be of therapeutic assistance to other imperfect human beings.

The greatest flaw in the statement of these conditions is that they are stated as if they were all-or-none elements, whereas conditions 2 to 6 all exist on continua. At some later date we may be able to say that the therapist must be genuine or congruent to such and such a degree in the relationship, and similarly for the other items. At the present we can only point out that the more marked the presence of conditions 2 to 6, the more certain it is that the process of therapy will get under way, and the greater the degree of reorganization which will take place. This function can only be stated qualitatively at the present time.

Evidence. Confirmatory evidence, particularly of item 5, is found in the studies by Fiedler [19, 20] and Quinn [52]. Fiedler's study showed that experienced therapists of different orientations created relationships in which one of the most prominent characteristics was the ability to understand the client's communications with the meaning these communications had for the client. Quinn found that the quality of therapist communication was of crucial significance in therapy. These studies add weight to the importance of empathic understanding.

Seeman [75] found that increase in the counselor's liking for the client during therapy was significantly associated with therapeutic success. Both Seeman and Lipkin [44] found that clients who felt themselves to be liked by the therapist tended to be more successful. These studies tend to confirm condition 4 (unconditional positive regard) and condition 6 (perception of this by the client).

Though clinical experience would support condition 2, the client's vulnerability or anxiety, there is little research which has been done in terms of these constructs. The study by Gallagher [21] indicates that less anxious clients tend never to become involved in therapy, but drop out.

B. *The Process of Therapy*

When the preceding conditions exist and continue, a process is set in motion which has these characteristic directions:

1. The client is increasingly free in expressing his *feelings*, through verbal and/or motor channels.
2. His expressed feelings increasingly have reference to the *self*, rather than nonself.
3. He increasingly differentiates and discriminates the objects of his *feelings* and *perceptions*, including his environment, other persons, his *self*, his *experiences*, and the interrelationships of these. He becomes less *intensional* and more *extensional* in his *perceptions*, or to put it in other terms, his experiences are more *accurately symbolized*.
4. His expressed *feelings* increasingly have reference to the *incongruity* between certain of his *experiences* and his *concept of self*.
5. He comes to experience in awareness the threat of such *incongruence*.
 - a. This *experience of threat* is possible only because of the continued *unconditional positive regard* of the therapist, which is extended to *incongruence* as much as to *congruence*, to *anxiety* as much as to absence of *anxiety*.
6. He *experiences* fully, in *awareness*, feelings which have in the past been *denied to awareness*, or *distorted in awareness*.
7. His *concept of self* becomes reorganized to assimilate and include these *experiences* which have previously been *distorted in* or *denied to awareness*.
8. As this reorganization of the *self-structure* continues, his *concept of self* becomes increasingly *congruent* with his *experience*; the *self* now including *experiences* which previously would have been too *threatening* to be in *awareness*.
 - a. A corollary tendency is toward fewer perceptual *distortions in awareness*, or *denials to awareness*, since there are fewer *experiences* which can be *threatening*. In other words, *defensiveness* is decreased.
9. He becomes increasingly able to *experience*, without a feeling of *threat*, the therapist's *unconditional positive regard*.
10. He increasingly feels an *unconditional positive self-regard*.
11. He increasingly *experiences* himself as the *locus of evaluation*.
12. He reacts to *experience* less in terms of his *conditions of worth* and more in terms of an *organismic valuing process*.

Comment. It cannot be stated with certainty that all of these are *necessary* elements of the process, though they are all characteristic. Both from the point of view of experience, and the logic of the theory, 3, 6, 7,

8, 10, 12, are necessary elements in the process. Item 5a is not a logical step in the theory but is put in as an explanatory note.

The element which will doubtless be most puzzling to the reader is the absence of explanatory mechanisms. It may be well to restate our scientific purpose in terms of an example. *If* one strokes a piece of steel with a magnet, and *if* one places the piece of steel so that it can rotate freely, *then* it will point to the north. This statement of the if-then variety has been proved thousands of times. Why does it happen? There have been various theoretical answers, and one would hesitate to say, even now, that we know with certitude *why* this occurs.

In the same way I have been saying in regard to therapy, "If these conditions exist, *then* these subsequent events will occur." Of course we have speculations as to *why* this relationship appears to exist, and those speculations will be increasingly spelled out as the presentation continues. Nevertheless the most basic element of our theory is that if the described conditions exist, then the process of therapy occurs, and the events which are called outcomes will be observed. We may be quite wrong as to *why* this sequence occurs. I believe there is an increasing body of evidence to show that it *does* occur.

Evidence. There is confirming evidence of varying degrees of relevance for a number of these items describing the therapeutic process. Item 2 (increasing self-reference) is supported by our many recorded therapeutic cases, but has not been reduced to a statistical finding. Stock's study [82] supports item 3, indicating that client self-referent expressions become more objective, less strongly emotional. Mitchell [47] shows that clients become more extensional.

Objective clinical evidence supporting items 4, 5, and 6 is provided in the form of recordings from a case by Rogers [67].

The findings of Vargas [85] are relevant to item 7, indicating the way the self is reorganized in terms of emergent new self-perceptions. Hogan [36] and Haigh [29] have studied the decrease in defensiveness during the process, as described in item 8a, their findings being confirmatory. The increased congruence of self and experience is supported in an exhaustive single case investigation by Rogers [67]. That such congruence is associated with lack of defensiveness is found by Chodoroff [10].

Item 10, the increase in the client's positive self-regard, is well attested by the studies of Snyder [79], Seeman [76], Raimy [55], Stock [82], Strom [83], Sheerer [78], Lipkin [44]. The client's trend toward experiencing himself as the locus of evaluation is most clearly shown by Raskin's research [56], but this is supported by evidence from Sheerer [78], Lipkin [44], Kessler [41].

C. *Outcomes in Personality and Behavior*

There is no clear distinction between process and outcome. Items of process are simply differentiated aspects of outcome. Hence the statements which follow could have been included under process. For reasons of convenience in understanding, there have been grouped here those changes which are customarily associated with the terms outcomes, or results, or are observed outside of the therapeutic relationship. These are the changes which are hypothesized as being relatively permanent:

1. The client is more *congruent*, more *open to his experience*, less *defensive*.

2. He is consequently more realistic, objective, *extensional* in his *perceptions*.

3. He is consequently more effective in problem solving.

4. His *psychological adjustment* is improved, being closer to the optimum.

a. This is owing to, and is a continuation of, the changes in *self-structure* described in B7 and B8.

5. As a result of the increased *congruence* of *self* and *experience* (C4 above) his *vulnerability to threat* is reduced.

6. As a consequence of C2 above, his perception of his *ideal self* is more realistic, more achievable.

7. As a consequence of the changes in C4 and C5 his *self* is more *congruent* with his *ideal self*.

8. As a consequence of the increased *congruence* of *self* and *ideal self* (C6) and the greater *congruence* of *self* and *experience*, tension of all types is reduced—physiological tension, psychological tension, and the specific type of psychological tension defined as *anxiety*.

9. He has an increased degree of *positive self-regard*.

10. He *perceives* the *locus of evaluation* and the locus of choice as residing within himself.

a. As a consequence of C9 and C10 he feels more confident and more self-directing.

b. As a consequence of C1 and C10, his values are determined by an *organismic valuing process*.

11. As a consequence of C1, and C2, he *perceives* others more realistically and accurately.

12. He *experiences* more *acceptance* of others, as a consequence of less need for distortion of his perceptions of them.

13. His behavior changes in various ways.

a. Since the proportion of *experience* assimilated into the *self-structure* is increased, the proportion of behaviors which can be "owned" as belonging to the *self* is increased.

- b. Conversely, the proportion of behaviors which are disowned as *self-experiences*, felt to be "not myself," is decreased.
- c. Hence his behavior is *perceived* as being more within his control.
- 14. His behavior is perceived by others as more socialized, more *mature*.
- 15. As a consequence of C1, 2, 3, his behavior is more creative, more uniquely adaptive to each new situation, and each new problem, more fully expressive of his own purposes and values.

Comment. The statement in part C which is essential is statement C1. Items 2 through 15 are actually a more explicit spelling out of the theoretical implications of statement 1. The only reason for including them is that though such implications follow readily enough from the logic of the theory, they are often not perceived unless they are pointed out.

Evidence. There is much confirmatory and some ambiguous or non-confirming evidence of the theoretical statement of outcomes. Grummon and John [28] find a decrease in defensiveness, basing judgements on the TAT. Hogan [36] and Haigh [29] also supply some scanty evidence on this point. As to the greater extensionality of perceptions (item 2), Jonietz [38] finds that therapy produces changes in perceptions and Mitchell [47] finds these changes to be in the direction of extensionality.

Item 4, stating that adjustment is improved, is supported by evidence based upon TAT, Rorschach, counselor rating, and other indexes, in the studies of Dymond [15, 16], Grummon and John [28], Haimowitz [30], Muench [49], Mosak [48], Cowen and Combs [13]. Carr [8], however, found no evidence of change in the Rorschach in nine cases.

Rudikoff [73] found that the self-ideal becomes more achievable, as stated in item 6. The increased congruence of self and ideal has been confirmed by Butler and Haigh [7], Hartley [33], and its significance for adjustment supported by Hanlon, Hofstaetter, and O'Connor (32).

The decrease in physiological tension over therapy is attested by the studies of Thetford [84] and Anderson [1]. The reduction in psychological tension as evidenced by the Discomfort-Relief Quotient has been confirmed by many investigators: Assum and Levy [4], Cofer and Chance [12], Kaufman and Raimy [39], N. Rogers [72], Zimmerman [86].

The increase in positive self-regard is well attested, as indicated in IB, Evidence. The shift in the locus of evaluation and choice is supported in the evidence provided by Raskin [56] and Sheerer [78]. Rudikoff [73] presents evidence which suggests that others may be perceived with greater realism. Sheerer [78] and Stock [82] and Rudikoff [73] show that others are perceived in a more acceptant fashion as postulated in item 11. Gordon and Cartwright [25] provide evidence which is

complex but in general nonconfirming on this point. M. Haimowitz [30] also has findings which seem to indicate that nonacceptance of minority groups may be more openly expressed.

The behavior changes specified in items 13 and 14 find support in the Rogers study [68] showing that in improved cases both the client and his friends observe greater maturity in his behavior. Hoffman [35] finds that the behavior the client describes in the interviews becomes more mature. Jonietz's study of [38] of perception of ink blots might lend some support to the postulate of item 15.

Comments on the theory of therapy. It is to be noted that this theory of therapy involves, basically, no intervening variables. The conditions of therapy, given in *A*, are all operationally definable, and some have already been given rather crude operational definitions in research already conducted. The theory states that if *A* exists, then *B* and *C* will follow. *B* and *C* are measurable events, predicted by *A*.

It should also be pointed out that the logic of the theory is such that: if *A*, then *B*; if *A*, then *B* and *C*; if *A*, then *C* (omitting consideration of *B*), if *B*, then *C* (omitting consideration of *A*).

Specification of functional relationships. At this point, the functional relationships can only be stated in general and qualitative form. The greater the degree of the conditions specified in *A*, the more marked or more extensive will be the process changes in *B*, and the greater or more extensive the outcome changes specified in *C*. Putting this in more general terms, the greater the degree of anxiety in the client, congruence in the therapist in the relationship, acceptance and empathy experienced by the therapist, and recognition by the client of these elements, the deeper will be the process of therapy, and the greater the extent of personality and behavioral change. To revert now to the theoretical logic, all we can say at present is that

$$B = (f)A \quad C = (f)A \quad B + C = (f)A \quad C = (f)B$$

Obviously there are many functional interrelationships not yet specified by the theory. For example, if anxiety is high, is congruence on the part of the therapist less necessary? There is much work to be done in investigating the functional relationships more fully.

D. Some Conclusions Regarding the Nature of the Individual

From the theory of therapy as stated above, certain conclusions are implicit regarding the nature of man. To make them explicit involves little more than looking at the same hypotheses from a somewhat different vantage point. It is well to state them explicitly, however, since they constitute an important explanatory link of a kind which gives this theory what-

ever uniqueness it may possess. They also constitute the impelling reason for developing a theory of personality. If the individual is what he is revealed to be in therapy, then what theory would account for such an individual?

We present these conclusions about the characteristics of the human organism:

1. The individual possesses the capacity *to experience in awareness* the factors in his *psychological maladjustment*, namely, the *incongruences* between his *self-concept* and the totality of his *experience*.

2. The individual possesses the capacity and has the tendency to reorganize his *self-concept* in such a way as to make it more *congruent* with the totality of his *experience*, thus moving himself away from a state of *psychological maladjustment*, and toward a state of *psychological adjustment*.

3. These capacities and this tendency, when latent rather than evident, will be released in any interpersonal *relationship* in which the other person is *congruent* in the *relationship*, experiences *unconditional positive regard* toward, and *empathic* understanding of the individual, and achieves some communication of these attitudes to the individual. (These are, of course, the characteristics already given under IA3, 4, 5, 6.)

It is this tendency which, in the following theory of personality, is elaborated into the tendency toward actualization.

I believe it is obvious that the basic capacity which is hypothesized is of very decided importance in its psychological and philosophical implications. It means that psychotherapy is the releasing of an already existing capacity in a potentially competent individual, not the expert manipulation of a more or less passive personality.² Philosophically it means that the individual has the capacity to guide, regulate, and control himself, providing only that certain definable conditions exist. Only in the absence of these conditions, and not in any basic sense, is it necessary to provide external control and regulation of the individual.

II. A THEORY OF PERSONALITY

In endeavoring to order our perceptions of the individual as he appears in therapy, a theory of the development of personality, and of the dynamics of behavior, has been constructed. It may be well to repeat the warning previously given, and to note that the initial propositions

²In order to correct a common misapprehension it should be stated that this tentative conclusion in regard to human capacity grew out of continuing work with clients in therapy. It was not an assumption or bias with which we started our therapeutic endeavors. A brief personal account of the way in which this conclusion was forced upon me is contained in an autobiographical paper [69].

of this theory are those which are furthest from the matrix of our experience and hence are most suspect. As one reads on, the propositions become steadily closer to the experience of therapy. As before, the defined terms and constructs are italicized, and are to be understood as previously defined.

A. Postulated Characteristics of the Human Infant

It is postulated that the individual, during the period of infancy, has at least these attributes.

1. He perceives his *experience* as reality. His *experience* is his reality.
- a. As a consequence he has greater potential *awareness* of what reality is for him than does anyone else, since no one else can completely assume his *internal frame of reference*.
2. He has an inherent tendency toward *actualizing* his organism.
3. He interacts with his reality in terms of his basic *actualizing* tendency. Thus his behavior is the goal-directed attempt of the organism to satisfy the experienced needs for *actualization* in the reality as *perceived*.
4. In this interaction he behaves as an organized whole, as a gestalt.
5. He engages in an *organismic valuing process*, valuing *experience* with reference to the *actualizing tendency* as a criterion. *Experiences* which are *perceived* as maintaining or enhancing the organism are valued positively. Those which are *perceived* as negating such maintenance or enhancement are valued negatively.
6. He behaves with adience toward positively valued *experiences* and with avoidance toward those negatively valued.

Comment. In this view as formally stated, the human infant is seen as having an inherent motivational system (which he shares in common with all living things) and a regulatory system (the valuing process) which by its "feedback" keeps the organism "on the beam" of satisfying his motivational needs. He lives in an environment which for theoretical purposes may be said to exist only in him, or to be of his own creation.

This last point seems difficult for some people to comprehend. It is the perception of the environment which constitutes the environment, regardless as to how this relates to some "real" reality which we may philosophically postulate. The infant may be picked up by a friendly, affectionate person. If his perception of the situation is that this is a strange and frightening experience, it is this perception, not the "reality" or the "stimulus" which will regulate his behavior. To be sure, the relationship with the environment is a transactional one, and if his continuing experience contradicts his initial perception, then in time his

perception will change. But the effective reality which influences behavior is at all times the perceived reality. We can operate theoretically from this base without having to resolve the difficult question of what "really" constitutes reality.

Another comment which may be in order is that no attempt has been made to supply a complete catalogue of the equipment with which the infant faces the world. Whether he possesses instincts, or an innate sucking reflex, or an innate need for affection, are interesting questions to pursue, but the answers seem peripheral rather than essential to a theory of personality.

B. The Development of the Self

1. In line with the tendency toward differentiation which is a part of the *actualizing tendency*, a portion of the individual's *experience* becomes differentiated and *symbolized* in an *awareness* of being, *awareness* of functioning. Such awareness may be described as *self-experience*.

2. This representation in *awareness* of being and functioning, becomes elaborated, through interaction with the environment, particularly the environment composed of significant others, into a *concept of self*, a perceptual object in his *experiential field*.

Comment. These are the logical first steps in the development of the self. It is by no means the way the construct developed in our own thinking, as has been indicated in the section of definitions. (A digression on the case history of a construct, p. 200.)

C. The Need for Positive Regard

1. As the awareness of self emerges, the individual develops a *need for positive regard*. This need is universal in human beings, and in the individual, is pervasive and persistent. Whether it is an inherent or learned need is irrelevant to the theory. Standal [80], who formulated the concept, regards it as the latter.

a. The satisfaction of this need is necessarily based upon inferences regarding the experiential field of another.

(1) Consequently it is often ambiguous.

b. It is associated with a very wide range of the individual's *experiences*.

c. It is reciprocal, in that when an individual discriminates himself as satisfying another's need for *positive regard*, he necessarily experiences satisfaction of his own need for *positive regard*.

(1) Hence it is rewarding both to satisfy this need in another, and to experience the satisfaction of one's own need by another.

- d. It is potent, in that the *positive regard* of any social other is communicated to the total *regard complex* which the individual associates with that social other.
- (1) Consequently the expression of positive regard by a significant social other can become more compelling than the *organismic valuing process*, and the individual becomes more adjient to the *positive regard* of such others than toward *experiences* which are of positive value in *actualizing* the organism.

D. *The Development of the Need for Self-regard*

1. The positive regard satisfactions or frustrations associated with any particular *self-experience* or group of *self-experiences* come to be *experienced* by the individual independently of *positive regard* transactions with social others. *Positive regard experienced* in this fashion is termed *self-regard*.

2. A *need for self-regard* develops as a learned need developing out of the association of *self-experiences* with the satisfaction or frustration of the need for positive regard.

3. The individual thus comes to *experience positive regard* or loss of *positive regard* independently of transactions with any social other. He becomes in a sense his own significant social other.

4. Like *positive regard*, *self-regard* which is *experienced* in relation to any particular *self-experience* or group of *self-experiences*, is communicated to the total *self-regard complex*.

E. *The Development of Conditions of Worth*

1. When *self-experiences* of the individual are discriminated by significant others as being more or less worthy of *positive regard*, then *self-regard* becomes similarly selective.

2. When a *self-experience* is avoided (or sought) solely because it is less (or more) worthy of *self-regard*, the individual is said to have acquired a *condition of worth*.

3. If an individual should *experience only unconditional positive regard*, then no *conditions of worth* would develop, *self-regard* would be unconditional, the needs for *positive regard* and *self-regard* would never be at variance with organismic evaluation, and the individual would continue to be *psychologically adjusted*, and would be fully functioning. This chain of events is hypothetically possible, and hence important theoretically, though it does not appear to occur in actuality.

Comment. This is an important sequence in personality development, stated more fully by Standal [80]. It may help to restate the sequence in informal, illustrative, and much less exact terms.

The infant learns to need love. Love is very satisfying, but to know whether he is receiving it or not he must observe his mother's face, gestures, and other ambiguous signs. He develops a total gestalt as to the way he is regarded by his mother and each new experience of love or rejection tends to alter the whole gestalt. Consequently each behavior on his mother's part such as a specific disapproval of a specific behavior tends to be experienced as disapproval in general. So important is this to the infant that he comes to be guided in his behavior not by the degree to which an experience maintains or enhances the organism, but by the likelihood of receiving maternal love.

Soon he learns to view himself in much the same way, liking or disliking himself as a total configuration. He tends, quite independently of his mother or others, to view himself and his behavior in the same way they have. This means that some behaviors are regarded positively which are not actually experienced organismically as satisfying. Other behaviors are regarded negatively which are not actually experienced as unsatisfying. It is when he behaves in accordance with these introjected values that he may be said to have acquired conditions of worth. He cannot regard himself positively, as having worth, unless he lives in terms of these conditions. He now reacts with adience or avoidance toward certain behaviors solely because of these introjected conditions of self-regard, quite without reference to the organismic consequences of these behaviors. This is what is meant by living in terms of introjected values (the phrase formerly used) or conditions of worth.

It is not theoretically necessary that such a sequence develop. If the infant always felt prized, if his own feelings were always accepted even though some behaviors were inhibited, then no conditions of worth would develop. This could at least theoretically be achieved if the parental attitude was genuinely of this sort: "I can understand how satisfying it feels to you to hit your baby brother (or to defecate when and where you please, or to destroy things) and I love you and am quite willing for you to have those feelings. But I am quite willing for me to have my feelings, too, and I feel very distressed when your brother is hurt, (or annoyed or sad at other behaviors) and so I do not let you hit him. Both your feelings and my feelings are important, and each of us can freely have his own." If the child were thus able to retain his own organismic evaluation of each experience, then his life would become a balancing of these satisfactions. Schematically he might feel, "I enjoy hitting baby brother. It feels good. I do not enjoy mother's distress. That feels dissatisfying to me. I enjoy pleasing her." Thus his behavior would sometimes involve the satisfaction of hitting his brother, sometimes the satisfaction of pleasing mother. But he would never have to disown the

feelings of satisfaction or dissatisfaction which he experienced in this differential way.

F. The Development of Incongruence between Self and Experience

1. Because of the need for *self-regard*, the individual *perceives* his *experience* selectively, in terms of the *conditions of worth* which have come to exist in him.

a. Experiences which are in accord with his *conditions of worth* are *perceived* and *symbolized* accurately in *awareness*.

b. Experiences which run contrary to the *conditions of worth* are *perceived* selectively and distortedly as if in accord with the *conditions of worth*, or are in part or whole, *denied to awareness*.

2. Consequently some experiences now occur in the organism which are not recognized as *self-experiences*, are not accurately *symbolized*, and are not organized into the *self-structure* in *accurately symbolized* form.

3. Thus from the time of the first selective *perception* in terms of *conditions of worth*, the states of *incongruence between self and experience*, of *psychological maladjustment* and of *vulnerability*, exist to some degree.

Comment. It is thus because of the distorted perceptions arising from the conditions of worth that the individual departs from the integration which characterizes his infant state. From this point on his concept of self includes distorted perceptions which do not accurately represent his experience, and his experience includes elements which are not included in the picture he has of himself. Thus he can no longer live as a unified whole person, but various part functions now become characteristic. Certain experiences tend to threaten the self. To maintain the self-structure defensive reactions are necessary. Behavior is regulated at times by the self and at times by those aspects of the organism's experience which are not included in the self. The personality is henceforth divided, with the tensions and inadequate functioning which accompany such lack of unity.

This, as we see it, is the basic estrangement in man. He has not been true to himself, to his own natural organismic valuing of experience, but for the sake of preserving the positive regard of others has now come to falsify some of the values he experiences and to perceive them only in terms based upon their value to others. Yet this has not been a conscious choice, but a natural—and tragic—development in infancy. The path of development toward psychological maturity, the path of therapy, is the undoing of this estrangement in man's functioning, the dissolving of conditions of worth, the achievement of a self which is congruent

with experience, and the restoration of a unified organismic valuing process as the regulator of behavior.

G. The Development of Discrepancies in Behavior

1. As a consequence of the incongruence between self and experience described in *F*, a similar incongruence arises in the behavior of the individual.

a. Some behaviors are consistent with the *self-concept* and maintain and actualize and enhance it.

(1) Such behaviors are *accurately symbolized in awareness*.

b. Some behaviors maintain, enhance, and actualize those aspects of the experience of the organism which are not assimilated into the *self-structure*.

(1) These behaviors are either unrecognized as *self-experiences* or perceived in distorted or selective fashion in such a way as to be congruent with the *self*.

selective perception

H. The Experience of Threat and the Process of Defense

1. As the organism continues to *experience*, an *experience* which is incongruent with the self-structure (and its incorporated *conditions of worth*) is *subceived as threatening*.

2. The essential nature of the *threat* is that if the *experience* were *accurately symbolized in awareness*, the *self-concept* would no longer be a consistent gestalt, the *conditions of worth* would be violated, and the *need for self-regard* would be frustrated. A state of *anxiety* would exist.

3. The process of *defense* is the reaction which prevents these events from occurring.

a. This process consists of the *selective perception* or *distortion* of the *experience* and/or the *denial to awareness* of the *experience* or some portion thereof, thus keeping the total *perception* of the *experience* consistent with the individual's *self-structure*, and consistent with his *conditions of worth*.

4. The general consequences of the process of *defense*, aside from its preservation of the above consistencies, are a rigidity of *perception*, due to the necessity of *distorting perceptions*, an *inaccurate perception* of reality, due to *distortion and omission of data*, and *intensionality*.

Comment. Section *G* describes the psychological basis for what are usually thought of as neurotic behaviors, and Section *H* describes the mechanisms of these behaviors. From our point of view it appears more fundamental to think of defensive behaviors (described in these two sections) and disorganized behaviors (described below). Thus the de-

fensive behaviors include not only the behaviors customarily regarded as neurotic—rationalization, compensation, fantasy, projection, compulsions, phobias, and the like—but also some of the behaviors customarily regarded as psychotic, notably paranoid behaviors and perhaps catatonic states. The disorganized category includes many of the “irrational” and “acute” psychotic behaviors, as will be explained below. This seems to be a more fundamental classification than those usually employed, and perhaps more fruitful in considering treatment. It also avoids any concept of neurosis and psychosis as entities in themselves, which we believe has been an unfortunate and misleading conception.

Let us consider for a moment the general range of the defensive behaviors from the simplest variety, common to all of us, to the more extreme and crippling varieties. Take first of all, rationalization. (“I didn’t really make that mistake. It was this way. . . .”) Such excuses involve a perception of behavior distorted in such a way as to make it congruent with our concept of self (as a person who doesn’t make mistakes). Fantasy is another example. (“I am a beautiful princess, and all the men adore me.”) Because the actual experience is threatening to the concept of self (as an adequate person, in this example), this experience is denied, and a new symbolic world is created which enhances the self, but completely avoids any recognition of the actual experience. Where the incongruent experience is a strong need, the organism actualizes itself by finding a way of expressing this need, but it is perceived in a way which is consistent with the self. Thus an individual whose self-concept involves no “bad” sexual thoughts may feel or express the thought “I am pure, but you are trying to make me think filthy thoughts.” This would be thought of as projection or as a paranoid idea. It involves the expression of the organism’s need for sexual satisfactions, but it is expressed in such a fashion that this need may be denied to awareness and the behavior perceived as consistent with the self. Such examples could be continued, but perhaps the point is clear that the incongruence between self and experience is handled by the distorted perception of experience or behavior, or by the denial of experience in awareness (behavior is rarely denied, though this is possible), or by some combination of distortion and denial.

1. The Process of Breakdown and Disorganization

Up to this point the theory of personality which has been formulated applies to every individual in a lesser or greater degree. In this and the following section certain processes are described which occur only when certain specified conditions are present.

1. If the individual has a large or significant degree of *incongruence* between self and experience and if a significant experience demonstrating

this *incongruence* occurs suddenly, or with a high degree of obviousness, then the organism's process of *defense* is unable to operate successfully.

2. As a result *anxiety* is *experienced* as the *incongruence* is subceived. The degree of *anxiety* is dependent upon the extent of the *self-structure* which is *threatened*.

3. The process of *defense* being unsuccessful, the *experience* is *accurately symbolized* in *awareness*, and the gestalt of the *self-structure* is broken by this *experience* of the *incongruence* in *awareness*. A state of disorganization results.

4. In such a state of disorganization the organism behaves at times in ways which are openly consistent with experiences which have hitherto been distorted or denied to awareness. At other times the self may temporarily regain regnancy, and the organism may behave in ways consistent with it. Thus in such a state of disorganization, the tension between the concept of self (with its included distorted perceptions) and the experiences which are not accurately symbolized or included in the concept of self, is expressed in a confused regnancy, first one and then the other supplying the "feedback" by which the organism regulates behavior.

Comment. This section, as will be evident from its less exact formulation, is new, tentative, and needs much more consideration. Its meaning can be illuminated by various examples.

Statements 1 and 2 above may be illustrated by anxiety-producing experiences in therapy, or by acute psychotic breakdowns. In the freedom of therapy, as the individual expresses more and more of himself, he finds himself on the verge of voicing a feeling which is obviously and undeniably true, but which is flatly contradictory to the conception of himself which he has held. [See 62, pp. 78-80, for a striking verbatim example of this experience.] Anxiety results, and if the situation is appropriate (as described under *J*) this anxiety is moderate, and the result is constructive. But if, through overzealous and effective interpretation by the therapist, or through some other means, the individual is brought face to face with more of his denied experiences than he can handle, disorganization ensues and a psychotic break occurs, as described in statement 3. We have known this to happen when an individual has sought "therapy" from several different sources simultaneously. It has also been illustrated by some of the early experience with sodium pentathol therapy. Under the drug the individual revealed many of the experiences which hitherto he had denied to himself, and which accounted for the incomprehensible elements in his behavior. Unwisely faced with the material in his normal state he could not deny its authenticity, his defensive processes could not deny or distort the experience, and hence the self-structure was broken, and a psychotic break occurred.

Acute psychotic behaviors appear often to be describable as behaviors which are consistent with the denied aspects of experience rather than consistent with the self. Thus the person who has kept sexual impulses rigidly under control, denying them as an aspect of self, may now make open sexual overtures to those with whom he is in contact. Many of the so-called irrational behaviors of psychosis are of this order.

Once the acute psychotic behaviors have been exhibited, a process of defense again sets in to protect the organism against the exceedingly painful awareness of incongruence. Here I would voice my opinion very tentatively as to this process of defense. In some instances perhaps the denied experiences are now regnant, and the organism defends itself against the awareness of the self. In other instances the self is again regnant, and behavior is consistent with it, but the self has been greatly altered. It is now a self concept which includes the important theme, "I am a crazy, inadequate, unreliable person who contains impulses and forces beyond my control." Thus it is a self in which little or no confidence is felt.

It is hoped that this portion of the theory may be further elaborated and refined and made more testable in the future.

J. The Process of Reintegration

In the situations described under sections *G* and *H*, (and probably in situations of breakdown as described under *I*, though there is less evidence on this) a process of reintegration is possible, a process which moves in the direction of increasing the congruence between *self* and *experience*. This may be described as follows:

1. In order for the process of *defense* to be reversed—for a customarily *threatening experience* to be *accurately symbolized* in *awareness* and assimilated into the *self-structure*, certain conditions must exist.
 - a. There must be a decrease in the *conditions of worth*.
 - b. There must be an increase in *unconditional self-regard*.
2. The communicated *unconditional positive regard* of a significant other is one way of achieving these conditions.
 - a. In order for the *unconditional positive regard* to be communicated, it must exist in a context of *empathic* understanding.
 - b. When the individual *perceives* such *unconditional positive regard*, existing *conditions of worth* are weakened or dissolved.
 - c. Another consequence is the increase in his own *unconditional positive self-regard*.
 - d. Conditions 2a and 2b above thus being met, *threat* is reduced, the process of *defense* is reversed, and *experiences* customarily *threatening* are *accurately symbolized* and integrated into the *self concept*.

3. The consequences of 1 and 2 above are that the individual is less likely to encounter *threatening experiences*; the process of *defense* is less frequent and its consequences reduced; *self* and *experience* are more *congruent*; *self-regard* is increased; *positive regard* for others is increased; *psychological adjustment* is increased; the *organismic valuing process* becomes increasingly the basis of regulating behavior; the individual becomes nearly fully functioning.

Comment. This section is simply the theory of therapy which we presented earlier, now stated in a slightly more general form. It is intended to emphasize the fact that the reintegration or restoration of personality occurs always and only (at least so we are hypothesizing) in the presence of certain definable conditions. These are essentially the same whether we are speaking of formal psychotherapy continued over a considerable period, in which rather drastic personality changes may occur, or whether we are speaking of the minor constructive changes which may be brought about by contact with an understanding friend or family member.

One other brief comment may be made about item 2a, above. Empathic understanding is always necessary if unconditional positive regard is to be fully communicated. If I know little or nothing of you, and experience an unconditional positive regard for you, this means little because further knowledge of you may reveal aspects which I cannot so regard. But if I know you thoroughly, knowing and empathically understanding a wide variety of your feelings and behaviors, and still experience an unconditional positive regard, this is very meaningful. It comes close to being fully known and fully accepted.

Specification of Functional Relationships in the Theory of Personality

In a fully developed theory it would be possible to specify, with mathematical accuracy, the functional relationships between the several variables. It is a measure of the immaturity of personality theory that only the most general description can be given of these functional relationships. We are not yet in a position to write any equations. Some of the relationships implied in section II may be specified as follows:

The more actualizing the experience, the more adient the behavior (A5, 6).

The more numerous or extensive the conditions of worth, the greater the proportion of experience which is potentially threatening (F1, 2).

The more numerous or extensive the conditions of worth, the greater the degree of vulnerability and psychological maladjustment (F3).

The greater the proportion of experience which is potentially threatening, the greater the probability of behaviors which maintain and en-

hance the organism without being recognized as self-experiences (*G1a, b*).

The more congruence between self and experience, the more accurate will be the symbolizations in awareness (*G1a*, and *H1, 2, 3*).

The more numerous or extensive the conditions of worth, the more marked will be the rigidity and inaccuracies of perception, and the greater the degree of intensionality (*H4*).

The greater the degree of incongruence experienced in awareness, the greater the likelihood and degree of disorganization (*I3*).

The greater the degree of experienced unconditional positive regard from another, based upon empathic understanding, the more marked will be the dissolution of conditions of worth, and the greater the proportion of incongruence which will be eliminated (*J2, 3*).

In other respects the relationships in section *J* have already been specified in the theory of therapy.

Evidence. The first sections of this theory are largely made up of logical constructs, and propositions which are only partly open to empirical proof or disproof.

Section *F* receives some confirmation from Cartwright [9], and Diller [14], Section *H* from Chodorkoff [10] and Cartwright [9], whereas Goldiamond [22] introduces evidence which might modify the definition of subception. Section *J* is supported by the evidence previously given for the theory of therapy in Part I.

Because it is a closely reasoned and significant experimental testing of certain of the hypotheses and functional relationships specified in this portion of the theory, Chodorkoff's study [10] will be described briefly. His definitions were taken directly from the theory. Defensiveness, for example, is defined as the process by which accurate symbolizations of threatening experiences are prevented from reaching awareness.

He concentrated on three hypotheses which may be stated in theoretical terms as follows:

1. The greater the congruence between self and experience, the less will be the degree of perceptual defensiveness exhibited.
2. The greater the congruence between self and experience, the more adequate will be the personality adjustment of the individual, as this phrase is commonly understood.
3. The more adequate the personality adjustment of the individual (as commonly understood), the less will be the degree of perceptual defensiveness exhibited.

Thus it will be seen that he was testing one of the definitions of the theory (Congruence equals psychological adjustment) against clinical and common-sense reality. He was also testing one of the relationships specified by the theory (Degree of congruence is inversely related to de-

gree of defensiveness). For good measure he also completes the triangle by testing the proposition that adjustment as commonly understood is inversely related to degree of defensiveness.

He gave the following operational meanings to the essential terms:

1. Self is defined as a *Q* sort of self-referent items sorted by the individual to represent himself as of now.

2. Experience. An exact matching of the theoretical meaning with given operations is of course difficult. Chodorkoff avoids the term "experience," but operationally defines it by an "objective description" which is a *Q* sort by a clinician of the same self-referent items, this sorting being based on a thorough clinical knowledge of the individual, gained through several projective tests. Thus the total experiencing of the individual, as distinct from the self-concept he possesses in awareness, is given a crude operational definition by this means.

3. Perceptual defensiveness is defined as the difference in recognition time between a group of neutral words tachistoscopically presented to the individual, and a group of personally threatening words similarly presented. (The selection of the words and the technique of presentation were very carefully worked out, but details would be too lengthy here.)

4. Personal adjustment as commonly understood was defined as a combined rating of the individual by four competent judges, the rating being based on biographical material, projective tests, and other information.

These definitions provide an operational basis for four measures entirely independent of one another.

Chodorkoff translates his hypotheses into operational predictions as follows:

1. The higher the correlation between the individual's self-sort and the clinician's sorting for his total personality, the less will be the difference in his recognition threshold between neutral and threatening words.

2. The higher the correlation between the self-sort and the clinician's sorting for the total personality the higher will be the rating of personal adjustment by the four judges.

3. The higher the adjustment rating by the four judges, the lower will be the difference in recognition threshold between neutral and threatening words.

All three of these predictions were empirically upheld at levels of statistical significance, thus confirming certain portions of the theory.

This study illustrates the way in which several of the theoretical constructs have been given a partial operational definition. It also shows how propositions taken or deduced from the theory may be empirically tested. It suggests, too, the complex and remote behavioral predictions which may be made from the theory.

III. A THEORY OF THE FULLY FUNCTIONING PERSON

Certain directional tendencies in the individual (*ID* and *IIA2*) and certain needs (*IIC*, *D*) have been explicitly postulated in the theory thus far presented. Since these tendencies operate more fully under certain defined conditions, there is already implicit in what has been given a concept of the ultimate in the actualization of the human organism. This ultimate hypothetical person would be synonymous with "the goal of social evolution," "the end point of optimal psychotherapy," etc. We have chosen to term this individual the fully functioning person.

Although it contains nothing not already stated earlier under I and II, it seems worthwhile to spell out this theoretical concept in its own right.

A. The individual has an inherent tendency toward *actualizing* his organism.

B. The individual has the capacity and tendency to *symbolize experiences* accurately in *awareness*.

1. A corollary statement is that he has the capacity and tendency to keep his *self-concept* congruent with his *experience*.

C. The individual has a *need for positive regard*.

D. The individual has a *need for positive self-regard*.

E. Tendencies *A* and *B* are most fully realized when needs *C* and *D* are met. More specifically, tendencies *A* and *B* tend to be most fully realized when

1. The individual *experiences unconditional positive regard* from significant others.

2. The pervasiveness of this *unconditional positive regard* is made evident through relationships marked by a complete and communicated *empathic* understanding of the individual's *frame of reference*.

F. If the conditions under *E* are met to a maximum degree, the individual who experiences these conditions will be a fully functioning person. The fully functioning person will have at least these characteristics:

1. He will be *open to his experience*.

a. The corollary statement is that he will exhibit no *defensiveness*.

2. Hence all *experiences* will be *available to awareness*.

3. All *symbolizations* will be as accurate as the experiential data will permit.

4. His *self-structure* will be congruent with his *experience*.

5. His *self-structure* will be a fluid gestalt, changing flexibly in the process of assimilation of new *experience*.

6. He will *experience* himself as the *locus of evaluation*.

a. The *valuing process* will be a continuing *organismic one*.

7. He will have no *conditions of worth*.

a. The corollary statement is that he will *experience unconditional self-regard*.

8. He will meet each situation with behavior which is a unique and creative adaptation to the newness of that moment.

9. He will find his *organismic valuing* a trustworthy guide to the most satisfying behaviors, because

a. All available experiential data will be available to *awareness* and used.

b. No datum of *experience* will be *distorted in, or denied to, awareness*.

c. The outcomes of behavior in *experience* will be *available to awareness*.

d. Hence any failure to achieve the maximum possible satisfaction, because of lack of data, will be corrected by this effective reality testing.

10. He will live with others in the maximum possible harmony, because of the rewarding character of reciprocal *positive regard* (*IIC1c*).

Comment. It should be evident that the term "the fully functioning person" is synonymous with optimal psychological adjustment, optimal psychological maturity, complete congruence, complete openness to experience, complete extensionality, as these terms have been defined.

Since some of these terms sound somewhat static, as though such a person "had arrived," it should be pointed out that all the characteristics of such a person are *process* characteristics. The fully functioning person would be a person-in-process, a person continually changing. Thus his specific behaviors cannot in any way be described in advance. The only statement which can be made is that the behaviors would be adequately adaptive to each new situation, and that the person would be continually in a process of further self-actualization. For a more complete exposition of this whole line of thought the reader may wish to see my paper on the fully functioning person [64].

Specification of Functions. Our present state of thinking can be given in one sentence. The more complete or more extensive the conditions *E1, E2*, the more closely will the individual approach the asymptotic characteristics *F1* through *F10*.

Evidence. The evidence regarding outcomes of therapy is in a general way confirmatory of the direction taken in this theory, though by its very nature it can never be completely tested, since it attempts to define an asymptote.

IV. A THEORY OF INTERPERSONAL RELATIONSHIP

The most recent extension of our theoretical constructs has been the attempt to formulate the order which appears to exist in all interpersonal relationships and interpersonal communication. This formulation

springs, as will be evident, primarily from the theory of therapy, viewing the therapeutic relationship as simply one instance of interpersonal relationship. For clarity of presentation the conditions, process, and outcome of a deteriorating relationship and a deepening or improving relationship will be set forth separately. Actually these are two points or spaces on a continuum.

A. The Conditions of a Deteriorating Relationship

For communication to be reduced, and for a relationship to deteriorate, the following conditions are necessary:

1. A person Y is willing to be in *contact* with person X and to receive communication from him. (Note: Y's characteristics do not need to be specified, beyond saying that he is an "average person," with some *maladjustment*, some *incongruence*, some *defensiveness*. The theory is stated largely in terms of person X.)

2. Person X desires (at least to a minimal degree) to communicate to and be in *contact* with Y.

3. Marked *incongruence* exists in X among the three following elements:

- a. His *experience* of the subject of communication with Y. (Which may be the relationship itself, or any other subject.)
- b. The *symbolization* of this *experience* in his *awareness*, in its relation to his *self-concept*.
- c. His *conscious* communicated expression (verbal and/or motor) of this *experience*.

Comment. If the discrepancy in 3 is *a* vs. *b*, *c*, then X is psychologically maladjusted in this respect, and the immediate consequences of the condition tend to be personal. If the discrepancy is *a*, *b*, vs. *c*, then the state tends to be labeled deceit, and the immediate consequences tend to be social.

The extreme of this incongruence, and hence one end point of the continuum, would be a complete or almost complete incongruence or dissociation between the experience, its cognitive meaning (symbolization), and its expression.

B. The Process of a Deteriorating Relationship

When the preceding conditions exist and continue, a process is initiated which tends to have these characteristics and directions:

1. The communications of X to Y is contradictory and/or ambiguous, containing

- a. Expressive behaviors which are consistent with X's *awareness* of the *experience* to be communicated.

- b. Expressive behaviors which are consistent with those aspects of the experience not accurately symbolized in X's *awareness*. (See IIG above.)
2. Y *experiences* these contradictions and ambiguities.
 - a. He tends to be *aware* only of B1a, that is X's conscious communication.³
 - b. Hence his *experience* of X's communication tends to be incongruent with his *awareness* of same.
 - c. Hence his response tends also to be contradictory and/or ambiguous, his responses having the same qualities described for X in B1a, b.
3. Since X is *vulnerable*, he tends to *perceive* Y's responses as potentially *threatening*.
 - a. Hence he tends to *perceive* them in *distorted* fashion, in ways which are *congruent* with his own *self-structure*.
 - b. Hence he is inaccurate in his *perception* of Y's *internal frame of reference*, and does not *experience* a high degree of *empathy*.
 - c. Because Y is *perceived* as a potential *threat*, X cannot and does not *experience unconditional positive regard* for Y. (Note: thus X provides the reverse of the conditions for therapy as described in I43, 4, 5.)
 4. Y *experiences* himself as receiving at most a selective *positive regard*.
 5. Y *experiences* a lack of understanding or *empathy*.
 6. The more Y *experiences* a selectiveness of *positive regard* and an absence of *empathy*, the less free he is to express *feelings*, the less likely he is to express self-referent feelings, the less likely he is to be *extensional* in his *perceptions*, the less likely he is to express incongruencies between *self* and *experience*, the less likely he is to reorganize his *self-concept*. (Note: in general, the process of personality changes as described in IB is reversed.)
 7. Since Y is expressing less of his *feelings*, X is even more unlikely to *perceive* Y's *internal frame of reference* with accuracy, and both inaccuracy of *perception* and *distortion of perception* make *defensive* reactions on X's part more likely.
 8. Another characteristic which may exist, particularly if X's communication is primarily of negative feelings, is that those aspects of *experience* which are not accurately *symbolized* by X in his *awareness* tend, by *defensive distortion of perception*, to be *perceived* in Y.
 9. If this occurs, Y tends to be *threatened* to the degree that these relate to his own *incongruences*, and to exhibit *defensive* behaviors.

³This is a crucial point. If Y is sufficiently *open to his experience* that he is *aware* of X's other communication—described in B1b—then b and c below do not follow, and his own response to X is clear and *congruent*. If in addition to his *awareness* of all of X's communication he experiences an *unconditional positive regard* for X, then this would become an improving relationship, as described in sections D, E, and F which follow.

C. The Outcome of a Deteriorating Relationship

The continuance of this process results in

1. Increased *defensiveness* on the part of X and Y.
2. Communication which is increasingly superficial, expressive of less of the total individual.
3. The *perceptions* of *self* and others, because of the increased *defensiveness*, are organized more tightly.
4. Hence *incongruence* of *self* and *expression* remains in *status quo*, or is increased.
5. *Psychological maladjustment* is to some degree facilitated in both.
6. The relationship is *experienced* as poor.

Comment on A, B, C. It may clarify this technical and theoretical description of a deteriorating relationship to illustrate it from some commonplace experience. Let us, for example, take the relationship of a mother, X, toward her child, Y. There is, of course, mutual willingness to be in psychological contact. The mother feels "You annoy me because you interfere with my career," but she cannot be aware of this because this experience is incongruent with her concept of herself as a good mother. Her perception of this experience in herself is distorted, becoming "I am annoyed at this instance of your behavior. I love you but I must punish you." This is an acceptable symbolization of her experience, and it is this which she consciously communicates to the child.

But Y receives not only this conscious communication. He also experiences (but tends to be unaware of) the expressive behaviors indicating a more general dislike of himself. His response may be of several sorts, but its essential characteristic is that it will express the incongruence which her divided communication has set up in him. One possibility is that he will experience himself as bad and unloved, even when his awareness of his behavior is that he is "good." Hence he will act and feel guilty and bad, even when behaving in an approved manner. This type of response is threatening to the mother, because his behaviors expressing badness and unlovedness threaten to bring into awareness her own rejecting feelings. Consequently she must further distort her perception of his behavior, which now seems to her "sneaky" or "hang-dog" as well as being occasionally annoying. The more this cycle continues, the less acceptance Y feels, the less adequately he can express his feelings, the more difficult it is for his mother to achieve any empathic understanding, the more completely the two are estranged in the relationship, the more maladjusted each becomes. It is the exact steps in such a relationship which we have endeavored to describe in the three foregoing sections—the conditions which bring it about, the process

by which deterioration takes place, and the outcomes of such a deteriorated relationship.

D. The Conditions of an Improving Relationship

For communication to increase, and the relationship to improve, the following conditions are necessary:

1. A person, Y', is willing to be in *contact* with person X', and to receive communication from him.
2. Person X' desires to communicate to and be in *contact* with Y'.
3. A high degree of *congruence* exists in X' between the three following elements:
 - a. His *experience* of the subject of communication with Y'.
 - b. The *symbolization* of this *experience* in *awareness* in its relation to his *self-concept*.
 - c. His communicative expression of this *experience*.

E. The Process of an Improving Relationship

1. The communication of X' to Y' is characterized by congruence of *experience, awareness, and communication*.

2. Y' *experiences* this *congruence* as clear communication. Hence his response is more likely to express a *congruence* of his own *experience* and awareness.

3. Since X' is *congruent* and not *vulnerable* in the area related to his communication, he is able to *perceive* the response of Y' in an accurate and *extensional* manner, *with empathy* for his *internal frame of reference*.

4. Feeling understood, Y' *experiences* some satisfaction of his *need for positive regard*.

5. X' *experiences* himself as having made a positive difference in the *experiential field* of Y'.

a. Hence reciprocally, X' tends to increase in *feeling of positive regard* for Y'.

b. Since X' is not *vulnerable* in the area of the communication, the *positive regard* he feels for Y' tends to be an *unconditional positive regard*.

6. Y' *experiences* himself in a *relationship* which, at least in the area of communication, is characterized by congruence on the part of X', an *empathic* understanding by X' of the *internal frame of reference*, and an *unconditional regard*. (See IA3, 4, 5.)

a. Hence all the characteristics of the process of therapy (IB) are initiated, within the confines of the subject of communication.

b. Because Y' has less need of any of his *defenses* in this *relationship*, any need for *distortion of perception* is decreased.

c. Hence he *perceives* the communications of X' more accurately.

7. Hence communication in both directions becomes increasingly *congruent*, is increasingly accurately *perceived*, and contains more reciprocal *positive regard*.

F. *Outcomes of an Improving Relationship*

The continuance of this process results in the following:

1. All of the outcomes of therapy (IC1 through 15) may occur, subject to the time limitation of the relationship between X' and Y', and also to the mutually understood limitations of the area of the relationship (e.g., it may be mutually understood that it is only a lawyer-client relationship, or only a teacher-pupil relationship, thus tending to exclude many areas of expression and hence to that degree limiting the extent of the outcomes). Thus, within these limitations, the relationship facilitates improved congruence and psychological adjustment in both X' and Y'.

G. *A Tentative Law of Interpersonal Relationships*

Taking all of this section, we may attempt to compress it into one overall law governing interpersonal relationships, specifying the functional relationship between the constructs. Here is such an attempt.

Assuming a minimal mutual willingness to be in *contact* and to receive communications, we may say that the greater the communicated *congruence* of *experience*, *awareness*, and behavior on the part of one individual, the more the ensuing relationship will involve a tendency toward reciprocal communication with the same qualities, mutually accurate understanding of the communications, improved *psychological adjustment* and functioning in both parties, and mutual satisfaction in the relationship.

Conversely, the greater the communicated *incongruence* of *experience*, *awareness*, and behavior, the more the ensuing relationship will involve further communication with the same quality, disintegration of accurate understanding, lessened *psychological adjustment* in both parties, and mutual dissatisfaction in the relationship.

Comment. This is still a theory in the making, rather than a finished product. It does not grow out of consideration of research data and grows only partly out of experience. Basically, it is deduced from the theory of therapy and projects into a new area a series of hypotheses which now require confirmation or disproof. The evidence gained in such studies should not only modify or confirm the theory of interpersonal relationships but should reflexively throw new light on the theory of therapy as well.

Evidence. It is believed that there is evidence from experience and some research evidence concerning this theory. It seems preferable, however, simply to present it as a deduced theory.

V. THEORIES OF APPLICATION

To spell out in detail the various theories of application which have been partially developed, would be too repetitious of what has gone before. Hence only a descriptive suggestion will be given in each area of the aspects of theory which would be applicable.

Family life. The theoretical implications would include these:

1. The greater the degree of *unconditional positive regard* which the parent experiences toward the child:

a. The fewer the *conditions of worth* in the child.

b. The more the child will be able to live in terms of a continuing *organismic valuing process*.

c. The higher the level of *psychological adjustment* of the child.

2. The parent experiences such *unconditional positive regard* only to the extent that he *experiences unconditional self-regard*.

3. To the extent that he *experiences unconditional self-regard*, the parent will be congruent in the relationship.

a. This implies genuineness or congruence in the expression of his own *feelings* (positive or negative).

4. To the extent that conditions 1, 2, and 3 exist, the parent will realistically and *empathically* understand the child's *internal frame of reference* and *experience an unconditional positive regard* for him.

5. To the extent that conditions 1 through 4 exist, the theory of the process and outcomes of therapy (*IB, C*), and the theory of the process and outcomes of an improving relationship (*IVE, F*), apply.

Comment. Stated thus briefly, the applications to family life may easily be misunderstood. For a presentation of these and related ideas, the reader is referred to [65].

Education and learning. To the extent that education is concerned with learnings which significantly influence behavior and facilitate change in personality, then the conditions of therapy (*IA*) and the conditions of an improving relationship (*IVD*) apply. This leads, among other things, to more realistic, accurate, and differentiated perceptions (*IC1, 2*) and to more responsible basing of behavior upon these perceptions (*IC3, 10, 15*).

Comment. Since a reasonably full statement of the theory of facilitating learning has already been set forth [62, chap. 9], no attempt will be made to spell it out in detail here, even though a number of the terms and constructs in this earlier presentation are not precisely those which are used here.

Evidence. Several studies of the application of this theory to the educational process have been made. Gross [26], Schwebel and Asch [74], Asch [3], and Faw [17, 18], supply evidence which in general is confirmatory.

Group leadership. Building upon the postulate regarding the nature of the individual (*ID*) and extending this to apply to groups, it has been hypothesized that to the extent that a perceived leader provides the conditions of therapy (*IA3*, 4, 5) or of an improving relationship (*IVD*), certain phenomena will occur in the group. Among these are the following: the perceptual resources of the group will be more widely used, more differentiated data will be provided by the group, thinking and perceptions will become more extensional, self-responsible thinking and action will increase, a greater degree of distributive leadership will develop, and there will be more effective long-range problem solving. All of these consequences flow logically from the theory thus far presented.

In two major expositions [24, 23], Gordon has set forth carefully the theory of application in this field, and it will not be repeated here. The reader is referred to these presentations for more detail.

Evidence. The studies by Roethlisberger and Dickson [57], Coch and French [11], Radke and Klisurich [53], Gordon, and others supply some confirmatory evidence of different aspects of the theory.

Group tension and conflict. In serious situations of group conflict, the conditions of a deteriorating interpersonal relationship (*IVA*) usually exist. Drawing both from the theory of therapy and the theory of interpersonal relationships, certain hypotheses have been formulated in regard to such situations. Since these introduce a somewhat new point, they will be formulated in more detail.

For our present purpose we may assume as given a group situation in which the conditions of a deteriorating relationship (*IVA*) already exist, with defensive behaviors and expressions being mutually increased between X and Y and Z, different members of the group, or between different subgroups represented by X, Y, and Z.

A. Conditions of Reduction in Group Conflict

Group conflict and tension will be reduced if these conditions exist.

1. A person (whom we term a facilitator) is in *contact* with X, Y, and Z.
2. The facilitator is *congruent* within himself in his separate *contacts* with X, Y, and Z.
3. The facilitator experiences toward X, Y, and Z, separately:
 - a. An *unconditional positive regard*, at least in the area in which the members of the group are communicating.

- b. An *empathic* understanding of the *internal frame of reference* of X, Y, Z, at least in the area in which the members of the group are communicating.
4. X, Y, and Z *perceive*, at least to a minimal degree, conditions 3a and 3b. (This is generally because 3b is communicated verbally.)

B. The Process of Reduction of Group Conflict

If the above conditions exist and continue, then:

1. The various elements of the process of therapy (IB) take place to some degree, at least within the area involved in the group communication.
 - a. One of the important elements of this process is the increase in differentiated *perceptions* and in *extensionality*.
 - b. Another important element is the reduction of *threat* (see IB8, 8a) in the experience of X, Y, Z.
2. Consequently the communications of Y to X or Z to X, are less *defensive*, and more nearly *congruent* with the *experience* of Y, and with the *experience* of Z.
3. These communications are perceived with increasing accuracy and *extensionality* by X.
 - a. Consequently X *experiences* more *empathic* understanding of Y and Z.
4. Because he is *experiencing* less *threat* from Y and Z and more *empathy* with their *internal frame of reference*:
 - a. X now symbolizes in awareness *incongruencies* which formerly existed between *experience* and *awareness*.
 - b. Consequently his *defensive* distortions of his own *experience* are reduced.
 - c. Hence his communication to Y and Z becomes a more *extensional* expression of his own total *experience* in regard to the area of communication.
5. The conditions now exist for the process of an improving relationship, and the phenomena described in IVE occur.

Comment. A more general statement of the views presented here theoretically will be found in two previous papers [63, 61]. This theory is a deduction from the theory of therapy, and the theory of interpersonal relationships.

Evidence. Although clinical evidence tends to confirm the theory in small face-to-face groups, and Axline [5] has given an account of such a clinical situation, there is as yet, I believe, no research evidence bearing on this aspect of the theory. Particularly crucial and important from a social point of view will be investigations involving different sizes of groups. Even if the theory is fully confirmed in small face-to-face groups, will it hold true in larger groups where communication is not face-to-

face? There is also a question involving groups composed of spokesmen, or representatives, where the individual feels that he cannot speak out of his own experience and feeling, but only in a way dictated by his constituents, who are not present. It is quite clear that the theory, as formulated here, would not directly apply to this last type of situation.

THE THEORETICAL SYSTEM IN A CONTEXT OF RESEARCH

Our presentation of the theoretical system is completed. It is to be hoped that the presentation has made it clear that this is a developing system, in which some of the older portions are being formulated with considerable logical rigor, while newer portions are more informal, and contain some logical and systematic gaps and flaws, and still others (not presented) exist as highly personal and subjective hunches in the minds of members of the client-centered group. It is also to be hoped that it is evident that this is a system which is in a continual state of modification and clarification. Comparison of the theory as given above with the theory of therapy and personality given in *Client-centered Therapy* in 1951 [62, chaps. 4, 11] or with the paper presented to the APA in 1947 [60] will show that although the major directions have not markedly changed, there have been many changes in the constructs employed, and far-reaching changes in the organization of the theory. This ongoing process of revision is expected to continue.

The major usefulness of the systematic theoretical thinking, aside from the personal satisfaction it has given, has been the stimulation of research. In this respect there seems little doubt that it has had considerable success. By and large the order of events seems to have been this—clinical therapeutic experience, formulation of theory, research which tests the theory, new aspects of experience perceived because of the research, modification of the theory in the light of the new experience and the research, further empirical testing of the revised hypotheses.

It would take too much space to review or even list the studies which have been made. This would also be an unnecessary duplication since Seeman and Raskin [77] have written a thoughtful analysis and criticism of 55 of the research studies in therapy and personality which have been stimulated by this point of view and completed during the years 1942–1951.⁴ Suffice it to say that clusters of research investigations have been made around each of the following subjects of inquiry:

1. The events and process of therapy. Analysis of recorded therapeutic interviews in terms of theoretical constructs has been a major tool here.

⁴ Since writing the above D. S. Cartwright has published: Annotated bibliography of research and theory construction in client-centered therapy, *J. counsel. Psychol.*, 1957, 4, 82–100.

2. The results or outcomes of therapy. Tests of personality and measures of different aspects of behavior have been the major instrumentation.

3. Investigation of personality theory. Hypotheses regarding perception of self, others, external reality, and perceived locus of evaluation have been investigated with a wide range of instruments.

4. Application of theory in specific fields. Investigations particularly in the facilitation of learning and in group leadership.

Since 1951, many more studies have been completed in the outcomes of therapy, an important collection of these being gathered in *Psychotherapy and Personality Change* [70]. In these studies the problem of a control group is much more adequately handled than heretofore, giving the findings a solidity which is noteworthy. If the reader wishes to obtain a first-hand grasp of the way in which refinements of instrumentation and general scientific sophistication have developed in this field, he should compare the seven studies of therapeutic outcome published in the *Journal of Consulting Psychology* in 1949 (the entire July issue, pp. 149-220) with the thirteen studies published in *Psychotherapy and Personality Change* (1954).

In addition to the many studies of outcome there are an increasing number which have as their primary purpose the investigation of empirical predictions made from personality theory. The study of Chodoroff [10], already cited, is an excellent example of this group. There are also studies now in progress which draw their hypotheses from an integration of the theory of therapy with a theory of perception or a theory of learning. Such studies will, it is hoped, link the findings in the field of therapy to the findings in older and more established fields of psychology.

The bases of stimulation of research. There are, in the writer's opinion, several basic reasons why this theoretical system has been helpful in giving impetus to a wide variety of research investigations.

The first is the orienting attitude mentioned in the first section of this document, that scientific study can begin anywhere, at any level of crudity or refinement, that it is a direction, not a fixed degree of instrumentation. From this point of view, a recorded interview is a small beginning in scientific endeavor, because it involves greater objectification than the memory of an interview; a crude conceptualization of therapy and crude instruments for measuring these concepts, are more scientific than no such attempts. Thus individual research workers have felt that they could begin to move in a scientific direction in the areas of greatest interest to them. Out of this attitude has come a series of instruments of increasing refinement for analyzing interview protocols, and significant beginnings have been made in measuring such seemingly intangible con-

structs as the self-concept and the psychological climate of a therapeutic relationship.

This leads me to what I believe to be the second major reason for the degree of success the theory has had in encouraging research. The constructs of the theory have, for the most part, been kept to those which can be given operational definition. This has seemed to meet a very pressing need for psychologists and others who have wished to advance knowledge in the field of personality but who have been handicapped by theoretical constructs which cannot be defined operationally. Take, for example, the general phenomena encompassed in such terms as the self, the ego, the person. If a construct is developed—as has been done—which includes those inner events not in the awareness of the individual as well as those in awareness, then there is no satisfactory way at the present time to give such a construct an operational definition. But by limiting the self-concept to events in awareness, the construct can be given increasingly refined operational definition through the Q technique, the analysis of interview protocols, etc., and thus a whole area of investigation is thrown open. In time the resulting studies may make it possible to give operational definition to the cluster of events not in awareness.

The use of operationally definable constructs has had one other effect. It has made completely unnecessary the use of "success" and "failure"—two terms which have no scientific usefulness—as criteria in studies of therapy. Predictions can instead be made in terms of operationally definable constructs, and these predictions can be confirmed or disconfirmed, quite separately from any value judgments as to whether the change represents "success" or "failure." Thus one of the major barriers to scientific advance in this area has been removed.

A third and final reason for whatever effectiveness the system has had in mediating research is that the constructs have generality. Because psychotherapy is such a microcosm of significant interpersonal relationship, significant learning, and significant change in perception and in personality, the constructs developed to order the field have a high degree of pervasiveness. Such constructs as the self-concept, or the need for positive regard, or the conditions of personality change, all have application to a wide variety of human activities. Hence such constructs may be used to study areas as widely variant as industrial or military leadership, personality change in psychotic individuals, the psychological climate of a family or a classroom, or the interrelation of psychological and physiological change.

The problem of measurement and quantification. I do not feel competent to discuss, at a sophisticated level of statistical knowledge, the problems of measurement which have been met by our group. This is

best left to others. I will only mention three examples of the continuing trend toward ever more refined quantification of the data of psychotherapy and personality.

The researches which have taken their start from client-centered theory have significantly advanced the field of analysis of verbal protocols. Working with recorded interviews, increasingly exact methods have been devised, so that reliability of categorization is high, and very subtle constructs, such as, for example, an "emergent self-perception" can be objectified and measured. The attempt has been made by Grummon [27] to integrate some of the methods we have developed with the more formal methods of language analysis.

Other research workers have taken the Q technique as developed by Stephenson [81], and have exploited it in a variety of ways. It has been used to give an operational definition to the self-concept, to provide objectifications of a diagnostician's perception of an individual immediately comparable to that individual's self-perception, to measure the quality of a relationship as perceived by the two participants, and to test a variety of hypotheses growing from personality theory.

Butler [6] has developed a new method for discovering the order which exists in such material as interview protocols. A number of people working with him have begun to apply this method—termed Rank Pattern Analysis—to problems of complex analysis which hitherto had been baffling.

Thus in a number of different areas the researches stimulated by client-centered theory have not only contributed to the empirical base of the theory, but have contributed to the development of methodology as well. In principle there seems no limit to the refinement of measurement in the areas covered by the theory. The major obstacle to progress has been the lack of sufficient inventiveness to develop tools of measurement adequate for the tasks set by the theory.

Incompatible evidence. Some of the evidence related to the theory has been cited in each section. It will have been noted that nearly all of this evidence has been confirmatory and that which is not confirming has tended to be confused. There is almost no research evidence which appears flatly to contradict the predictions from the theory.

Two related exceptions are the study reported by Carr [8], and a portion of the study made by Grummon and John [28, also 37] which is discussed by Vargas [85]. Briefly, the facts seem to be that Carr and John had pre- and posttherapy projective tests analyzed by psychologists who were basically diagnosticians. They found little or no change in the degree of adjustment, in the projective material. In a series of 10 cases, the John ratings as discussed by Vargas had a significant negative correlation with counselor ratings. Yet when these

same materials are analyzed "blind" by therapeutically oriented researchers (for example, Dymond) positive change is found, and the correlation with counselor ratings is significantly positive.

The explanation suggested by Vargas is that the diagnostician tends to think of adjustment as stability, a more or less fixed "level of defense" which is socially acceptable. The therapeutically oriented worker—especially if influenced by client-centered theory—tends to think of psychological adjustment as an openness to experience, a more fluid expressiveness and adaptiveness. Hence what the diagnostician perceives as loss of control or even disorganization may be perceived by the therapeutically oriented person as progress toward reduced defensiveness and greater openness to experience. How deep this contradiction goes, and its full implications, can only be evaluated in the light of further research.

The main source of incompatible evidence is not research evidence, but a clinical point of view. By and large the psychoanalytically oriented Freudian group has developed, out of its rich clinical experience, a point of view which is almost diametrically opposed to the hypotheses regarding the capacities and tendencies of the human organism formulated above in *D1*, 2, 3, and also diametrically opposed to the theory of the fully functioning person in *III*. Very briefly stated, the Freudian group, on the basis of its experience, tends to see the individual as "innately destructive" (to use Karl Menninger's words) and hence in need of control. To members of this group the hypothetical individual pictured earlier under *A Theory of the Fully Functioning Person* is a psychopathic personality, because they see nothing that would control him. The hypothesis that self-control would be natural to the person who is without defenses appears to them untenable.

In very much related fashion, the theory which Gordon and others have formulated regarding group behavior and group leadership is almost diametrically opposed to the Freudian theory in this respect. Freud's statements that "groups have never thirsted after truth" and that "a group is an obedient herd which could never live without a master" suggests something of the deep discrepancy which exists between the two views.

Though the psychoanalytic theory in these two respects is not supported by any research evidence, it nevertheless deserves serious consideration because of the soil of clinical experience out of which it originally grew. The discrepancy seems even more puzzling and challenging when it is realized that both the Freudian group and the client-centered group have developed their theories out of the deep and intimate personal relationships of psychotherapy.

It is my belief that the discrepancy can be understood in a way

which leaves the client-centered theory intact, but this does not seem to be the place for such a discussion. It seems best to present these incompatible views for what they are, two theoretical stands which are in flat contradiction on some basic points. Only new integrations of theory and much deeper research investigations can resolve the difference.

A continuing program of theory and research. The theoretical system and the research program which are connected with client-centered therapy have grown from within themselves. This point can hardly be overemphasized. The thought that we were making a start on a theoretical system would for me have been a most distasteful notion even as little as a dozen years ago. I was a practical clinician and held (*horrible dictu!*) an open scorn of all psychological theory, as my early students at Ohio State can testify. This was true even at the same time that I was beginning to discern the orderliness which existed in the therapeutic process. I like to think that the theoretical system and far-reaching web of research which have developed, have grown in an organic fashion. Each plodding step has simply been a desire to find out this, a desire to find out that, a need for perceiving whatever consistencies, or invariances, or order exists in the material thus far unearthed.

Consequently when I am asked, as I am in the outline suggested for this paper, "the extent to which the systematic program has been realized," I feel it is the wrong question for this system. I have no idea what will be the ultimate realization of the living program which has developed. I can see some of the likely next steps, or the current directions, but have no assurance that these will be taken. We have continued to move in the directions which are *experienced* as rewarding, not necessarily in those directions which logic points out. I believe this has been the strength of the program, and I trust it will continue.

Thus I believe that we are likely to see progress in the following directions, but I am not sure of any of them. It seems likely that further moves will be made toward theory and research in the field of perception, enriching that field by the insights gained in therapy, and being enriched by the wealth of research data and theory in perception which can be brought to bear in the refinement of the theories we are developing. One such study now in progress, for example, is attempting to investigate perceptual changes which occur during therapy. The measures range from those entirely concerned with social perception—of people, of relationships—to those entirely concerned with the physical perception of form, color, and line. Does therapy change only social perception, or does it alter even the most basic perceptual processes? If not, where on this continuum does change cease to occur?

I visualize the same type of *rapprochement* with learning theory,

where in my judgment we have much to offer in the way of new directions in that field, as well as being able to use much of the material available there. It also seems likely that a number of the hypotheses we are formulating may be tested in the laboratory, some on human and some on animal subjects, thus linking the field of personality and therapy with so-called experimental psychology. There seems no reason, for example, why research on the establishment and consequences of conditions of worth, as spelled out in this theory, might not be carried out on higher animals, with a wider range of experimental conditions and more adequate controls than could be achieved with human subjects.

I regard it as possible that there may be a closer linking of our theory with the developing interest in creativity in the humanities and social sciences generally, and I trust that this theory may provide a number of relevant hypotheses for testing. I regard it as very likely that the implications of this body of theory for industrial production will be developed much more fully—the beginnings, as described by Richard in Gordon's book [23], seem very exciting. I believe it is possible that the near future may see a clear linking with the psychiatric group and a testing of the theory in a wider variety of human disorders, with a reduction in the professional parochialism which has thus far kept the medical group largely ignorant of the research in this field.

One direction which appears only theoretically possible is the exploitation in governmental affairs and international relations of some of the implications of this theory. I do not regard this as likely in the near future.

I suspect that the discovery and development of a contextual basis for this theory in some form of existential philosophy will continue. The general orientation of philosophical phenomenology is also likely to continue to have its influence in this respect. These are some of the potentialities for future development—rather grandiose, to be sure—which I see. The extent to which any of them will organically grow is a matter which demands a gift of prophecy I do not have.

Immediate strategy of development. To return, in closing, to the much more immediate issues facing us in the systematic development of the theory, I see several problems which have very high priority if our general systematic thinking is to have a healthy development. I will list these problems and tasks, but the order of listing has no significance, since I cannot determine the priority.

1. We are urgently in need of new and more ingenious tools of measurement. Stephenson's Q technique [81] has been most helpful and Osgood's method for quantifying semantic space [51] also seems promising. But most urgently needed of all is a method whereby we might give operational definition to the construct *experience* in our theory, so

that discrepancies between self-concept and experience, awareness and experience, etc., might be measured. This would permit the testing of some of the most crucial hypotheses of the theoretical system. To be sure, some attempts have been made to approach such an operational definition, but the instrumentation is exceedingly cumbersome and admittedly inadequate.

2. An increased amount of experience with individuals classed as psychotic, and the testing of a variety of the theoretical hypotheses in therapeutic work with this group and in research with psychotics as subjects, would round out and enrich our systematic thinking in an area in which it is at present inadequate. It would provide the type of extreme reality test which is most helpful in the confirmation, modification, or disproof of a theoretical system. There would seem to be no barriers except practical ones to such a development.

3. An increased amount of experience and careful studies of hypotheses developed from the theory are needed in the area of group relationships. Hypotheses regarding leadership, facilitation of learning, and reduction of social conflict seem particularly fruitful to study. Here again, the test of the theory at one of its deduced extremes would be most helpful in confirming or revising its core.

4. Still another urgent need—no doubt quite evident to readers of this presentation—is the translation of the present theory into terms which meet the rigorous requirements of the logic of science. Although progress in this direction has been made there is still a woefully long distance to go. Such a development, carried through by competent persons, would greatly sharpen the deductive hypotheses which might be drawn from the system, and hence provide more crucial tests of it.

5. The final need I wish to mention may seem to some very contradictory to the one just voiced. Personally I see it as a possible evolutionary step, not as a contradictory one. I see a great need for creative thinking and theorizing in regard to the methods of social science. There is a rather widespread feeling in our group that the logical positivism in which we were professionally reared is not necessarily the final philosophical word in an area in which the phenomenon of subjectivity plays such a vital and central part. Have we evolved the optimal method for approximating the truth in this area? Is there some view, possibly developing out of an existentialist orientation, which might preserve the values of logical positivism and the scientific advances which it has helped to foster and yet find more room for the existing subjective person who is at the heart and base even of our system of science? This is a highly speculative dream of an intangible goal, but I believe that many of us have a readiness to respond to the person or persons who can evolve a tentative answer to the riddle.

CONCLUSION

I find myself somewhat appalled at the length and scope of the material which has been presented. I suspect the reader shares this feeling. I can only say, somewhat apologetically, that I had not fully recognized the ramifying pervasiveness of our theoretical thinking until I endeavored to bring it all under one verbal roof. If many of the outlying structures appear to the reader flimsy or unfit for occupancy, I hope that he will find the central foundation, the theory of therapy, more solid. If to some degree this formulation bestirs individuals to more activity in research designed to prove or disprove these hypotheses, or to more activity in building a better, more rigorous, more integrated theory, then the group which is collectively responsible for the foregoing theories will be fully satisfied.

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PERSONALITY THEORY GROWING FROM MULTIVARIATE QUANTITATIVE RESEARCH

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DEFINITION OF THE APPROACH

The maturity of theoretical developments may be tested by two touchstones. First, a scientific system is generally more mature when its concepts arise from specially developed operations and techniques other than those available to everyday observation and to the layman. Secondly, theory is more mature if we can point to ensuing predictive and controlling powers which are real enough to have led to potent technologies, recognizable in specially developed social institutions.

By these touchstones, "personality theory" ranges more widely in developmental level than do most other areas of psychological theory, presenting examples from rarefied heights and from degraded depths of scientific acceptability and status. On the one hand, there is voluminous theory—principally in the clinical area—based on no better methods of

observation than have been available for centuries, entangled in verbal stereotypes that are almost certainly false or purely local in reference, intuitive in observation, inexplicit as to assumptions, and in general, not precisely, operationally based or confirmable. From this level of scientific poverty it rises, on the other hand, to rational, objective, quantitative, and intricately developed concepts which can truly be said to surpass, in both complexity of testable theory and effectiveness of technological results, such neighboring fields as, say, learning theory and group dynamics.

The present essay is concerned exclusively with the kind of personality theory which has developed out of quantitative and objective methods, whether that is based upon clinical, abnormal data, upon social or educational fields of observation, or upon laboratory and physiological study of the normal individual. It is also demarcated from neighboring developments by emphasis on multivariate analytic experiment rather than on manipulative univariate experiment. This distinction will be drawn more clearly in a moment; at the outset, let it be said that in *intention* researchers in the present area are aiming at an *experimentally based personality theory* and that the emphasis which has developed on multivariate rather than on the traditional, controlled, univariate experimental method is considered only an intelligent strategic adaptation to the needs of personality investigation at its present stage. It is contended that much effort has been relatively wasted in unimaginative application of classical experimental design by psychologists of impeccable scientific aspirations, who have failed to perceive that in psychology (as distinct from the physical sciences) we encounter a situation and kind of data to which classical design is not the best approach. In particular, the new multivariate experimenter contends that classical univariate experiment has insufficiently realized: (1) that in psychology (compared with physics), special steps must be taken to isolate organically unitary and unique behavior structures, i.e., "significant variables," before univariate experiment can be strategically applied, (2) that where so many variables exist (even if restricted to "significant" ones) the multivariate approach is far more economical and powerful in mapping those systematic relations among variables from perception of which the better-adapted hypotheses and models will arise.

This contrast with univariate experiment is not the whole story regarding the character of the multivariate experimental approach considered in this essay. Indeed, in later stages, it has lost some of the characters which initially distinguished it from the univariate experimental tradition; notably, it has begun to manipulate variables (thus introducing dependent-independent variable concepts), although in a framework of simultaneous operation with many variables. But it has also gathered new characters of its own through growing into fresh branches of highly

technical specialized experiment and conceptualization which have no counterparts in univariate experiment.

In conformity with the plan suggested for the present contributions on systematic viewpoints we shall begin with background factors and orienting attitudes. Fortunately, in the present case these are so well known that only a very brief sketch need be given. The main historical *roots* of the method lie directly in the study of *individual differences*, though in the last decade it has emancipated itself completely from this restriction and has been largely concerned with lifting the study of structure and process to new technical levels. In social science generally, the multivariate method began along with statistical method, or at least, with the second covariational-analysis phase of the development of statistics by Galton and Karl Pearson. In psychology, it grew to a lusty adolescence in the study of individual differences in ability and school achievement. This growth began with Spearman's and Burt's attempts fifty years ago to place intelligence testing on a firm basis of theory and continued through Thurstone's development of multifactor analysis. In some isolated backwaters of academic teaching, the multivariate approach is still seen in these terms of individual differences, of nonmanipulative experiment, of the merely economical objective of finding convenient "dimensions," and of restriction largely to educational and cognitive psychological problems.

Actually, multivariate methods, of which factor analysis remains the chief development, now handle far more issues than this and have as much to contribute in personality, learning, and motivation study as in the field of abilities. They offer as much in general experimental design as in the psychometric study of individual differences. It is a truism of scientific history that classifications which appear early are rarely those which are ultimately realized through the logical, inherent characters of the methods concerned. The approach defined here may be seen historically as beginning with the structural and taxonomic problems of classifying abilities. Yet in terms of its inherent, logical nature and the real applications indicated for the future, multivariate analysis must be seen as one of the two main experimental methods available in science generally. Incidentally it may be a matter of justifiable pride to psychologists that although multivariate analytical methods are being used, *crescendo*, in physiology, medicine, meteorology, and sociology, they were largely developed within psychology (as the univariate methods were within the older physical sciences).

Before proceeding to bring out more explicitly the procedures and assumptions of the multivariate approach, we may help the reader toward perspective by giving some indication of substantive content, and also of the relation of this contribution to others in the same series.

The theories developed here are flanked, on the motivational side, by Miller's development of conflict theory (vol. 2) and Rapaport's account of psychoanalytic structural and dynamic concepts. The factor analytic account of unitary drives needs to be aligned with Morgan's physiological picture (vol. 1). Our discussion of factors of temperament and the methodology of psychological genetics should be brought into relation with studies of human heredity in various fields (e.g., Kallmann). Our general structural theory of personality has affinities to and differences from the stimulus-response formulations of Hull and Spence (vol. 2) and the more clinical analysis by Murray. On the social side, our mathematical models for attitudes and for roles can be related to the contributions of Katz and Stotland and of Newcomb, respectively.

As these relationships are studied, it will become evident that the present approach is not so much concerned with a theory, i.e., with a particular set of constructs and concepts in, for example, personality and motivation, as with *many* possible theories, all dependent upon the resolving power of a particular methodological approach. Its unity is not that of adherence to conceptual beliefs but of the natural integration which exists in findings from a particular method and model, flexibly applied and checked against other methods where possible. Nevertheless, we admit a certain attachment to the theories *per se*, and certainly we concentrate on the theories to the extent that this essay is not concerned with *all* derivatives of the multivariate method, e.g., in group dynamics, culture-pattern psychology, physiology, but with those developed in personality and motivation. When concentrating on the theories, however, it is important to distinguish them from superficially similar notions, often with similar names (e.g., Freud's notion of ego strength or McDougall's concept of the self-regarding sentiment), which do not arise from this mathematical model or bear the hallmark of statistical precision in measurement which the present concepts always imply.

To orient the reader from this point on, the author should state that despite his intention to follow the excellent editorial outline suggested for all contributions, he has been unable entirely to adapt the present systematic material to the rubrics indicated. Thus, after the above statement of background the outline proceeds to the structure of the system, particularly the systematic independent, intervening, and dependent variables. This sequence is ill-adapted to the present case because initially there are no dependent and independent variables. At least in the factor analytic method as used by Spearman, Burt, and Thurstone *all* the individual difference measurement variables stand on an equal footing.

Accordingly, it has seemed best to follow an order of exposition which most clearly develops an understanding of the dependence of ideas upon

procedures (some of which will probably not be initially known to the reader) and then, in a final section, explicitly to summarize our position in terms of the issues raised by the outline.

PERSONALITY RESEARCH IN RELATION TO THE TWO BASIC SCIENTIFIC METHODS

After attempting to demarcate the special character and intention of this particular contribution to personality theory, we now examine more closely its chief instrument—multivariate analytical experiment. Since the terms univariate and multivariate may not be understood in the same sense by all, a brief comparative analysis, of these and other allegedly distinct methods used in personality research, is necessary before empirical findings, and the psychological concepts ensuing, can be properly focused.

Actually, it is common to hear *three* methods mentioned in personality research: the clinical method, the controlled experimental method, and the multivariate analytic method—besides special emphases and approaches cutting across these, such as the anthropological, the physiological, etc. In the controlled, manipulative classical or univariate experimental method, the independent variable is manipulated, or allowed to alter, while all other variables are considered to be controlled, except for the changes in the single dependent variable, which are recorded. (Hence univariate, for occasionally the *independent* variable is multiplied to two or three, as in the Fisherian factorial design.) Except in a purely positivistic theoretical framework, the *empirical* independent variable is understood by the “classical” experimenter to represent a *systematic* independent variable—a concept or construct which he has postulated to be so represented. But, for the moment, we shall set aside what the experimenter *thinks* he is doing—for this can be differently conceived—and ask only what distinguishes univariate and multivariate experiment in terms of what the experimenter actually does.

Before proceeding, we must deny the third approach, the “clinical method,” any status as a fundamentally distinct method. The only logically possible treatments of relations among variables are in pairs and sequentially, as in univariate experiment, or in large numbers and, usually, without knowledge of sequence, as in multivariate experiment. The clinician is generally a multivariate experimenter, who abstracts laws and concepts from observing (“globally” or by “gestalts” as he might say) simultaneous changes in a large number of uncontrolled variables. Fundamentally he does exactly—or perhaps we should say inexactly—what the factor analyst or multivariate experimenter does, but he does it without the benefit of precise instrumental measurement or explicit correlational procedures (or other mathematical treatment of functional

relations). His intuitions about functional unities are thus approximations to the analyst's independent factors, and his statements about mutual influences of factors are made without benefit of an F or t test. The "clinical method" does exist and function usefully as a rough "reconnaissance" form of the basic multivariate experimental method. Claims that it is anything other than this confuse the clinical method as a subdivision of *therapy*, which it undoubtedly is, with an independent *scientific* method, which it undoubtedly is not. For such claims give merely local skills and methodological accretions grown up around clinical practice the status properly due only to a fundamental difference of design.

One cannot avoid the judgment that the valuable contribution of clinical practice as an exploratory method has lately been more than offset by its tendency to choke the growth of sound, checkable personality theory in a rank weedy jungle of facile verbal concepts. Where quantitative and computational checks are not possible—or, at least, are avoided by the formulation of "theories"—the theoretical field becomes a mere playground for persons of high fluency. If we apply the test of maturity of theory suggested above—the production of an effective technology—then the few existing examinations, notably by Kelley and Fiske [67], Meehl [76], and Eysenck [50], showing that clinical psychology is indistinguishably above chance in diagnosis or therapy, leave us no conclusion but that purely clinically derived theory is in a bad way.

Ironically, however, when clinicians or others have tried to put their house in order and to extract the true metal of science from the ores in which clinical data are richer than laboratory data, they have reverted to classical instead of that multivariate experiment which is intrinsic to "clinical method" and the potential source of its greatest contribution. This failure is rooted partly in education—the rarity of coordination of mathematical and clinical training—and perhaps partly in temperament. Lack of foresighted handling of the clinical research training programs in this respect is likely to be responsible for our knowing in 1970 laws which we might have known and applied in 1960. For, before controlled experiment can go to work on the relation of, say, superego strength to early family attitudes, or the changes of "free" and "bound" anxieties under treatment by ataractic drugs, multivariate research must first substantiate the existence of a unitary factor of superego strength, show tests which measure it with defined concept validity and reliability, and discover whether anxiety in fact falls into one, or two, or more, independent sets of manifestations.

The emphasis in the present contribution on theory derived from multivariate rather than univariate quantitative research is, in summary,

justified both by the historical situation and by the intrinsic logic of method, as follows:

First, by the purely sociohistorical fact mentioned above, that the method has been subject to gross and untimely neglect in relation to realistically evaluated potential contribution. It has been neglected because, at least in Germany and America, those interested in objective scientific research in psychology have largely been conservatively trained in the half-truth that in psychology, as in physics, science consists of controlled experiment. The clinicians who had the courage to break away from this tradition realized that the more important emotional situations could not be used in controlled experiments with man. A rigid adherence to laboratory experiment would lead to the restriction of data to such specialized but "trivial" fields as perception, the psychophysiology of reflexes, or the sense organs, or to experiments on the emotions of animals, which could never be applied, except by uncertain analogy, to the personalities of human beings.

Secondly, at this primitive stage of personality research especially, the multivariate method offers a swifter and surer approach to the significant variables for controlled experimentation. In personality, as in psychology and the life sciences as a whole, the investigator has an *infinite* array of variables from which to choose. It is not surprising—and is perhaps a comment on our ways of striving for originality—that one and the same empirical (not conceptual) variable rarely gets confirmatory investigation by as many as two psychologists. Apparently, there are at least as many variables claimed to be of outstanding significance as there are psychologists.

One of the common schemata underlying presentations in this book is the statement of independent, intervening, and dependent variables in each field. It has been editorially suggested that an independent (or dependent) variable should be further considered in experimental, mathematical, and ideational (systematic) senses. This initial clarification is best adapted to the univariate methodology from which it was derived; in the multivariate field it needs further structuring. A factor is both a systematic, *conceptual* independent (or dependent) variable and an intermediate variable. The strict multivariate methodologist is unlikely, indeed, to introduce any conceptual intermediate variable that is not first revealed as a unitary factor. However, the proof that a unitary entity *exists*, and that it is therefore profitable to begin setting up hypotheses about it, *as* a unitary concept, may occur years ahead of the confirmation of *what* the entity is.

The contention of the multivariate analyst is that too many psychologists have immaturely "jumped the gun" by imitating the univari-

ate experiment of physics in psychology, without regard to the different stages and *natures* of these sciences. Greater shrewdness might have foretold the impotence which these incontinent procedures have demonstrated during more than half a century. It would have—and now emphatically has—indicated that a better strategy is to reduce the chaos of infinite possible variables to more tractable and significant numbers and natures by factor analysis before much hypothesis formation and manipulative experiment begins. A decade devoted largely to systematic, cooperative studies of this kind would not now be out of place in personality study, or indeed in learning, physiological, and social psychology.

The third and last relation to be emphasized between multivariate and univariate methods justifies greater resort to the former not only because of aptness to the present developmental phase, but generally. This is the argument from *research economy and certainty of inference*. It springs from three sources:

1. One multivariate research with, say, 30 variables yields evidence on $(30 \times 29)/2 = 435$ relationships, with only fifteen times the experimental work required in one univariate experiment. Consequently it achieves the results of 435 univariate experiments with about one-thirtieth of the expenditure.

2. The relationships are determined under conditions in which all variables are allowed to vary over their full range together. Consequently one does not have the uncertainty, which occurs in trying to make inferences from many univariate experiments, as to possible interaction effects lost through the controlled situation or as to corrections necessary in integration because the different univariate relations have been found on diverse samples.

3. The hypothesis being tested is made more determinate through being represented by a factor measurement based on several empirical independent variables instead of one only. For example, an investigator may set out to test the hypothesis that rigidity is related to rate of conditioning and state that operation X defines operationally his hypothesis or concept of the nature of rigidity, i.e., X is the empirical independent variable defining his systematic independent variable, rigidity. But factor analysis might show, as it frequently has, that only one-third of the variance of X is accounted for by a rigidity factor and that the rest is equally determined by two other factors, say, intelligence and fatigue. It may take several blind conceptual-trial-and-error studies before the univariate experimenter hits on a better variable to represent rigidity (as judged by more consistent or positive results), and during that time his conclusions could just as well be statements about relations of conditioning to intelligence or fatigue as to rigidity. A factorist would first determine the factor structure and then tie the factor down by several

operational representatives. For it is rare to find a complex concept that can be represented by a single operation, and it is still more rare for a univariate experimenter to land on it at the first attempt.

Against these substantial advantages two shortcomings can be charged to the multivariate method, (1) that it only handles linear relationships, (2) that it omits time sequence and therefore does not permit unambiguous causal inference. (More narrowly stated, it deals with response-response relations rather than stimulus-response.) The first is true, but it is generally desirable to observe any relation in the approximate, linear form before proceeding to more complex functions. How many relations do we yet know of in psychology involving a law that is indubitably different from one of simple proportion? And are not most controlled experiments content with an analysis of variance significance test, proving nothing at all about the *form* of the relation? As to the second, it rests largely on lack of reading in multivariate methods. The condition-response factor design [23, 42] systematically investigates the relation of controlled changes of stimulus to response; P technique [35] and incremental R technique use factor analysis over time intervals rather than in instantaneous, nonsequential analysis.

These later, more developed multivariate designs permit causal inference about interaction of factors to be drawn from the same experiment as that which structures the variables into factors, as will be seen in examples in the following sections. They retain, however, the advantage that manipulative control of most variables is not necessary, as it typically has to be in most univariate experiment. Instead of "isolating by control," the multivariate experiment allows nature to vary as it will (often producing effects we should not dare to duplicate in human experiment) and then isolates by superior statistical analysis what cannot be isolated by physical manipulation. For example, one might be interested in the effects of Group Morale Factor 2 [41] upon individual responses expressed in murder rates. Fortunately we are spared responsibility for the latter because we do not know how to manipulate factor 2, but we can accurately measure its changes and investigate the relations accordingly. The wider realm of multivariate experimental design can be read about elsewhere [23, 28, 58, 90], so we shall now confine ourselves to the relevant essentials of the factor analytic model.

THE LOGIC OF FACTOR ANALYTIC EXPERIMENT

We need not deal with the mathematics and the computational procedures of factor analysis here [see 23, 58, 90], but its logic should be briefly stated. Any of the standard factor analytic procedures will reduce the variance on a large member n of individual variables to variance on

a small number of common factors k plus variance on n specific factors. Thereafter the score P of a person i on a specific variable j can be estimated by the specification equation:

$$P_{ji} = s_{j1}F_{1i} + s_{j2}F_{2i} + \dots + s_{jk}F_{ki} + s_jF_{j_i}$$

where the s 's are the situational indices or loadings, obtained by factoring the correlation matrix for the n variables, and the F 's are the strengths of the endowments of the individual i in the various factors. Factors 1 to n are factors common to this and other performances, whereas factor j is specific to this particular response. The factor matrix, obtained by factor analytic procedures from the correlation matrix, gives us all we need for the above general equation. Each row of the matrix gives the set of s 's for estimation of the given variable, and each column, presenting a factor, shows which variables need to have their weighted scores added together to give an estimate of that factor for any individual.

It will be noted in passing that this formulation again transcends, or requires a new view of, the reduction of scientific systems to independent and dependent variables and intermediate variables or constructs. For the initial variables are (at least in timeless, instantaneous factor analysis) both the independent variables from which the construct—the factor—is inferred, *and* the dependent variables predicted from these intermediate variables, in the specification equation.

The majority of factor analytic researches are not carried out with the object of proceeding to actual specification equation computations but rather with the general scientific aim of determining the number and nature of the psychological factors at work in a given phenomenal area. At this level, issues have been much confused by difference of purpose between mathematical statisticians and psychological researchers. The mathematical statisticians are content if they can find a reduced number of orthogonal factors which will reproduce the correlations, and if possible also the given scores, within the given experiment. But psychologists are concerned to know that they have found *the* correct number of factors and that they have *the* correct nature (pattern) for each factor, in terms of other experiments beyond the one in question, i.e., in terms of general scientific concepts. Consequently psychologists do not see advantage in the mathematical neatness of orthogonality; they positively reject it, because it is highly probable that all factors in the same universe have interaction and are likely to be somewhat correlated among themselves.

The mathematician knows many—indeed an infinite number—of combinations of numbers and natures of factors that will reproduce the

given variable correlations and scores, and if he prefers any one, it will be for mathematical neatness. The psychologist wants conditions for determining a *unique* solution, i.e., a fixed number of factors rotated to *one* fixed position, and he is more concerned that this unique interpretation fit the interpretation of other experimental matrices than that it fit certain concepts of mere mathematical convenience within one matrix. The pursuit of this latter aim is tied up technically with development of (1) communality estimation theories, (2) the invention of formulas for standard errors for factors and loadings, and (3) the determination of unique rotation positions by simple structure, criterion rotation, and parallel profiles [1, 7, 23, 29, 31, 44, 81, 90, 95]. In these, statistical logic has had at times to limp along with the help of a crutch derived from empirical generalizations; but as of 1958, the major problems have been overcome just sufficiently, though not always to the satisfaction of the theoretical statistician. That is to say, researchers will in general now agree on how many factors there are, and the "payoff" of arranging findings from many studies side by side shows that simple structure is capable of revealing the *same* factor patterns from different, independent experimental studies. An essential part of this completion of adequate techniques has been the development of factor-matching indices, such as the recent formula by Cattell and Baggaley [31], which permit us to give fiducial limits to the goodness of a given matching of factors from one study to another. With improved techniques for obtaining unique resolution into factors, and improved methods of checking factors from study to study, it has been possible to demonstrate the invariance of 10 to 20 personality and ability factors.

The logic of the resolution of variance on many variables into a set of unique common factors, specifics, and error, is the same for all factor analytic designs, regardless of experimental setting. But the uses of factor analysis in different contexts of stimulus, response, and organism, and the scientific meaning of the factors derived therefrom, fall basically into six distinct experimental designs—actually a set of three basic designs, each analyzable in two different ways [22, 23]. The three basic designs arise from the nature of behavioral measurement. *Any behavioral measurement is defined and tagged by five referents: a particular organism making the response, a particular stimulus situation in which the response occurs, a particular moment in time, a particular point in space, and a particular observer* [22]. Setting aside the two last as irrelevant to the basic designs, we have three characteristics, any one of which can be repeated many times to create the series of entries required for correlation purposes. Thus we can have the same stimulus situation and class of response, measured at the same moment in time on a series of different organisms of the same class. This is the traditional correlation

procedure, e.g., measuring a set of schoolboys on their response to an intelligence test and then on a mathematics test and correlating the two series. When carried to a factor analysis it is called R technique. Secondly, we may correlate over a series of occasions (moments in time) instead of a series of persons, taking again and again the same set of stimulus-response (test situation) measures upon one person. This is called P technique. The three basic designs, or experimental possibilities of cor-

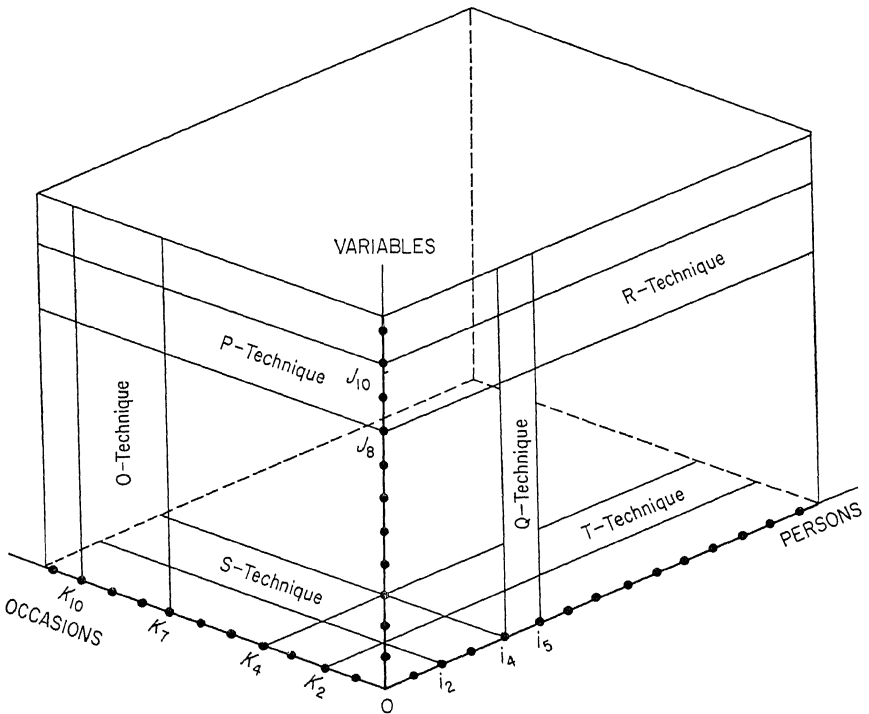


FIG. 1. The covariation chart. From [14].

relation, are called P, R, and T techniques and are shown, with their transposes, in the *Covariation Chart* in Fig. 1.

The meaning of the various possibilities in this chart the reader may work out for himself, or consult fuller accounts [14]. Until the generalized statement of covariation possibilities was published [14] in 1946, about 99 per cent of all correlation and factoring had been R technique and the rest Q and P techniques. Since 1946, most of the theoretical possibilities have been tried in practice and there has been much wider use of P and Q techniques. It will be observed that each of the three basic designs, R, P, and T, permits a transposed factoring of the same score matrix, namely, Q, O, and S, respectively. It is now

generally accepted that the same factors are obtainable from any design and its transpose, so that the decision to use one or the other of the pair depends upon convenience. For example, with many subjects and few tests, R technique is appropriate; the converse suggests the Q-technique transpose.

Furthermore, the three basic designs and their transposes may be modified further to produce several other useful designs. For example, *incremental R technique*, instead of factoring an absolute score for each person, can enter correlation with the *difference* of score for each person between stimulus occasions one and two [28, 102]. Moreover, as suggested above, although factor analysis grew up using naturally occurring, not experimentally created variation, nothing intrinsic to multivariate design prevents its also being used with varying stimuli as well as with varying responses. What has been called the *condition response design* [23], which randomizes several controlled, varying stimulus conditions with respect to one another, is one example of such use. Essentially, it factors stimuli and responses together, obtaining at once the unitary patterns of response and their relations to stimulus conditions.

No thorough treatment of the varied possible factor analytic experimental designs is possible here. Our objective in glancing over them is merely to point out that, as conceived by the psychologist, the factor, or "source trait" [14], differs from a mere mathematical factor not only by reappearing in several distinct R-technique studies, as already mentioned, *but also by its capacity to reappear as the same pattern in these different experimental designs*. For example, a factor labeled Surgency-and-Desurgency has been found in R-technique analysis, in terms of individual differences, loading such manifest variables as cheerful, impulsive, talkative, unworried, and some physiological variables, notably serum cholinesterase concentration. When the same variables are measured from day to day on a single individual and their trends are intercorrelated, the factor analysis produces an intraindividual pattern of just the same form. That is to say, Surgency-Desurgency is a unity in terms of individual differences and also in terms of function fluctuation within one person.

Finally, it should be recognized that a more generalized factor analytic model does not preclude nonparametric variables or functional relations of factors with variables more complex than those of simple linearity, as shown by Coombs and Satter [43] and discussed more fully elsewhere [28]. Until the modified models are developed in terms of computational analysis it will still be necessary to *find* the factors by the present linear approximation, operating, therefore, over the small variance ranges where the approximation better holds. But once the source traits are recognized and measurable, the nonlinear formulas better ex-

pressing their relation to any particular criterion can be more accurately determined in the usual curve-fitting way.

As shown elsewhere [28], approaches in terms of patterns and types are merely the obverse of the factor approach. The methods suggested by Horst, Lubin, Meehl, McQuitty, Ellson, Gibson, and others for finding or using type, pattern, or profile functions are most simply applied to factors rather than single variables. Indeed there are serious objections to applying them to variables [28]. A whole new development of theoretical understanding lies open, in terms of *pattern emergent functions* applied to *factor profiles*, once our grasp of the number and nature of personality factors in man has reached acceptable precision.

THE CONCEPTUAL STATUS AND INTERPRETATION OF FACTORS

The notion that a factor is a single unitary influence underlying many manifestations rests on the logical premise, found for example in the writings of the logician J. S. Mill, that covariation betokens a common cause or elements. It has been suggested, however, [14] that in psychology we should not conceive a unity as an all-or-nothing "reality" but admit *degrees of efficacy* or potency. A certain unity may show itself in R-technique studies with adults but not with children, as some of the primary abilities do. Another may show itself in all R-technique analyses but not in P technique, as general body size and general intelligence do, and so on. In general, a unitary influence is capable of maintaining its unity only through certain ranges of conditions. Parenthetically, a unity which shows itself in P technique should in general be expected to show itself in R technique, but not vice versa; for the levels of variables as caught at a given moment, in a given population sample, contain both the fixed individual differences and the internal fluctuation, i.e., they represent both a trait and a state. Recent evidence agrees in systematically turning up more factors for the same set of variables in R technique than in P technique [28].

At this point, we may state that not merely will a unitary influence show itself as a factor, but that no inference about the existence of unitary influences is possible, by known scientific method, *except* through multivariate analysis, over the range of designs here listed. The existence of a unity cannot be proved by intuitive perception, or by univariate experiment, or by clinical inference. (For the last is but an approximation to the statistical multivariate analysis procedures.) However, a controlled experiment may be *part* of the proof of existence of a unity, provided that the experiment has multiple dependent variables. For example, Cureton obtained six R-technique factors in about one hundred physical-performance variables, and labeled one of these factors "cardio-

vascular efficiency," i.e., capacity to bring oxygen to the tissues. Later experiments with the same variables in a "high altitude" oxygen decompression chamber, using oxygen pressure as the single independent variable, showed that it was the particular pattern of variables loaded in this factor, and no others, which showed deterioration with drop in oxygen tension. Another example is the discovery of the same particular stress-response pattern, on the one hand, by Selye's fitting together of evidence from several kinds of controlled experiment on animals and, on the other, by our P-technique factoring of the same variables in human beings [27]. On the other hand the checking of a hypothesis about unitariness by a hypothetico-deductive sequence of univariate experiments is less satisfactory, because of our inability confidently to integrate correlational evidence from many different samples.

Granted that a unitary pattern is established, qua pattern—and this requires statistical checks, such as the salient variable similarity index—the next step is its interpretation, as a cause or dimension, or at least the formulation of a progressively testable hypothesis about it. The interpretation of a factor is sometimes made by considering the set of variables *highly* loaded in it, i.e., those whose variance is substantially accounted for by the factor, and seeking to abstract some quality, content, or principle common to them all. This is all right as far as it goes, but the complete procedure requires attention not only to the variables highly positively and negatively loaded but also to those with essentially zero loadings. For we infer the nature of a thing not only from what happens when it is present but also from what happens when it is absent. Moreover, the psychologist needs to be more alert than he generally is to the possibility that a variable important to his deductions actually was *experimentally included* in the researches he is surveying. There are instances of psychologists forming hypotheses on the assumption that a given variable forms no part of the factor pattern when it was, indeed, never included in the correlation matrix and so could not possibly manifest a loading.

The process of deeper interpretation of a manifest factor pattern in terms of a *source trait* entity consists usually of a hypothetico-deductive experimental sequence. Incidentally, it has often been maintained, even by factor analysts [49], that the multivariate design differs from the controlled univariate experiment in not being hypothetico-deductive. On first seeing a factor loading pattern, meeting the conditions of simple structure, the experimenter forms a hypothesis about the nature of the source trait. From this he deduces that a previously unused variable *A* should be more highly positively loaded than anything he now has in the matrix, that another, *B*, should be more negatively loaded, and that a third, *C*, should be unaffected. With these three (or more) new

variables he reenters experiment, to see whether his deduction is confirmed.

Incidentally, we should note that this is a more logically exacting, and frequently a more statistically exacting, test of a hypothesis than the mere establishment of significant difference on a single variable, as in univariate experiment. For in the former the experimenter predicts that a whole *pattern* of variables will behave in a certain fashion, whereas the fact that a single variable increases or decreases as predicted, in a univariate experiment, usually leaves inference much more undetermined. However, it is very rarely that a correct hypothesis for a factor has been reached in a single act of reasoning, and more commonly we proceed through a spiral of hypotheses and experiments, gradually raising the loadings of variables toward that value of unity (when corrected for attenuation) which permits us to say we have found *the* underlying variable which *is* the factor.

In this connection we should note that though "factor" and "source trait" are often used as synonyms, yet there is in fact a conceptual duality. On the one hand, we have the factor (not necessarily a factor in a single matrix) which is strictly a *factor pattern of loadings*, as inferred for a parent population; on the other, we have the concept of a single underlying "intermediate variable" [75] which causes this pattern. The pattern is our only means of referring to the source trait, of recognizing and defining it. (At least, unless there is supplementary controlled experimental evidence as mentioned above.) And yet we know that this pattern can never be exactly the same from one sample to another, because of sampling and experimental error; or from one population to another, because of systematic influences; or from one technique to another, since, for example, some variables, which do not vary from person to person, can fluctuate in P technique, over time, and vice versa. The *source trait* is the entity, whether it remains abstractly a construct and concept, or comes to be representable by a literal variable never seen before; whereas the *factor* is only a pattern found in some complex statistical derivatives called loadings.

The identification of the source trait from the pattern can always be made by understanding and applying the statistical and other laws which produce the various pattern modifications. But the duality remains, and must be carefully preserved in thinking. The chief practical reason for respecting it is that many years may elapse between the recognition of an invariant, experimentally replicable pattern (including its *proof* as a pattern), on the one hand, and its successful interpretation by a correctly named and conceived source trait on the other. During this period in limbo, it is important to preserve the pattern with a label which is as far as possible descriptive rather than interpretive. For the downfall of

“faculty psychology” was brought about, not by any fallacy in the notion of a faculty, but by the fact that the faculties were allowed to form themselves merely on the patterns of existing words. Incidentally, the odium which science properly attaches to this verbal vice (even though the vice is now driven out of personality research where it most flourished), still attaches itself to some concepts in learning theory and comparative physiological psychology. Nevertheless, although factor analysis from the beginning seeks the real evidence of functional covariation, instead of unconsciously accepting the false unity of words, yet the premature attachment of interpretive labels to factors may prejudice real freedom of thought and experiment. It is for this reason, and to facilitate work *on the establishment of factor patterns per se*, that the present writer has suggested a Universal Index, with a number for each pattern believed matched over at least three independent studies [26, 28]. Some of the factors believed established will be discussed in the following section.

CLASSIFICATION OF FACTOR PHENOMENA BY MODALITY, DATA, AND ORDER

Every substantial science has its taxonomy. Each passes through a phase in which greatest activity is directed to producing order and stability of nomenclature, before its more comprehensive theories—at least, *genuine* comprehensive theories—can hope to emerge. So in personality study, before “findings” can be discussed in terms of purely psychological concepts and laws, some statistical and methodological points have still to be clarified concerning the classification and ordering of factor patterns *per se*. It is usual to speak of ability factors and temperament factors, of general and specific factors, of behavior factors and questionnaire factors, of first- and second-order factors, and so on. How correct is it to use these categories, and on what are they founded? Perhaps four questions will get to the heart of these problems:

1. What is the relation of a factor founded on behavioral phenomena to one founded on introspective, questionnaire response?
2. How do we know that the factor dimensions we obtain span the whole personality, or some given domain of it?
3. How do we know when a factor belongs to one modality or region, e.g., that it is an ability factor rather than a motivation factor?
4. If there are first- and second-order factors, how do we know at which level we are operating in a given case?

The first two questions need simultaneous discussion. The question of whether a factor is truly general, i.e., whether it spans the whole domain of human behavior, involves also asking whether experiment has

yet covered all human behavior. To ask how we know that a factor is "general" is in a sense as ridiculous as asking, "How do we know when the science of physics is finished?" But consider the question, "How do geographers know when all new land has been found?" and it will become apparent that there may, nevertheless, be possibilities of progressively detailed exploration within a definite, finite area.

Development of an acceptable notion of a total, definitive area of personality manifestation would have considerable appeal in relation to several theoretical problems in structured (factor) measurement. In the first place there are greater difficulties in attempting to integrate a piecemeal, step-by-step exploration of different areas of variables (as is feasible in most other scientific areas), compared with those encountered in an approach attempting to "block in" the main perspectives from a *total* realm fixed from the beginning. But a "total realm of phenomena" requires the concept of a "population of variables," with sampling properties similar to that of a population of persons. (In terms of R and Q techniques, or any other pair of transpose techniques, the persons and the variables have, of course, just such a reciprocal, equivalent relationship.) Variables consist of stimulus-response pairs, so *in principle* the possibility exists of defining a total population of stimuli, response habits, or linkages of these, within a given culture pattern. This special, but basic, issue is discussed more fully in an appendix to the present article.

Before a solution is suggested here, however, it behooves us to note that the stimulus-response behavior of the human organism is observed in three distinct media. It may be observed as behavior, embedded in the actual life situations, in which case we will call it the life record medium, or L data. Or it may be observed as introspective responses made to a questionnaire. This we shall call Q data. A good deal of subtle reasoning could be followed up about Q data, but the main point is that they really consist of two distinct kinds of data, with different properties, according to *how the questionnaire is scored*. If, on the one hand, we accept the common meaning of words, i.e., accept the answers as fact about the individual's consciousness and behavior, we shall call it Q' data. It yields "mental interiors" [14] and is susceptible to no reliability coefficient between two equivalent, but different, observers. On the other hand, if the answers are accepted only as a form of behavior, i.e., if when S responds "I am shy" we do not take it as evidence of shyness, but only proof that S so responds, we shall call it Q data. Such data belongs with the other test approach—objective tests—and so, if we abandon the introspective, Q' data, we have *essentially* only two media, the life situations and the test situations.

Now the notion of a population of variables must rest on the life situations, for tests can be multiplied according to whim. Lacking re-

sources to make a cultural time-sampling of human behavior around the clock, the present writer suggested using language as a mirror condensing this behavior. It was assumed that the dictionary must, by the twentieth century, have stabilized the number of symbols required to refer to all aspects of human behavior of interest to man. This symbol collection is called the *personality sphere*, envisaged as a finite but unbounded set of symbols, which can be represented as points in hyperspace bearing a spatial relation to one another which is some function of their meaning relationship.

As a broad strategy of research it has been advocated [14, 28] that source traits should first be found within a stratified sample of variables based on this form of L data (behavior *in situ*, rated with special precautions). L data should have primacy because (1) according to the above argument, we can be reasonably certain of covering the principal dimensions thereby, (2) the factors will appear clothed in terms already familiar to us (in everyday and clinical language), so that interpretive hypotheses may be readily reached, and (3) the construction of tests objectively to measure these primary factors will then be guided by these hypotheses and will no longer be at the mercy of disproportionate multiplication of test behavior merely in some test-convenient areas. Enough of factor research has followed this strategy to permit development along the lines indicated.

The question of whether a factor is an ability or a dynamic or a temperamental trait has usually been confidently decided, among most psychologists, by common sense—until those numerous borderline cases arose which proved common sense inadequate. A special analysis of this problem [14] has suggested that there are in fact three possible modalities of factors, as implied above, though any given *variable* in general expresses in varying degrees *all* three modalities. A variable (trait element) is defined as dynamic in proportion to the degree that the mean score (for a population) changes in response to changes in the incentive. When the score is in an “irrelevance range” of immunity to changes in incentive, and thus becomes sensitive to changes in *complexity* of the situation, the test becomes mainly an ability test. A measure which is insensitive to both changes in complexity and changes in incentive is defined as a temperament measure. Changes in the situation which are not changes in incentive are changes in complexity. For logical completeness this system of definitions now requires an independent definition of an *incentive*. This can be achieved by longitudinal analysis of behavior, defining a goal by consummatory responses at which a train of behavior is found to cease. However, there are complexities in the modality question which require such space for discussion that the reader must be referred to the original [14] statement of theorems.

Although operational definitions of the three varieties of modality can thus be obtained, to supplant the rough hunches of psychological common sense, yet it may be asked whether the modality classification of factors—on variables—has any value except as an academic exercise. The answer to this would seem to be that the properties of abilities, temperament traits, and dynamic traits differ in so many ways that there is real predictive convenience in having factors as far as possible representing purely one modality or another. Now the factors that we usually get from random and unsorted variables and samples will normally be *wholistic factors* [14], i.e., extending *across* modalities. To take an even broader example, in a group of children ranging from five to ten years where physical and mental variables are factored together, we might get a single growth factor, covering both intelligence and physical size. On the other hand, in most factor analytic studies we actually get *conditional factors*, i.e., factors restricted to a particular modality, because the variable sample is suitably restricted and all the variables are in any case presented with certain conditions retained in constancy. For example, in ability measures a high and sufficient *motivation* is normally maintained throughout, whereas, in motivation measures, intellectual complications as such are implicitly eliminated. Consequently, in what follows we shall generally deal with (*a*) ability, (*b*) “general personality and temperament” factors, and (*c*) purely motivational factors, wherever “conditional” experiment has been attempted. However, it has to be admitted that complete modality separation and clarity has not yet been reached, either theoretically or in the findings.

Our fourth question dealt with the general nature of factors, particularly the thorny issue of determining whether a factor is of first, second, or higher order. It might seem sufficient to say that any factor found by factoring an initial collection of operationally defined variables is a first-order factor, and that any obtained by factoring the resultant factors is a second-order factor, and so on. A little reflection will show that although this should suffice *generally*, it *may* fail. By factoring 30 to 50 varied ability tests, Thurstone and others [57, 90], have found about a dozen primary abilities. The simple structure shows these to be oblique in relation to one another. When the correlation matrix among these primary factors is then factored, one or more second-order factors appear, and that which is most general to all the abilities is considered to be Spearman's general intelligence factor. It has been shown that its loadings directly in the tests are the same as those obtained for Spearman's general factor, “g” [28]. Thus general ability can be obtained either as a first-order (primary) factor or as a second-order factor, though in the first case we have to take special precautions (tetrad differences made to equal zero) in choosing the variables from which we shall work.

Parenthetically one should note that about half the writers on second-order factors have them wrongly conceived as factors obtained from the correlations of either (1) the reference vectors, when indeed a corrected *inverse* of this reference vector matrix is what actually has to be used, or (2) the factors as literally experimentally measured, by some battery of constituent subtests. Owing to the immense labor and skill required accurately to determine the exact hyperplane angles in (1) as well as the need to take a mean of several studies, no data fit for a second-order analysis of personality factors have been available until quite recently [28], though the present writer must confess to a premature attempt to determine second-order structure in 1947.

The second-order factors of general anxiety, extraversion, etc., recently found in personality will be mentioned in the ensuing brief survey of experimental findings, but the important point for the present methodological and taxonomic discussion is that in these, as with the older established general ability factor, it has happened experimentally that the same factor has, in different settings, been picked up both as a first-order and a second-order factor. Indeed it is possible to see theoretically that this could happen. For example, if we started a supposed first-order ability factorization and happened to use as variables *very pure measures* of Thurstone's primaries, and *only one* measure of each, our factors immediately obtained would be factors generally encountered only as second-order factors. In personality most measures are not so pure—the proposed measure for factor A contains also some B, C, etc. Consequently, if there are enough variables, the factoring of these supposed first-order factors still yields first-order factors, as was found in Lovell's and Thurstone's reanalysis of Guilford's highly intercorrelating questionnaire measures of factors. But factorings of the Sixteen Personality Factor Questionnaire, with its relatively pure first-order factor measures, have yielded second-order factors immediately [28].

Without laboring what may seem an unduly technical point, let us take it that both theoretically and practically we know that, with rather unusual circumstances, it is possible to "go right through the floor" directly from variables to second-order factors without intervening primaries. Consequently one cannot *infallibly* tell on which floor one has landed by the merely operational definition that first-order factors are what you get from test variables and second-order factors are what you get from factoring factors. Other things being equal, the danger of confusion through going directly to second-order factors, unknowingly, from variables is much greater when variables are very diverse in nature and chosen sparsely from a wide area. This argument that factor order is related to density of variables can be extended, as far as one can see, to third- and higher-order factors. An acceptable taxonomy and classifica-

tion of factors according to order, and some other factor concepts, would therefore be assisted by an operational concept of *density of variable sampling*. [For more adequate examination of the assumptions in variable density the reader must be referred elsewhere, 28]. Research has been able to keep factor orders tolerably clear by a common-sense regard for frequency of variables in a given area. The accumulating evidence now strongly suggests that what we commonly call primary personality factors are on the same level as general intelligence, for "g" appears consistently as one of them [26]. If this is so, the primary abilities are one order lower than primary personality factors, whereas the broad, second-order factors among personality factors are actually on a third level. At least three factor orders are thus known and used today.

There are accumulating indications that in general the correlations among first-order factors are smaller than among variables, and those among second-order smaller than among first-order, so that we shall probably find that factoring of factors will quickly come to an end, and probably three or four orders will suffice. Conceptually, the higher-order factors are organizers among organizers and may carry the investigator outside the academic field in which he began his work. For example, the second-order general ability factor might turn out to be a function of the total number of effective cortical neurones, i.e., a physiological concept, whereas the primaries are evidently psychological specializations of a general "relation-perceiving" capacity, in numerical, verbal, and other fields. On the other hand, the step from one order of organizers to another may carry us out of psychology in a different direction, into sociology, since one of the second-order factors among personality factors looks like the orientation of those factors produced by social status.

In sum, there is a rationale for an initial taxonomy of factors according to classification by (1) medium of observation, covering L, Q, and T data, (2) modality, covering ability, temperament, and motivation, (3) order, involving the notion of variable density and the personality sphere. These are additional to the earlier (4) R-, P-, and T-technique design origins, and to a further split that will be made later among motivation factors.

THE PRESENT STATUS OF FINDINGS

The year 1958 is a fortunate one in which to be summarizing actual findings. Throughout the thirties and forties there was chaos; only in the last two or three years have the results of many studies finally begun to fall into place, showing both order and gaps, as the periodic table did in the generation of Mendeléef. Since the coherences are still patchy,

however, it is desirable to treat each medium and modality on its own, proceeding to speculative total integration only after the firmer partial steps have been separately described.

Even in one modality and field of data the decision as to existence of a confirmed factor pattern rests on several technical instruments. It requires, for example, first, a test such as Bargmann's [7] showing that the simple structure rotation position is uniquely significant, and secondly, the confirmation of the same pattern by at least three quite independent, blindly rotated experimental studies, all with adequate samples, etc. This is a matter much assisted by the *social* organization of research, e.g., the provision of a universal factor pattern indexing system, as described earlier, and of precisely defined variables in a master index list of "markers." Finally, it requires a device to measure the significance of pattern matching between factors from different studies.

Actually the matching part can rest on three approaches:

1. By establishing a similarity of *the loading pattern*, on variables common to the two studies, which exceeds chance expectation by the usually accepted significances. Actually this similarity of the factors per se can be examined over more than the loading pattern only, e.g., by a comparison of their mean variances, the angles to other known factors, and other properties of the factors, though, in practice, fiducial limits have so far been worked out only for the loading pattern.

2. By measuring the same population on both factors and showing that the correlation of the two factors thus measured is not significantly short of unity.

3. Possibly by the as yet untried hybrid "transformation analysis" method of Ahmavaara [1].

The first method can be used only when identical variables—marker variables—are carried through the two studies; the second can be used either with or without this condition. In either case it will be seen that the day is past when psychologists might be permitted to match factors on an intuition that they were "psychologically very similar in nature." Textbooks are full of factorial castles in Spain built on sincere convictions that certain factors confirm the hypothesis set out by an earlier factor—or even by some verbal definition of the author's concept.

Matching through the pattern in common variables has so far been done by (1) correlating loading patterns, (2) matching patterns by the pattern similarity coefficient r_p which, unlike r , takes *level* as well as *shape* into account [17, 44], (3) using the nonparametric *salient variable similarity index* s devised by Cattell and Baggaley [31] specifically for factor matching. The technicalities of the relative emphasis on these matching tests cannot be entered upon here, but their recent use on a series of three to ten planned studies [26, 27, 28] with sufficient common

marker variables, has shown for the first time, beyond cavil, that simple structure yields unique, replicable stable factors from study to study.

Fortunately, the detailed documentation needed to substantiate our account of the 1957 status of replicated factors in L, Q, and T data, and in general personality and motivational modalities can be omitted because of the simultaneous publication of an intensive survey of this whole field [28]. There it is shown that in L data (i.e., life record data using common verbal definitions of specific behaviors observed in everyday life) "criterion" situations, some 14 or 15 factors have been established, each in a minimum of 3 studies. Striking similarities also exist between some of these and personality dimension concepts, e.g., schizothymia, anxiety, sex drive, commonly derived from experimental and clinical fields [101]. The interesting fact is that the list of patterns agrees as far as the latter concepts go, but that they also go beyond known concepts into dimensions unperceived by the unaided clinical eye. For example, although the first and largest factor is the "cyclothyme-schizothyme" dimension, long regarded as basic in psychiatry, there is now also a *second* schizothyme factor concerned with a pattern of shy withdrawal (H factor) not associated with hostility, as it is in the first pattern, and this has not been reliably perceived except by factor analysis.

The familiar clinical concepts of ego strength and superego strength are now confirmed as independent unities, and it is shown that, in the normal range, guilt plays a very small part in the functioning of the latter, in contrast to the pattern perception as biased by clinical sampling. Other multivariate patterns that can also be recognized from premetric concepts are dominance-submission, paranoid trend, timidity, and tension. The surface trait or second-order factor of extraversion-introversion, as conceived by Jung, is found to resolve itself into at least four functionally independent factors, the most outstanding of which are Surgency-Desurgency and the factors named Parmia and Praxernia. These three factors are interpreted as representing, respectively, freedom from past punishment, parasympathetic resistance to threat reactivity, and a temperamental conversion-hysteria component. All fifteen L-data factors have been represented in the Universal Index as U.I.(L) 1 through 15, or in a noncommittal, local laboratory order (of mean variance), by the letters A through O.

In the questionnaire or Q-data medium, independent factorings and matchings have similarly established [19, 28, 38, 56, 60], in at least 3 studies, some 18 factors in adults and 12 in children. The most general of the former are included in the Sixteen Personality Factor Questionnaire, and Thurstone and Guilford-Zimmerman questionnaires; the use of these against various social, occupational, and clinical criteria has done much to enrich our practical knowledge and theoretical interpretation of the factors.

The *direct* impression one gains from the "mental interiors" presented by the items loaded in these factors is that they agree one to one with the behavioral exteriors in the L data; but the above objective checks have to be applied, and in this bridging unfortunately the easier methods of matching by variables cannot be used, since no variables can be identical in the two media. Accordingly, cross-media matching has to be carried out by the second of the above matching methods, i.e., by obtaining the two sets of factors on a common population of subjects, and intercorrelating the factor scores. The results largely confirm the psychological impressions; such factors as A, E, F, G, H, L, and O run through both media, showing that a real trait keeps its functional unity despite different behavioral media, simply changing its dress as the realm of possible manifestation changes. On the other hand there are *some* behavioral, L-data factors—D, J, and K—and *some* questionnaire factors—Q₁, Q₂, Q₃, and Q₄—which have not yet been found in the opposite medium. This may stem from their having much smaller variance in the other medium, from some real influence associated with the standpoints of the internal and external observers, or from their manifestation's being truly confined to one medium.

Since use of the questionnaire and rating techniques, outside the fully cooperative atmospheres producible in the pure research situation, is liable to motivational and other distortions, there is urgent practical need to transfer measurement of personality and motivation factors to *objective* tests. By an objective test we mean an exactly reproducible situation and set of instructions in which the subject's responses are scored in ways of which the relation to his personality is obscure to him. (Needless to say, the responses must be understood and scored similarly by different psychologists.) A considerable variety of tests in the form of miniature situations, "projective" or misperception tests, stylistic, and physiological measures—and other forms yet without a name—have been tried out in factor analytic designs by Brogden [9], Ryans [83], Crutcher [45], Rethlingshafer [82], Thornton [89], Thurstone [92], the laboratory of the present writer, and others. In the last fifteen years, the writer and his colleagues alone have produced over five hundred different test designs [15, 21, 42, 30, 24, 39, 36, 37, 26] based on the hypotheses about individual factors found in the L- and Q-data studies, but the evidence below suggests that these and such tests as the Rorschach, Downey, Szondi, etc., still leave important dimensions of personality to be covered.

Surveys of factors in objective tests were made by the present writer in 1946 [14] and by French in 1953 [55]. However, a firm evaluation became possible only with Bargmann's devisal of a significance test for simple structure [7], with the introduction of the salient variable similarity index for factor matching [31], and above all, with the fruition

of a long-term research plan designed specifically to carry systematically representative markers through several independent population samples, factor extractions, and rotations. The matching of seven studies has now proved to be good; in 1955, there appeared a final integration and interpretation, revealing twelve factors of a relatively high degree of definition and invariance and six of a less satisfactory degree. These eighteen factors included [26] those found by Eysenck [50], Thurstone [92], Gruen [37], and Dubin [36] on special groups and showed that these factor patterns persist with little change through normal and abnormal, younger and older populations.

On the other hand, the alignment of these T-data factors with those in L and Q data is far from simple. Three studies [40] have been carried out, using the second (and only possible) method of matching. They show that there is good matching of L and Q factors, but that relatively few of the L and Q factors have yet been located in objective tests, and that in some cases what appears as a first-order factor in T data is second-order in the other realms. For example, the anxiety factor [U.I. (T) 24] in objective tests correlates substantially with the distinct questionnaire anxiety factors O, Q₄, L, and C(-), now known to form a second-order Q factor, while the invia-exvia factor [U.I.(T) 32] appears to be a second-order factor among a group of three questionnaire factors defining introversion-extraversion behavior. The L- and Q-data factors which appear to be most *directly* represented in objective test factor equivalents are: G, superego, M, Autia, I, Premsia, K, Comention, N, shrewdness, and L, Protension, or paranoid tendency. Quite apart from matching with other media, the psychological meaning and consistency of the T factors are good, and one can recognize among them such factors as general character development, anxiety, assertion, psychoticism, general inhibition or restraint, neuroticism (checked by Eysenck by criterion rotation on neurotic groups), hypomanic tendency, "corticalertia," and a superego-like set of responses called "Critical Practicality" or U.I.(T) 19.

Although the primary research task at present is the confirmation of the patterns themselves in further age groups, cultures, etc., and the discovery of test designs that will measure them with increased construct validity, their use in applied psychology, e.g., against clinical, occupational, and educational criteria, would also greatly help the task of interpretation. Essentially, we have reached the vital point where the factors are *verified as patterns*, and prior to which speculative hypothesis formation would have been a waste of time. But the stage is now set for more intensive hypothetico-deductive experiment, and much of this can be carried out by smaller-scale, univariate designs, eliminating the complex multivariate methods necessary at the "blocking in" phase.

Some special objective test factoring developments should be indicated in order that the full scope of present results may be properly perceived. These developments diverge from the line of general tests for general personality factors in that they factor a series of tests all involving the same kind of response, notably musical preference reactions, esthetic tastes, humor preferences, and motivation responses. The last deserves a special section later. The factoring of laughter response to jokes is based on the Freudian theory that strength of reaction to wit betrays the strength of particular repressed tendencies in the unconscious. Stable factors, not quite as clean-cut in simple structure as for general objective tests, have been found in these realms by Andrews [5], Eysenck [49], Cattell and Anderson [30], and others. Except for a few special cases, e.g., Eysenck's relation of the "bright, clear color," picture preference factor to Surgency-Desurgency (hysteria-dysthymia) and Cattell's definite relation of the "sexual and debonair wit" factor to L and Q factor H, Parmia, the relation of these factors to those in other realms of expression remains to be determined.

All the matching and confirmation problems so far considered have been those among the different media using R technique, in which the great majority of published factor analyses are expressed. In the last ten years, however, a brief but vitally interesting collection of P-technique analyses has arisen, i.e., of longitudinal factor analyses within single individuals, and these have planfully used the same variables as those in the R-technique factorings [34, 35, 27]. In cross matching from R to P no exact statistic can be used, because sampling is not comparable, and certain systematic differences would be expected in the pattern from the same source traits by the two situations. However, it is notable that considerable agreement exists, both in L and R data, and that the factors A, C, E, F, G, and H in L data, and U.I.(T) 17, 19, 21, 22, 23, 26, and 29 are believed now to be found both in terms of individual differences and in patterns of diurnal or other fluctuation (function fluctuation) within one individual.

P technique lends itself well to investigation of psycho-physiological connections, because many physiological measures fluctuate appreciably and are yet of a nature which permits of their being repeatedly tested, without the disturbances resulting from repeated application of psychological tests. This work, summarized elsewhere [27], has led to more precise delineation of sympathetic, parasympathetic, and stress reactions, and to recognition of the physiological associates connected with swings in the major psychological trait patterns. Notably here is the identification of high serum cholinesterase and low alkalinity of saliva with states of surgency, of reduced metabolism with the paranoid states, and of higher blood pressure, pulse rate, and ketosteroid output with anxiety.

P technique has also been extremely useful in discovering the factor structure of motivation manifestations, but since special theoretical developments are necessary in presenting the motivation factor findings, they are deferred for separate description. The rapid review above, which should be supplemented by reading in the systematic factual surveys now available [28, 56], shows that the harvest of consistent, confirmed empirical findings has relatively suddenly become far more extensive than is commonly realized. This is especially true of certain experimental and applied fields, which could avail now themselves of the new structure with great advantage.

COMPLEX FUNCTION AND CONFIGURAL, TYPE PREDICTION FROM SOURCE TRAITS

At this point, with actual factor findings available for illustration and testing of the assumed properties in the model, we can return to a further development of the theoretical position set out under Personality Research in Relation to the Two Basic Scientific Methods and The Logic of Factor Analytic Experiment above. This explicit treatment is necessary not only to develop the full use of personality factor measurements of *all* kinds, but also to lay the foundations for the more complex research concepts encountered in factoring dynamic motivational data. Principally we are now concerned to make a more explicit statement of the assumptions in the mathematical model which we are using, and to make a further transformation of some current rather vague psychological concepts in personality into exact operations related to our model.

Two major assumptions (besides homoscedasticity) are made in factor analysis:

1. That linear relations exist (*a*) among the variables (so that product-moment correlations can be used) and (*b*) between the factors and the criteria,
2. That factor functions, whether of the first or higher powers, are influences which combine additively, rather than by some more complex interaction.

The linear and the additive statements are constantly confused in discussion. As to the correctness of the first it can be said that real curvilinear relations have very occasionally been found in personality variables, but that in the typical correlation matrix used, as many as two or three thousand plots have sometimes been examined without finding a single significant departure from linearity.¹ On the other hand, there

¹A practical comment on this situation was made by Flanagan: "extensive study of large samples in research during the war failed completely in establishing anticipated curvilinear relationships" [55].

are indications, though scarcely proofs, that curvilinear relations between factors and *criteria* are fairly common. For example, ratings on occupational proficiency suggest, but do not yet prove, that middle values in certain temperament factors are sometimes more effective than either extreme value.

If there were definite demonstration of a relation of this kind between a factor and a criterion—instances are Eysenck's suggestion that neurosis may be more common with high and low than average intelligence, and the present writer's finding that low ego strength (C factor) is found with both high and low extremes of rigidity [11, 24]—it could be handled either by modifying the specification equation, or in applied work, simply by scoring the factor on a new scale from a new zero point. In the former case, a parabolic curve would be represented, expressible, according to its axis, by making the criterion *C* a function of plus or minus F^2 or $F^{1/2}$. Of course, this modified specification equation could no longer strictly be used as an integral part of ordinary models for *factor extraction and rotation*. However, it is likely that by choosing small ranges, in which the linear approximation is good, the current model can continue to be used to *find* factors, even when curvilinearity or non-additivity ("joint functional relationship") exists. Then, in *using* the factor, over wider ranges of variation, the appropriate nonlinear function could be found and used. For the speculative nonlinear factor models of Coombs and Satter [43], though of great interest and promise, have not yet been worked out in a way that would permit extraction of factors, and specification equations, from experimentally observed relations among variables.

The second assumption—that factor functions are additive—could break down in many different ways. It might, for example, be necessary to change the equation to product relations, or to include both simple summation *and* product terms (interaction terms in the analysis of variance conceptualization), or to introduce products involving higher powers of the factors. The variations are indeed infinite. The notion so frequently raised—but rarely in clear or testable form—in clinical psychology, that the profile or pattern may have effects through its *shape*, independent of the effects of the absolute levels, is another way of bringing up the same question.

The normal procedure in science is to adopt the simpler model unless and until results prove that a more complex one is required. Although clinicians, in particular, have brought up apparent examples requiring a more complex model, the present writer knows of no well-substantiated, *cross-validated* example that cannot be worked out as well by the simpler model. Empirical results are conflicting. Improved results are claimed for configural scoring by Meehl [76], Saunders [84], Fiedler [53], and

Lubin and Osburn [70], but the simple linear treatment was found superior to pattern, configural, or complex function methods by Tucker [94], Bell [8], Ward [97], Lubin [69], and Lee [68]. However, it is very probable that true cases of more complex, interactive configural effects of factors exist and we shall return to the problem in a moment, after a clearer development of assumptions in the main model.

Practically, it can be said that as far as *locating* factors is concerned—as distinct from using them in more complex situations—we can either (a) locate them initially among variables (the majority) in which the relations are linear, or (b) take small ranges, as stated above, in which the linear approximation is close enough to permit the model to work. In this connection it should be pointed out that artificial examples by Thurstone [90], Bargmann [6], and others, in which complex functions (higher powers of factors and products of factors) have deliberately been introduced as the basis for correlation, have nevertheless always proved susceptible to factor analytic reduction, and the complex relationship has been found to appear in terms of its nearest simple additive equivalent.

In any case, a great deal of work remains to be done in psychology with the present proved effective model. As usual, armchair speculation is running far ahead—or astray—of effective integration of theory with actual research. Before speculating indefinitely and philosophically, it behooves us to understand fully the implications of our present model and to use it as a tool to advance psychological knowledge (and therefore knowledge of the required model modifications). These implications are:

1. That although conceptually we analyze the individual into dimensions, any of his acts is an act of the total personality. We represent this integration by giving influence to the majority of dimensions (in the specification equation) in estimating the magnitude of *each response*.

2. Since loadings can be both positive and negative, we recognize that some factors help in some circumstances and interfere in others. As we shall see (under The Evidence for Motivational and Organic Factors; Erg and Sentiments) in the special case of motivation factors, this difference of sign is interpreted as evidence of conflict.

3. The same level of response can be reached, according to the specification equation, by persons having different (but equivalent) factor endowment patterns. This equivalence of different behavior can readily be perceived as true in psychological observation, and we then say that the “quality” of the performance is different in the two cases, even though the quantitative *level* is the same.

4. Although the factors added are all in standard equivalent scores,

these scores are not identical in terms of any absolute dimensions and qualitative characters of the units from factor to factor. For example, we have no evidence that the variability (range) of people in intelligence is smaller, equal to, or greater than their range in Surgency. But in the specification equation we add ability, temperament, and motivation units, or habit strength and frequency units, in the same realm of standard scores.

5. The *s*'s, or factor loadings in the specification equation for a particular stimulus-response variable, may be considered as the psychological dimensions of the *situation*.

6. This last statement, like most quantitative psychological statements, has meaning only relative to a given population. Indeed, the factor pattern itself, similarly, is something defined in terms of a population (or in P technique, a population of occasions in the individual's life).

7. The usual factor measurement assigned to an individual is only a statement that he is at the given level on that factor at the moment of measurement. At what level he will be on other occasions is to be inferred from our psychological knowledge of the factor, the statistical findings on function fluctuation (complement of stability coefficient), and the general psychological laws of learning and maturation for that factor. For example, the factoring of dynamic data reveals a sentiment pattern of "interest in one's profession" affecting a whole pattern of interest and skills. In industrial psychology it is not unusual to predict a person's future adjustment to a particular occupation from an "occupational interest blank" measuring his interest *before* he is actually in the occupation. The learning from repeated actual exposures to the occupational situation is likely to increase the strength of this factor to a point at which individual differences are likely to have little relation to those before learning. Consequently the effective use of factor source trait measures requires general psychological understanding of the way in which maturational and learning laws are likely to affect their future course. Indeed, one of the major superiorities of source trait formulation over mere use of variables, in applied and experimental psychology, is the fact that these meaningful unities can be effectively brought into relation with general psychological and physiological laws of growth and learning.

With this brief statement of the psychological implications of the present factor model, let us turn to its relation to configural and typological prediction. The recent furor of enthusiasm for the latter seems to have confused, rather than developed, two basic truths:

1. That configural methods resolve either into (*a*) use of complex mathematical functions of the profile of factor (or variable) scores or

(*b*) a simple recognition of types, together with an Aristotelian logic, "This is a dog: therefore, it may bite," i.e., the use of memory rather than calculation. Ellson, McQuitty, and Lubin and Osburn are among the few who have recognized that the task in the latter approach is simply to key a *species* against the *criterion properties* of the species, without any immediate attempt to "understand" the property in terms of general scientific parameters defining the species. Conversely, Horst [64] has demonstrated that many attempts at configural pattern scoring are properly examples of—and would be more clearly conceived as—a modified specification equation, as in configural method (*a*), using the regular factor or other parameters in mathematical functions.

2. No matter which way one decides to *use* types, they can be *found* as modal patterns in a distribution of patterns in a space of dimensions or parameters common to all types. Their final separation, however, may require addition of dimensions, for particular pairs or sets of types, not common to all types. Thus raises the basic proposition that "trait" and "type" descriptions are not in different worlds, but are simply reciprocal, complementary, and mutually dependent ways of analyzing and abstracting the same data. This can be seen most clearly in the case of R and Q techniques, which are mathematical transposes. In other words, traits (or, beyond psychology, attributes) are abstractions made from correlating variables over sets of organisms, and types are abstractions made from correlating organisms over sets of traits. The approaches duplicate in statistics the division in language between adjectives on the one hand and nouns on the other (or, if processes rather than persons are our concern, between adverbs and verbs).

The present writer has explored elsewhere [28] the implications of the two brief statements above, at the much greater length which alone makes possible intelligible, if not final, formulation of the problems. At the risk of apparent dogmatism, the following points from that discussion may briefly complete the present picture:

1. Regardless of the mode of further use of types (*1a* or *1b* above), the discovery of types as modes (*2* above) inevitably falls into two different approaches with different conceptual systems:

- a.* One may take complex functions of the elements in the profile, e.g., a function best predicting the criterion, and find the modes in this univariate (complex function) distribution.
- b.* One may measure the resemblance of every individual to every other, by some pattern index operating upon attributes, and find the pattern modes ("correlation" clusters, in terms of the index values) among people.

2. The latter process has so far been used in a way which could not yield unambiguous results because:

- a. The indices employed, e.g., the correlation coefficient in Stephenson's Q' technique,² or in d^2 advocated by Osgood [80] and Cronbach [44], are not truly comparable from study to study, or they emphasize "shape," "level," or "deviation" in the profile matching at the expense of the total similarity. A pattern similarity coefficient r_p has been proposed [17] which takes all three aspects of a pattern profile into account and which has resulted in good functional grouping of national culture patterns [20]. Mahalanobis's general solution is also valuable [see 81] here. Parenthetically, it should be mentioned that discriminant function methods are of no use for typing; they require one to have some prior means of designating criterion groups and thus are circular in argument.
- b. Variables, instead of independent factors, have frequently been used as elements in the profile. Since variables may be highly correlated, one area of behavior may then be weighted out of all proportion to another, i.e., any figure for the similarity of two people is purely arbitrary, depending on the variables thrown into the matrix. The problem of sampling variables remains to be solved. Getting profile similarities with *factor* measures as elements solves this to the extent that *whatever is represented is equally represented*, though it still leaves the question of whether unknown regions of behavior are being omitted.

A grouping of persons in occupations, according to similarity of profiles of personality *factors*, has recently been attempted by Day and Meeland [see 28] and it seems that thereby more invariant groupings are obtained, as in the national culture pattern studies [17, 20], than in, say, McQuitty's [74] use of patterns on *variables* (test items). However, it is not only the advantage of statistical invariance but also of psychological meaning which points to handling patterns and types in terms of *factor* elements, as a more intelligent strategy.

Ideally, taxonomic and predictive problems are best handled in an integrated combination of type and parameter methods:

1. Choose variables on which all types can be measured, factor, and determine by r_p on factor profiles the modes (types) and their positions in this framework of generic transtype dimensions.

² Q' is best used instead of Q , because the three primary factor analytic designs and their transposes have been symbolized as R and Q, P and O, T and S. Locating types, on the other hand, involves only finding *clusters* in the correlation matrix and thus stops short of any true factor analysis, and is best indicated by Q' .

2. Factor within each type on dimensions both common to types and peculiar to the within-type variance of that type.

3. Handle any given individual by assigning him to a type, according to (1), and then predicting the deviation of his behavior from the mean behavior of that type in terms of his endowments on the within-type factors.

This approach allows for the existence of differences of behavior between types which are categorical, Aristotelian, and not yet predictable from the parametric traits by any pattern emergent function known to us.

As Thorndike, McQuitty, Ellson, and others [74], have shown for normal persons, types, as pattern modes, are mostly found in occupational skills and social role behaviors, rather than in basic personality source traits, which tend to be normally distributed. In abnormal persons, however, as Wittenborn's data tend to show [101], we may be dealing with segregating, modal patterns; and such patterns are clearly evident in some biological abnormalities, e.g., phenylketonuria, Huntingdon's chorea. Nevertheless, even in basic source traits, one might expect distinct types to emerge, if samples from different races and cultures are included in the analyzed sample. Currently in progress, is an experiment to determine the constancy of personality factor patterns across seven different countries, but regardless of the degree of constancy found, it should be possible to determine transcultural factors by factoring a sample with one representative from, say, each of a hundred cultures, and then plotting the distribution of patterns on these factors in a larger sample taking many from each country.

The problems of complex factor function, configural and type prediction are complicated. We have the mathematical and statistical tools for handling them, but we can use those tools intelligently only when we attend to what *is* rather than what might be. Conceivably some configurations will give "emergents"—in Lloyd Morgan's and Bergson's sense—which cannot be predicted by any mathematical combination or discoverable function of elements, and then a sheer type approach must be used; but it is to be hoped that the above stated combination of type and trait formulation, with its greater intelligibility and scientific appeal by generalizability, will fit the findings.

THE EVIDENCE FOR MOTIVATIONAL AND DYNAMIC FACTORS; ERGS AND SENTIMENTS

With the foundations of our model thus further clarified, we can turn to a new realm of psychological application in which some more exacting demands are made on it. The application of multivariate experiment to

dynamic motivational areas is quite recent—since 1948, in fact—but has had its results so quickly confirmed and has led to such a promising development of dynamic calculus, that it deserves special theoretical attention—and also special practical attention from the clinician.

As indicated in the earlier discussion of modality, *conditional factors*, largely peculiar to one modality, can be obtained by restricting variables to that modality. Thus, in this dynamic field the variables must be unquestionably motivational. The plan has been to redefine “attitude” as a basic motivational surface manifestation³ and, by factoring on a foundation of attitudes, to explore the dynamic structure of personality in terms of drives, sentiments, self-structures, or whatever other forms may turn up among the attitude elements. An attitude is a stimulus-response habit, expressible in the paradigm:

“In these circumstances . . . I . . . want so much to do this with that.”		
Stimulus situation	Organism	Response (defined as a course of action: “to do this”; of given intensity: “wants so much”; generally involving some reference to an object: “with that.”)

It is supposed that every major dynamic system must eventually express itself in attitudes, and in the courses of action that go therewith, so that by experimental analysis of these it should be possible to reveal the underlying systems. Parenthetically it must be stressed that *the above definition and measurement of individual attitudes cannot be equated with much of the attitude measurement that has been done in sociology*, because (1) an attitude here is not narrowly conceived as “for or against an object” but is free to assume any of a wide range of emotional qualities, e.g., curiosity about, or anxiety about, an object. (2) The self-conscious, self-evaluation, verbal, opinionaire method of measuring attitudes is not accepted as valid. Instead, a group of diverse (physiological, learning, perceptive, indirect verbal) *subtests of motivation strength* is used. The traditional verbal opinionaire correlates only about .3 with the pool of general motivation measures and thus deals with only some peculiar one-tenth of the total attitude strength variance.

In developing the new objective measures of attitude strength, over fifty widely chosen subtest devices [21, 32, 35] were intercorrelated (with respect to each of a number of representative attitudes). Factoring these devices of measurement methods revealed that all the tests by which strength of a motive is supposed to manifest itself do not “go together.”

³ Other terms are unsuitable. “Motive” may mean either a structure, as we intend, an incentive, or a process. “Interest” is equally uncertain, commonly meaning interest in an object rather than in a course of action.

There is not a *single* motive strength underlying all, but no fewer than five independent factors. These have been called *motivation component* factors to distinguish them from *dynamic* factors, discussed later, which structure the whole area of attitudes. Although the names may not appeal to experimentalists, the fact is that the character of these five factors so closely corresponds to the psychoanalytic concept of id, ego, superego, and unconscious complex components (plus a physiological component) that we have tentatively so named them. For example, the motivation component factor we have called the "id" contains all the "I want" manifestations, together with high fluency on good consequences, low fluency on bad, autistic misbelief and misperception phenomena, rationalization, and other ego-defense mechanisms [32]. At the same time it has no loading in the manifestations found in the realistic ego component, such as knowledge and skills in reaching the goal of the attitude, readiness to make effort and to learn, tendency to relate cognitively to other interests, etc.; and it lacks the GSR response, blood pressure changes, and other "complex indicators" present in what we have called the unconscious complex component [32].

Later it was shown that these primary motivation factors could be resolved into one or two second-order factors permitting, in the first case, a *single* over-all measurement of integrated motivation strength for any given attitude. A second phase of research next developed in which an objective test battery, to cover the main second-order factor or factors, on the above foundation of evidence, was applied to each of 30 to 50 variously chosen attitudes, in order to factor the *dynamic* structure, i.e., to find whatever drive or acquired dynamic⁴ habit patterns exist among human interests. In comparing the outcome with others, e.g., the work of Torr [92] and of Guilford and coworkers [see 28], it should be stressed that each attitude, though *represented* by a *verbal* statement, is actually measured by perhaps 30 or 40 responses made in *objective* tests, i.e., in the battery of GSR, word association, fluency, etc., measures as just described, and validated also against objective criteria, notably the actual amount of time and money spent on a given attitude-interest. The intercorrelating and factoring of 30 to 50 varied, but objectively measured, attitudes, on 3 substantial samples of young adults, has shown remarkable agreement of outcome [21, 33]. The results indicate that most dynamic structure factors are drive patterns, but some others correspond to socially acquired patterns of attitudes which may be called *sentiments*, e.g., religious, career, patriotic, sports and games, hunting, mechanical, etc.

⁴ We have refrained from using the generic term "habit," for dynamic structures generally, because many psychologists rightly include in habits many purely cognitive patterns and motor skills which are at the service of *any* dynamic structure.

To avoid entanglement in the prolonged verbal, nonoperational disputes about instincts, drives, and propensities, e.g., those of Watson, Murray, and McDougall, the new term *ergs* has been suggested specifically for the patterns found in factoring of motivational traits which do not correspond to any known sociocultural institution (as the sentiments do) and which closely resemble in emotional and goal quality the drives seen in the primates and the higher mammals. Incidentally, Anderson [3], Haverland [63], and others have factored motivation manifestations in the rat and have arrived at similar identification of

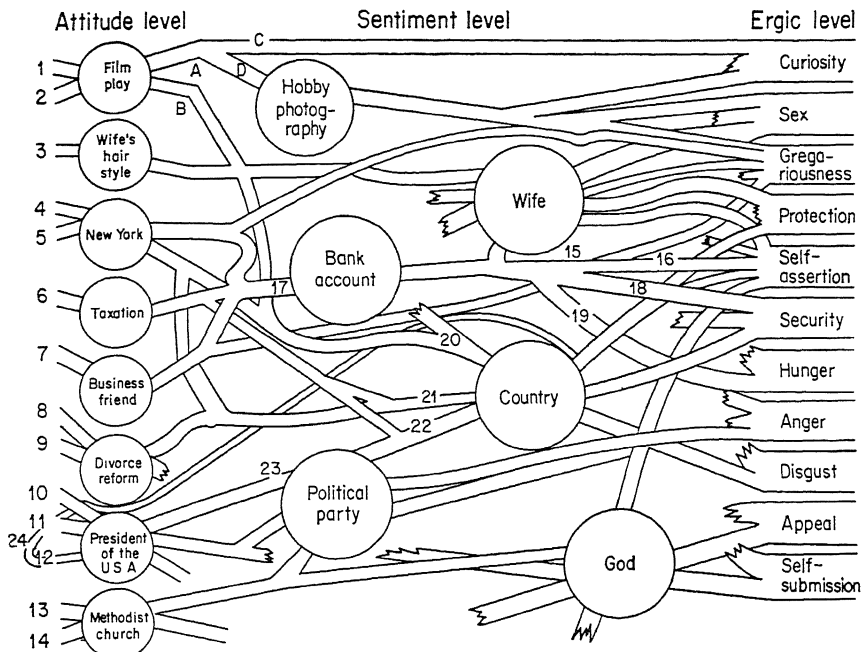


FIG. 2. The dynamic lattice. From [20].

factors with drives. Their work suggests that measures of drive strength in most univariate learning experiments with rats, etc., have been of a low order of accuracy and could be improved by representing the drive by its factor loaded variables instead of a single variable. For example, "period of deprivation of food" does not load the hunger drive factor any better than "degree of restlessness," or as well as "speed of running to previous food goal." Incidentally, in earlier qualitative observations Harlow [61] had already suggested that the experimentalist's faith in hours of deprivation as a good operational measure of hunger strength is ill-founded. The factor studies suggest it has a validity of only about .5 and that a composite battery, with appropriate factor weightings, would give a much improved hunger-tension measure.

The ergic patterns which seem best substantiated in man are sex, self-assertion, escape, fear (or anxiety), parental protectiveness, gregariousness, rest-seeking (sleep), curiosity, exploration, narcissistic sex, appeal, construction (two studies only).

Each of these can be scored from responses on six to twelve attitudes saliently loaded therein—and let it be reiterated that each of these attitude response strengths is itself determined, not from any single verbal assertion, but from some four objective subtests (e.g., GSR, autistic misbelief, word association, information) covering about forty actual response measures.

A valuable check exists in the finding that the same dynamic factors have also appeared in P-technique study [35], wherein a clinical case was tested on the same attitude strengths from day to day for eighty days. Factoring of occasion-to-occasion variance again brought out a simple structure in which such drives as sex, fear, parental protectiveness, etc., appeared. In this case a factor score could be assigned to each erg for each occasion, and comparison of these tension levels with the diary and clinical records showed that the strengths of the drives from day to day can be closely connected with recorded stimulus situations and deprivations, thus providing evidence of the ergic nature independent of that inferred from attitude content.

In both R- and P-technique studies the constituent attitudes were carefully chosen to provide a check on the ergic and sentiment hypotheses. For some pairs were chosen to have a common goal character but quite different sociocultural content and history, whereas others were all concerned with a particular social molding influence, e.g., religion, career, sports, but were clinically considered to exercise very different kinds of drive satisfaction. This design arose from the conception of the *dynamic lattice*, according to which attitudes are organized in learned, environmentally determined subsidiations, forming chains (crisscrossing in lattice formation) from the most recently acquired and culturally complex at the distal (left) boundary to the given, biological consummatory goal activities at the proximal (right) border. The conception of the dynamic lattice is both clinical and experimental in origin. The notion of "subsidiation," derived from Murray [78], arises largely from clinical experience with free association, in which superficial interests are followed to deeper and deeper drive goal interests. But the notion is also rooted in experiments on animal learning, in which behavior Z' , leading to goal Z , is followed by further learning of behavior Y' , leading to goal Y , when the situation is made such that the animal finds it cannot *immediately* start from subgoal Y .

Essentially the dynamic lattice concept is at only a low level of abstraction from the facts—it is an undeniable, almost literal description

of the way dynamic habit systems get organized in any organism that must learn ways to its drive goals. But, with the aid of the hydraulic model, and other specific postulates about its mode of operation, the dynamic lattice develops into a powerful model which has already permitted a range of important hypotheses to be more exactly investigated than hitherto. For example, according to this view of attitude structure, we should expect that all attitudes which subsidiate to a particular ergic goal would wax and wane in strength simultaneously with changes in that goal need, and would thus appear in correlation studies as loaded in a single factor. This involves the assumption also of conditions most quickly defined as "the hydraulic analogy" in which, from rate of flow observed at certain "outlets," the underlying "feed-pipe" connections can be inferred from observed covariations in the rates of flow.

For variations in drive tension level, the above argument is clear enough and works as expected, but the corresponding argument for the appearance of a *sentiment* structure as a factor pattern is beset by more qualifying assumptions. Indeed, the initial failure [21] to find sentiment patterns suggested possible flaws in the argument, but later findings support the main position. If all of a set of n attitudes are involved in a particular sentiment, then it will follow from the unity of the social institution involved that the individual who has most frequent occasion to express himself through one of these will also be in a position more frequently to express himself through the others. If *frequency* of reinforcement has influence on the strength of a habit, then all the attitude-habits socially involved in a single sentiment will tend to be simultaneously weak or simultaneously strong. Consequently we should expect to recognize such connected habits by their being loaded in a single factor. Actually, in the last resort, a distinction must be drawn between two kinds of sentiment structure, namely, sentiment as an *object of intersection* of attitudes and sentiment as a *subgoal*, with somewhat different covariance properties; but this refinement must be left for better discussion elsewhere [28].

The finding that these sentiment patterns have variance much lower than that of the ergs, in some studies, but quite comparable variance in others, can be most intelligibly connected with the type of objective attitude measurement used. When the test measures are predominantly those of the first motivation factor,⁵ notably the defense mechanism and

⁵ For clarity of discussion it is necessary to stress again here the distinction made earlier between the five or more factors found in *motivational measurement devices*, on the one hand—which we shall call *motivation component factors*—and the fifteen or more factors found among attitudes—which we shall call *dynamic structure factors*. The first classification has to do with the more basic sources of energy, whereas the latter deals with general dynamic structure.

autism devices which caused us to identify this first factor with the id component in attitudes, ergs stand out strongly, but sentiments do not. On the other hand, there are at least indications that when the measures are those most highly loaded in the ego component, the sentiment factors become of substantial variance. This is what would be expected, since the id is mainly concerned with desires, whereas the ego is built of habits adjusted to reality and derived largely from social learning. Indeed, several such necessary connections between dynamic factors and motivation factors (see note 5 above) are deducible from the hypotheses and urgently need investigation as a check on the whole notion of cross classification of dynamic manifestations by motivational and dynamic abstraction.

Meanwhile, it is necessary to make those controlled experimental and background-associating checks (in ways suggested in the next section) on the ergic and sentiment interpretations which are so strongly suggested initially by the content and selective patterns of these factors. Recently, Humphreys and Lawrence made possible a first check on the nature of the dynamic factors by correlating them with the general personality factors as measured in Q data (the Sixteen Personality Factor Test). Although a few significant correlations were found, suggesting some real second-order connections between temperament and drive patterns (the sentiment patterns significantly had few associations), yet the results clearly confirmed the general contention that these are "conditional" [14] dynamic factors in a realm new and distinct from that of the established general personality factors.

THE DYNAMIC CALCULUS OF ERGIC STRENGTHS, THE SELF-SENTIMENT, CONFLICT, AND INTEGRATION

The findings outlined in the previous section have opened up what amount to new systems of dynamic concepts and of dynamic calculations which have considerable importance for theoretical developments in psychology, as well as for clinical practice, though it may take some time before the latter is realized. This system, which must be examined in the present section largely in hypothetical terms, because of the scarcity and recency of experiment, includes the following concepts: the dynamic lattice, a dynamic vector calculus for ergs and sentiments, a formula for drive tension, definition and measurement of conflict and integration, analysis of the self-sentiment and its defenses.

As to the dynamic lattice, its initial rationale and sources for fuller development have already been indicated [28]. To summarize briefly: the whole dynamic structure of the individual, conscious and unconscious, can be expressed in this lattice, and the relations of attitudes, sentiment

structures (including the self-sentiment), and ultimate subsidiations to ergic goals can be graphically represented. This lattice representation must also admit feedbacks (or reverberatory circuits). Ergic and sentiment structures therein can be located by factor analysis, and it is probable that other relations therein can be handled systematically by models successful in hydraulics and by the mathematics of lattices and networks. It is further hypothesized that individual differences in certain *general* properties of the lattice as a whole will relate to other personality characteristics; notably, that the amount of long-circuiting (summation of goal distances in a representative sample of attitudes) will determine the individual level of general anxiety [U.I.(T) 24], that the complexity (count of cross connections and feedbacks) will relate to strength of personality integration (factor Q_8), and that the function fluctuation of attitude strengths (occasion to occasion) will correlate (negatively) with the factor of ego strength [U.I.(L) 3]. The truth of the last has been independently shown by Cattell [24] and Das [47].

Turning next to the proposal for a dynamic vector calculus we encounter the following theorems. First, a *vector summation* of the attitudes loaded in an ergic factor will give the tension level of that erg (1) in terms of individual differences, as the general level of need in the individual in the given life-situation, and (2) using occasion measures, for a particular occasion in a given individual. In the former case we cannot tell, from the factor measure alone, how much the ergic tension measure expresses (*a*) a congenitally greater need in that individual for that drive satisfaction, (*b*) a (temporary) greater stimulation of that drive by the particular stimulus situation in the life environment, or (*c*) a level dictated by lesser opportunities for goal satisfaction, with the same amount of stimulation. More completely analyzed, the measurements involved in the hypotheses of ergic tension level may be expressed in the following equation:

$$E = S[C + H + (P - aG)] - bG$$

where E is the ergic tension as measured by the factor score, based on the motivation measurement devices, S is the stimulation given by the existing life situation to the erg, C is a constitutional component in need strength, H is a component from the previous history of exercise of the drive as a whole (including, e.g., any repression of it), P is a physiological condition (temporary) component, G is the extent to which the drive is receiving satisfaction in the general life situation, and a and b are constants representing the effect of the last directly on physiological satiation and on psychological satiation respectively. All this analysis is initially at the level of hypothesis from general psychological observation, but it leads to more precise experimental testing through the new ability

to measure drive tension in man. In verbal terms, the expression $S[C + H + (P - aG)]$ is *drive strength*, i.e., the tension level measured apart from the satisfaction incurred. Further, the part within the square brackets is *need strength*, i.e., the strength apart from stimulation. A recent P-technique study [35], showing that the function fluctuation of E is of about the same order as individual difference variance and that it relates closely to known environmental stimuli, suggests that we shall find the larger part of the variance in E to lie in S . In any case, the possibility now of measuring E , instead of some single variable alleged to represent it, makes a new level of computational accuracy possible in motivation and learning experiments using such a formula. It will be observed that the principal difference from current learning theory formulations is the use of sums instead of products.

As to the calculus of sentiments, the following operations become possible. Since any set of measured attitudes, being themselves vectors, can be added vectorially to a single resultant (as used by the engineer in a polygon of forces), it should be possible, from inspection of all the attitudes in the dynamic lattice gaining satisfaction through a particular object, to calculate the ergic projections of a single vector which will represent the strength (and quality) of the (object-intersection) sentiment as a whole. Suppose now, we accept the preliminary findings that there is no erg of pugnacity, but that the strength of anger-destruction behavior is simply a function of the total strength of the ergs frustrated by the removal of the object of a given sentiment. Then we have a possible experimental check on the above calculation; namely, that the strength of anger-destruction ("aggression") behavior at the threat of removal of the sentiment object concerned should equal the figure calculated for the attitude resultant. Incidentally, the only operational sense that can be given to the "for and against," sociological, habit of talking about attitudes is through considering that the concept really applies to sentiments rather than attitudes. Then "for" represents a balance of satisfactions from the continued existence of the object, whereas "against" means that the various attitudes intersecting in the object (not represented in the lattice) sum to a negative total and would thus give a gain of satisfaction if the object were done away with.

This calculation of the amount and kind of ergic satisfaction in an object has especial value in attempts by social psychologists to anticipate the new equilibrium likely to be reached when one institution is abolished and another substituted. For vector addition of ergic tensions in attitudes can be carried out not only within one individual but, with suitable attention to metric, in any dynamic system, e.g., a set of sentiments in one individual or a single social attitude rooted in many individuals. In particular it has been proposed that calculations on group morale, using

the concept of group synergy [41], be made by obtaining vector resultants of individual attitudes "I want to continue to belong to this group" [16] summed over all the individuals in the group. This direction of development raises many radiating problems of definition which unfortunately cannot be followed up here. For example, it raises the question whether what we measure in ergic tension is the analogue of force or of energy, and whether ergs should properly be given equal weight in the specification equation, as they are by the usual use of standard scores. For the present, until more research is done in genetic and physiological components (C and P) by multiple variance analysis designs [28], the ergic tension breakdown is the least secure of the formulations. We are safer to say that our proof is simply of an erg as a dynamic factor, such that any one of an array of stimuli excites it and any one of an array of responses reduces its excitement. The *form* of these patterns of possible stimulation and response is presumably culturally determined but the *degree* to which a person possesses them is partly genetically determined.

If now, as supposed above, the ergic factors are unities of tension level, whereas the sentiment factors are unities of experience of repeated reward, the adaptation of the factor specification equation to a peculiarly dynamic analysis equation must be expressed by having two kinds of factors, which we can call E , or ergic tension, factors, and M , or engram, factors, where an engram means any kind of empirically, factorially demonstrable unity due to patterns of experience—and therefore covering sentiments (object intersection and subgoal) and complexes, as far as present psychological conceptions go. Thus we have:

$$R_{ij} = s_{je1}E_{1i} + s_{je2}E_{2i} + \dots + s_{jm1}M_{1i} + s_{jm2}M_{2i}$$

(omitting, for simplicity the n th terms and the specifics) where R_{ij} is the magnitude of response in a given attitude situation j of the individual i , and the E 's and M 's are i 's endowments in the ergic tension and engram learning levels. Again it will be observed that, in contrast to some prominent learning theory formulations, this starts out with the simpler assumption of summation rather than multiplication of drive and experience components. But this is not basically important, for summation is an approximation to multiplication, and in neither field is experiment yet exact enough to decide.

Turning now to the fourth concept listed earlier, we come to the derivation, within this dynamic calculus, of formulas for the degree of conflict and of integration (or adjustment) in a particular dynamic system or a person. This development begins with the interesting inductive conclusion that in factoring dynamic variables the loadings, un-

like those found for general personality factors, tend to be predominantly positive. On reflection it is easy to see that this must be so, because no attitudes, sentiments, drives, or individuals could continue without a balance of satisfaction over dissatisfaction. When an attitude (habitual course of action) is negatively loaded on certain ergs and positively on others it means that a necessary adjustment has been reached in which the individual denies himself satisfaction on one drive (by suppression, repression, or any mechanism capable of producing adjustment) in order to gain greater satisfaction on another. Conflict can be regarded either as a transient state of indecision (in which case it is either a conflict of means to an agreed ergic goal or a conflict of ergic goals, as Maier, Brown, and Farber, and especially Mowrer, Masserman, and Maslow have brought out) or as an accepted compromise, in which one erg continues to get satisfaction at the cost of greater or lesser dissatisfaction to another. The present studies [16, 18, 21, 32, 33, 35, 42] in so far as they deal with *settled* attitudes, are concerned with conditions in the second phase of "fixated conflict," rather than the first phase of "active conflict"; but the conflict is not less real because it has ceased to be conscious and the focus of decisions.

Our proposition is, therefore, that fixated conflict in any attitude, erg, or sentiment system is shown by the existence of opposite sign factor loadings. Consequently the amount of conflict, for any of the possible referents (attitude, erg, sentiment, person, group), can be obtained by calculating the amount of cancellation which occurs, i.e., the sum of negative values, or the ratio of this to the arithmetic sum.

The concept of integration or adjustment is honored more in fine phrases than in calculations; when calculations have been made, as by Hartshorne and May [62], Hull [66], McQuitty [73], Das [47], and others, they generally turn out to score something other than dynamic integration, e.g., conformity to the group, central tendency of profile, stability of attitudes, agreement only of self-ideal and self-concept, etc. If we accept the definition of dynamic integration as the *extent to which one dynamic trend does not undo another* [20], in other words, that it is the ratio of total satisfaction to total drive need (considered in a stable situation over a sufficient length of time), then a true calculation of individual dynamic integration is possible. Taking a stratified sample of important life attitudes, and performing a P-technique factorization on the individuals to be compared, we should obtain standard motivation factors for all, but with loadings on the representative attitudes differing for each individual. The expression for adjustment would then be the total algebraic sum of each such person's attitude factor matrix divided by the total arithmetical sum thereon. Questions of metric must be handled, and particularly, the problem of perhaps substituting for that

weighting of the factors given by the latent root values a weighting on some "absolute energy of a drive" concept. However, these refinements do not invalidate the main conception and their treatment must be deferred to a less condensed presentation. The above can be summarized as:

$$C = \frac{\sum s(-)}{\sum s(+)} \quad \text{and} \quad I = 1 - C$$

Where C is conflict, I is integration, and s is factor loading. A test of this drive measure conflict formulation has recently been made by Williams [100] who found the predicted highly significant relation (average $\pm = 0.6$) between C and (1) patient-nonpatient difference, (2) the ego weakness factor, and (3) psychiatric evaluations of conflict.

In the fifth and last concept above, namely, the self-sentiment, we encounter one of the most difficult conceptual problems in the whole of psychology. How does one bring the self and the self-sentiment into the dynamic lattice? Most writers on the self-concept—Sherif and Cantril, Rogers, McDougall, and the psychoanalysts among them—assign to it a powerful dynamic influence in controlling impulses and view it as a comprehensive clearinghouse to which all kinds of behavior systems are referred. To state the conclusion of much discussion, the self-concept must be considered as central in a widely ramifying sentiment which subsidates to almost all satisfactions, but particularly those of security and self-assertion. It does so because foresight concerning the physical, social, and moral preservation of the self is actually a prerequisite for the satisfaction of most other drives and sentiments.

By such reasoning the self-sentiment should appear in the dynamic lattice as a late development, i.e., most distal from the ergic goals, and affecting a wide range of attitudes but especially those directed to social reputation, self-control, and the general preservation of the self. Such a single broad factor, over and above the sentiment factors concerned with career, hobbies, etc., has now been replicated in three independent researches [21, 33, 35]. In the P-technique study [35] it was possible to check that the loadings of attitudes in this sentiment agreed with the emotional values in which the individual (and his self-concept) had been raised. But much remains to be investigated in these terms, and indeed, we are only on the threshold of measurement and calculation and its relation to clinical background data.

In connection with the ego one must also raise the question of what quantitative multivariate research has contributed to the knowledge of *ego defenses*, hitherto based on clinical insights. To an appreciable degree, the extension of motivation study into this area is linked to the meaning of so-called projective tests. As suggested elsewhere [4] "pro-

jective" has been an unfortunate term, implying a definite projection process in a realm of behavior where research has been so poor that no knowledge of the real processes at work has yet been established. In a new approach [18, 28, 42, 98] it has been suggested:

1. That this class of tests be defined as *misperception tests*, indicating that the essential operation is one of measuring the deviation of a perception from a norm, or from a measurable reality.

2. That the assumption that this is the result of a single dynamic tendency, "projection," is wrong. Wenig's factorization [42] of a variety of misperception and defense mechanisms revealed five distinct factors: (a) poor or incorrect cognitive furniture (low "g" and information), (b) naive projection, (c) true projection, (d) fantasy, and (e) autism. Thus some misperception is due to ego-defense dynamisms and some to processes of a different kind, but since we are concerned with misperception phenomena only in so far as they throw light on dynamisms, we shall here follow up only the former. Further research on the same lines might well prove additional defense dynamisms, but for the present we have proof of functional independence (in terms of individual differences) of naive projection, true projection, rationalization, and probably, reaction formation, and identification.

3. Clinicians have seldom stated, still less established, whether misperception should be positively correlated with conscious self-integrated needs or unconscious rejected needs. They generally seem to assume that if a person sees more aggression in a TAT picture he himself has more than average aggression, and the same is vaguely indicated for any other trait. Actually, the foregoing proof of independently acting defense mechanisms shows that they can both reinforce and oppose one another in the direction of the misperception resulting from one and the same given dynamic source in the person. So long as the test is not designed to separate their interfering actions, only a poor correlation could be expected—and only poor correlations are in fact found. When objectively scorable misperception tests are used, the evidence points to a low positive correlation between misperception and *overt* behavior. This can be reconciled with the fact that positive correlations are *also* obtained with unconscious motivation—as shown in the Cattell and Baggaley "id factor" [32]—in varied misperception tests only by the additional *hypothesis of covert-overt proportionality* [4]. This supposes that those persons who, through constitution (or through infantile experience), deviate initially from the norm, will be culturally pressed, in overt behavior, toward the norm *but tend not to reach it*. Thus the internal (repressive-suppressive) adjustment results in the covert, unconscious component deviating on the same side of the norm as does the overt component (not oppositely, as is so frequently and unquestioningly assumed). Paren-

thetically, this seems to apply as much to nondynamic, temperamental dimensions as to drives and is supported by quite independent evidence from genetic studies with the multiple variance analysis method, showing a marked predominance of negative interactions between hereditary and environmental variances [28].

With the general personality dimensions measurable with tolerable reliability, the ergs and engrams measurable by objective test batteries, and the demonstration of location of defense mechanisms by multivariate methods, the stage has at last been set for reliable experimental investigation of fairly complex hypotheses. Already, in the last two years, some very provocative relations have been found in terms of second-order factors among dynamic traits [28], and of significant correlations between the defense mechanisms and certain personality traits [42, 99], as well as relations between the strength of the self-sentiment and of the superego factor and anxiety level.

These correlations and factorings are leading to more exact understanding of the dynamics of the self, but although the general concepts in this area continue to be in approximate agreement with the non-quantitative and necessarily vaguer clinical concepts, it cannot be said that the multivariate methods have yet unearthed anything corresponding to, or positively requiring, the psychoanalytic concepts of conscious and unconscious. The defense mechanism findings just discussed offer fragmentary evidence which would inferentially fit the hypothesis of the unconscious (but also others); and there are manipulative experiments on forgetting, conflict, projection, etc.—few but well known—which point the same way [50, 71, 77, 85, 86, 88]. There is also the evidence interpreted as showing id, ego, and superego motivation components in any attitude [32], as discussed above. But on the whole, the division found between the two second-order factors in motivation components [32] is better described as that between *integrated and reality-tested* systems on the one hand, and *wishful, reality-distorting* (but not necessarily unconscious) systems on the other.

SUMMARY AND SYSTEMATIC ANALYSIS OF THE PRESENT

The development of a neat abstract formulation of concepts and postulates in regard to the theories of personality growing from multivariate quantitative approaches was impossible in the early stages of this presentation because terms did not exist in common language to handle them. With the preceding survey of the empirical findings, of the dependence of constructs upon procedures, and of the unique logical character of the concepts developed in this area, a more compact formulation can now be made. In this summary we shall conclude by relating con-

cepts to the issues posed in the editorial outline, as promised at the beginning, and shall add reference to some additional purely theoretical developments not reached in the survey of experiment.

Of the concepts which have grown uniquely in this field and which the reader needs to keep in mind for the neater formulations, he can be reminded by a glossary of such terms as source and surface trait, simple structure, L, Q, and T data, specification equation, cooperative factor, transcultural factor, pattern similarity coefficient, transposed factor analyses, incremental R technique, P technique, Surgency, ego strength, Parmia, motivation component factors, dynamic structure factors, ergs, engrams, subgoal sentiment, distal end of lattice, ergic tension level, need strength, self-sentiment, naive and true projection, misperception measure, and the law of overt-covert proportionality.

The first part of our more basic summary will simply condense the main exposition, reviewing conclusions in an order which can now be a compromise between that necessary to show historical sequence in research and that desirable in terms of logical dependence and clarity. After a 16-point précis, we shall turn to the editorial discussion outline. We shall then summarize the bearing of the present paper on each of the editorially suggested items, giving special attention to those not already directly treated.

1. Personality research on a quantitative basis proceeds both by the classical univariate controlled experiment of the older sciences and by the multivariate analysis designs which have been developed in the life sciences; but at the present phase of personality research, good strategy would give the latter much larger scope to define the functional unities with which controlled experiment may best concern itself.

2. The definition of the factor analytic model merely *begins* with the matrix transformation theorems, i.e., reduction of many vectors to few coordinates, as known to the mathematician. Its scientific use and rationale involve many more restrictive conditions and more complex ideas, notably, use of statistical criteria of unique rotation, planned and statistically tested matching in cross validations from research to research, coordinated experiment to examine the degree of scientific "efficacy" of factors in R, P, and R-incremental designs, and the checking and further interpretation of factors through their use in controlled experiment. However, all uses have in common the aim of finding *naturally occurring*, underlying functional unities in stimulus-response variables. Underlying unities of pattern, too complex to be perceived by unaided clinical or univariate experiment, thus constitute the "intervening variables" around which concepts can profitably be developed, and upon the interactions among which more basic laws can be hopefully built than upon innumerable empirical paired-variable relations.

The specification equation, which is the central theme of the model, has the following basic form:

$$R_{ji} = s_{j1}F_{1i} = s_{j2}F_{2i} + \dots + s_{jn}F_{ni} + s_jF_{ji} \quad (1)$$

and defines the stimulus situation j for the response R by a pattern of "situational indices" or dimensions, s_{j1} , s_{j2} , etc., and the organism also by a set of dimensions, F_1 , F_2 , etc. It is thus a more developed stimulus-response formulation.

3. A distinction must be drawn between the concept of the *factor pattern*, by which factors are first recognized, and that of the underlying *source trait*, which is expected to manifest itself in modified patterns according to sampling and other transformations. The interpretation of factors is a hypothetico-deductive spiral employing both factor analysis (contrasting the nature of variables with high and low loadings) and controlled experiment; but multivariate experimental designs exist which permit the same sequential, causal inference as in manipulative univariate experiment, and with higher powers of definition.

4. Factors may be classified (*a*) according to the three exclusive sources of observation, as L-, Q-, or T-*data* factors, (*b*) according to *modality* of variables, as ability, temperament, and dynamic factors, and (*c*) according to *density* of variable representation as first-, second-, and higher-order factors. A sampling of behavior space is implied in the concept of the personality sphere.

5. At present there is acceptable replication and confirmation of about 20 ability factors, 30 general personality factors, and about 15 dynamic factors. In personality manifestations it is assumed that the same real dimension will express itself in all three media, but so far, research has not succeeded in finding many factors crossing all media. Cross-media checking cannot be carried out by the "s" index [31] because of absence of common markers but must be by direct correlation or transformation analysis. Some five second-order factors have been found in each realm, and some of these, such as the anxiety factor among L- and Q-data factors, and inhibition factor among dynamic traits, give substance to concepts long appearing as clinical hypotheses.

6. The model of the specification equation assumes linearity of variables to factors and additive relation among factors with respect to the criterion, i.e., no "joint functional relationships" in the mathematicians' definition. Demonstrable absence of fit is rare, but certainly exists. However, in artificial examples it has been shown that factor analysis is able to yield the correct factors, but with linear approximations to more complex relations, when complex relations do exist. The model admits the possibility of different individuals' obtaining the same score in dif-

ferent ways, and in general fits psychological conceptions of the personality as an integrated set of traits.

7. In the absence of computing methods to fit the speculative models of Coombs and Satter [43], or of parametric analysis of the predictive device of Lubin and Osburn [69, 70], or to accommodate to the whole range of possible mathematical functions, the best approach is to isolate factors by the present model but to determine the curves of their relation to various dependent variables by controlled experiment. Thus, one can arrive at more complex functions than the simple specification equation, in the rather uncommon instances where the latter breaks down.

So-called configural scoring is but a special case of predicting criteria through a general mathematical function of elements, i.e., use of the developed specification equation. But types, as pattern modes, are of two kinds—special purpose, based on distribution of a complex function, and general purpose, based on Q' technique using the pattern similarity coefficient. Trait and type approaches are face and obverse of the same method, however, and are best used in conjunction, types being defined as modal patterns in profiles based on factors as elements.

8. Elementary dynamic variables, defined as attitudes, have been found to factor, i.e., to give reproducible simple structure patterns, as readily as ability and other modalities used earlier, and indeed, to be particularly responsive to P-technique designs. Reasoning from the nature of the experimental design, and the relation of discovered factors to stimuli, etc., strongly suggests that the fifteen or so replicable patterns found are those of drives, specifically defined here as nine *ergs*, and some six *engram* (mainly sentiment) patterns. Engrams are learned patterns, resident in memory. The measured strength of such a pattern in an individual corresponds to the degree of exposure (frequency-reward learning) to the social institutions through which the component attitude-habits are learned. The specification equation for an individual attitude, which resolves a symptom into dynamic factors, amounts to a quantitative "psychoanalysis" of motive.

9. An alternative analytic split of dynamic data can be considered, in which the forms of manifestation of motive (for any attitude) such as misperception ("projection"), ego defenses, learning, attention, physiological and autonomic response are factored for a single attitude. This constitutes a complementary or reciprocal treatment to 8, for instead of factoring a single operational manifestation of many attitudes it factors many motivational manifestations of a single attitude. It has yielded some five ego-defense mechanisms and four or five "motivation component" factors, which seem to correspond to id, ego, and superego interest components, present in every attitude. Although these are "cross factorizations," certain systematic relations would be expected between

the motivational level factors and the dynamic structure factors, notably that the "ego component" (realistic and realized habit expression of an interest) should be stronger in engrams than in ergs.

10. The dynamic lattice of subsidiated attitudes, which is a construct almost at the descriptive level, together with the hydraulic model used to make predictions in it, leads to positive designs for analyzing dynamic structure both by multivariate and manipulative univariate experiment. As indicated in 9 the multivariate method has successfully abstracted from the lattice both ergic structures and engram structures. Consequently, the typical expression for the strength of interest in the course of action defined by an attitude is a weighted sum of ergic tension levels and engram (sentiment, or Freudian "complex," experiential) components, as follows:

$$(I_j \text{ or})^6 R_j = s_{je1}E_1 + \dots + s_{jen}E_n + \dots + s_{jm1}M_1 + \dots + s_{jmn}M_n + s_jM_j \quad (2)$$

where E and M are respectively erg and engram factor scores and M_j is an engram absolutely specific to the given attitude.

An attitude is thus a vector, amenable to vector summation to get the ergic and engram composition of any dynamic system in an individual, or in group phenomena, from the interaction of many individuals.

By hypothesis the ergic tension factor levels can be broken down as follows:

$$E = S[C + H + (P - aG)] - bG \quad (3)$$

i.e., drive strength $S[C + H + (P - aG)]$, need strength $[C + H + (P - aG)]$, situational stimulation level S , satisfaction or satiation levels $aG + bG$, constitutional and historical components C and H in the need strength itself, as well as a physiologically manipulable physiological component in need strength P . The relations of these formulations to learning theory formulations have been commented upon. The principal differences are use of additive instead of product relations, and the splitting of both drive and reinforcement experience components into a pattern of dimensions instead of a single term. The stimulus situation is also expressed as a pattern of dimensions.

11. Central in the notion of total dynamic personality structure is the empirically demonstrated *self-sentiment structure*, as well as the super-ego and certain independently functioning defense mechanisms. These agree approximately with the clinical concepts but take on more definite

⁶ I_j or strength of interest in a course of action, if the response is inhibited, but R_j if we literally measure the magnitude of response in the given course of action.

properties and implications. For example, the self-sentiment is distal in the dynamic lattice and is correlated significantly with certain drive strengths and general personality dimensions. Further, the two second-order factors in motivation components suggest, instead of the clinically prominent division into conscious and unconscious, a division into integrated, reality-oriented dynamic systems and wishful, unrealized, not-reality-tested but not wholly unconscious systems.

12. The notion of ergic vector projections permits a dynamic calculus of interest investments and conflict, by R technique for the average man or P technique for a particular clinical case. Fixated, if not active, conflict can then be measured as the sum of negative projections in a dynamic system, divided by the sum of positive projections. A meaningful value, $I = 1 - C$, can be derived for I , the adjustment (integration) of an individual, where C is the conflict index, equal to $\Sigma s(-) / \Sigma s(+)$, the s 's being ergic projections for a stratified sample of important everyday life attitudes. This index has been shown to be substantially correlated with clinical ratings of adjustment and with patient-nonpatient differences [100].

Conceivably, by applying the calculus of interest strength in attitudes, as in Eq. (2) above, one can give a meaning to psychological energy, through multiplying this force (interest strength I_j) by a measure of distance achieved toward a goal. This speculative notion is introduced to indicate that a considerable possibility of further theoretical development resides in the present formulations.

13. Each use of the factor analytic specification equation and the included source trait measurements, in personality, supposes only that the relation holds *at the given moment*, in terms of the factor measures also taken at the moment. However, the approach through factors (rather than specific tests, etc., as in most current applied psychology) implies, and opens up, the experimental possibility of supplementing statistical prediction (using only strictly factor analytic, actuarial, "instantaneous" estimates) by use of general psychological laws dealing with the expected change of factors and stimulus situations over time. Indeed, with the replicable factors now available, it at last becomes possible to proceed to these laws of growth, learning, physiological determination, etc., of factor strength which will integrate present personality theory with other general psychological laws. Similarly, the definition of stimulus situations by situational indices, i.e., by assigning scores of common psychological dimensions to all stimulus situations, opens up possibilities of generalization about stimulus situations which should make possible a psychophysical calculus permitting extension of behavioral prediction to situations that have not yet actually been used to determine a specification equation.

14. The metric of factors, like that of most other psychological scales, is essentially ordinal. However, because of the fact that many component subtest measures enter into any factor scale there is a better argument than usual for the assumption that the distribution is a Gaussian one and that equal intervals might be found by cutting according to units giving a normal distribution. In the case of P technique, and of interest measurement, it has been proposed [28] that ipsative rather than normative scoring should be used, and most have agreed with this.

Whatever the *nature* (not the metric) of the units for the single factors—in a sense analogous to the dimensionality of the units of physics—the dimensionality of the units of the variables predicted by the specification equation must be multiple. For we add at least three modalities—abilities, temperament traits, and dynamic traits (the last being dual)—when, after factor analysis, we put the individual together again in the single emergent behavior defined by the specification equation.

15. Although the factor analytic model has been considered by most psychologists only as a means of measuring individuals (as to their traits or states), it should theoretically be equally important in defining and measuring *stimulus situations*, their dimensions and changes. (In terms of a vector for each situation $S = s_1, s_2, s_3, \dots, s_n$.) The definition is not physical but psychological, i.e., in terms of the behavior of the species of organism reacting to the physical world. The relation of the psychological valences of a situation, thus factor analytically determined, to the physical properties constitutes a considerable new area of possible development of psychophysics, beyond the purely cognitive psychophysics of the Weber-Fechner tradition, into an affective-dynamic psychophysics peculiar to each culture and every species of organism.

16. There exist certain forms of behavior and of temporary change of behavior, notably that under the sociological conception of “adopting a role,” but also under “mood change,” which in terms of the model could be expressed in either of two ways: (a) by changing the situational indices, the *s*'s, corresponding to the verbal equivalent that “the person perceives the situations differently” or “the situation has changed its meaning”; (b) by changing the quantitative terms for the individual's personality (the *T*'s, or *E*'s or *M*'s), but by some formulation which indicates that it is a temporary phase.

The latter seems preferable, for much current talk about the “new look in perception” makes the basic theoretical error of introducing unnecessary terms. Instead of introducing “change of perceptual meaning in the situation” as a middle term, one may simply say that, in a role, the behavior changes and that the terms for personality have changed. Of course, in some kinds of perceptual change it may actually be more

economical, for a number of predictions, to introduce a term corresponding to the introspective experience of a change in perception, and this alternative we have considered under *a* below, illustrated in the case of a role change producing a change of perception. In any case, in role change and most other changes of perception, it does not suffice to depend on introspection, as sociologists and psychologists in perception have done, for knowing when the change exists. It must be inferred from a change of pattern in behavior. This is best determined, at least in the case of roles, by applying the profile similarity coefficient, r_p , to the behavior of many people in many situations, using Q' technique [23, 28].

Granted the demonstration of a role change, it can be formulated in our model as stated:

- a.* As a perceptual change, through introducing a second type of situational index S_r multiplying or otherwise modifying the usual nonrole s 's, in such a way that the new response is accounted for, thus:

$$R_{jri} = S_{rj}s_{j1}T_{1i} + \dots + S_{rj}s_{jn}T_{ni} \quad (4)$$

the S_r being determined by factoring role behavior.

- b.* As a temporary change in personality, definable for the role by introducing a profile of trait modifiers, t_{r1} to t_{rn} , which can be applied as a grid, the same for all people, as they step into the role, thus:

$$R_{jri} = s_{j1}(t_{r1})T_{1i} + \dots + s_{jn}(t_{rn})T_{ni} \quad (5)$$

Incidentally, 4 should remind the reader that our general formulation of situational indices is such that every stimulus situation is really conceived as a Chinese "nest of boxes," situation within situation, which for initial simplicity, we divide into an immediate situation s and a life situation S .

Let us now examine the series of 12 editorial rubrics to see where supplementation of the previous 16 points is necessary.

Point {1}, Background Factors and Orienting Attitudes, was dealt with at the beginning. On point {2}, the Structure of the System, in terms of independent, intervening, and dependent variables, we had to run off the rails of traditional analysis, because factors do not fit into this univariate scheme but require other modes of thinking. Unless time sequences are introduced into factor analytic experiment, only incomplete inferences are possible as to whether the factor is a dependent or an independent variable. But in either role it has the advantage that it directly represents a *systematic* rather than an *empirical* variable, i.e., it permits direct and sure reference of the dependent-independent rela-

tionship to concepts or constructs, more certainly than in nonpositivist approaches, and at the same time gives comprehensive information about the intervening variable that is missing in positivist approaches. This essential point will be evident from considering summary 2 above and the earlier, more detailed exposition which it covers.

Editorial point {3}, the Initial Evidential Grounds for Considering the Formulation Promising, is, in the case of this system, scattered widely over psychology, in the success in relating test variables to ability concepts in education, etc., and in the various empirical findings systematically organized here.

As to point {4}, The Construction of Function Forms, it has been seen that the specification is initially restricted to linear relationships between variable and variable, and factor and variable, but that the approximation, when nonlinear, is usually good enough to permit factors to emerge, after which all manner of complex functions and factor profile derivatives can be determined by controlled experiment. Practically all the theoretically assumed mathematical function relationships in ordinary R technique, in incremental R technique, in condition-response factoring, in P technique, in factor matching, etc., have been tried out experimentally and found to check with the formulations as well as, or better than, those in any other area of mathematical formulation in psychology. On the other hand, the more recent formulations for change of perception of a situation, change of personality in a role, the origin of ergic tension, and the measurement of internal conflict have not been checked to anything like the same level of certainty. Point {5}, on Mensuration Procedures, is handled by 14 in our summary above.

Editorial point {6}, concerning the Formal Organization of the System, is answered in practically every page of our presentation. It will by now be realized that this system has a high degree of formal organization. Yet one should note that in the main, such organization has not been arrived at by clearly formulating axioms or postulates and proceeding to check inferences. Instead, there has been much groping and intuition, and especially, the formulation of limited-scope subsystems to fit particular areas, before attempting any more general or "grandiose" postulates. The main ultimate axioms are (a) personality or the totality of behavior can be analyzed into a number of functional unities or factors, (b) that these interact additively (in the first approximation) to produce the degree of behavior observed, (c) hierarchies can be found among these factors such that each primary affects only a limited area of behavior, but higher-order factors organize several primaries. This implies that factors are not uncorrelated but demonstrate their independence by influencing independent sets of variables.

Point {7}, the Scope or Range of Application, has been sufficiently

illustrated, as also has {8}, the History of the System to Date in Mediating Research. Potentially the scope includes the investigation and explanation (at a certain level) of all multivariate phenomena, psychological and nonpsychological, e.g., meteorological, biological, physiological. For although factor analysis and related multivariate methods were born in psychology, they belong to the life sciences and social sciences generally, constituting a second main approach not needed in the simpler realm of the physical sciences. In the last five years there has been a remarkable growth of factor analytic findings in physiology, biology, economics, sociology, and anthropology. For example, Sokal, Stroud, and others have also used it in the complex taxonomic problems of entomology, Damarin in physiology, and Driver in anthropological culture-pattern study. These extensions are to be welcomed by the psychologist, for there are statistical and logical problems in the method that will be far more readily solved when factors receive wider scientific exemplification and when diverse statistical developments from these new sources are integrated with it.

Within psychology the chief interrelations, present and potential, are with learning theory, whenever learning theory begins to deal with motivation effects in a more positive and detailed fashion, and especially when it progresses from means-end learning to integration learning. Secondly, there are models developed from the present system, but set out elsewhere [28, 41], which deal with matters of increasing importance to social psychology; viz., the degree of constancy of personality factors across cultures, the theory of common scales where factor patterns are not identical, the means of relating personality measurements to measurements of the behavior of groups per se [41], and the dimensions of culture patterns.

Thirdly, there are major possibilities in relation to genetics and physiology. In the latter it has been shown that autonomic and stress states can be factor analytically identified and integrated into a total "trait and state" formulation of individual behavior [28]. In the former there have been explorations, independently by W. Thompson and the present writer, of the possibility of understanding gene structure more specifically by combinations of factor analysis and existing biometric genetics.

Concerning editorial point {9}, the Extent of Supporting Evidence and of Evidence Embarrassing to the System, the chief instances of the latter arise from (a) occasional evidence of curvilinearity, the handling of which, experimentally and in terms of slight modification of the model, has already been discussed, (b) a few clear instances of the "permissive relation," in which one factor refuses to come into operation *at all* until another reaches a certain level, (c) the state of confusion which still

persists in the "intermedia" realm [40] after the initial discovery that the same personality factors do not neatly crop out simultaneously in all three media. (c) is partly resolved (1) by finding that first orders (primaries) in one medium are, in at least four instances, second orders in another, e.g., in the case of anxiety and *exvia-invia*, and (2) by the good line-up found in general between rating and questionnaire media. Only the problem of separating "instrument factors," and of pursuing further questionnaire-objective test matchings, still remains. What concerns incompleteness of model and method, rather than sheer incompatibility of evidence, will, however, be dealt with separately below.

The Applicability of the System to Areas beyond That in Which It Has Been Used, editorial point {10}, has been indicated for areas outside psychology under editorial point {7} above, but its concepts and principles have frequently been urged as powerful aids in psychological contexts other than personality theory:

1. In determining the dimensions of syntality in groups, as a preliminary to relating them to structural arrangements and population means.

2. In clinical psychology, the factor analysis of the individual case by P technique could give more positive understanding of the individual dynamic lattice and lead to more precise dynamic laws.

3. In learning experiments, it is contended that there would be a better chance of hitting upon laws of a systematic nature if drive strengths on the one hand, and learning effects on the other, were measured in terms of factors instead of single empirical variables.

4. Accepting an important dichotomy of psychological research as *process-centered* (e.g., perception, learning) vs. *organism-centered* (e.g., personality), our model and method have so far applied largely to the latter, but untouched applications exist in the former. The applications would consist principally, (a) of using profile similarity statistics (e.g., [17]; see under The Conceptual Status and Interpretation of Factors above) to identify "types" of unitary process, i.e., to recognize the independent processes to be studied in a taxonomy of process, and (b) of using P technique (with lead-and-lag correlations), incremental R technique, or even simple R technique at different process stages (as in the learning studies of Fleischman) to throw light on the phases and developmental patterns of psychological processes. In short there are structural and taxonomic methods and concepts implicit in multivariate experimental design which transcend the present local psychological theories and have wide, permanent, and "philosophical" applicability.

The important editorial question {11}: in effect, To What Degree Has Research in the Area Taken on a Programmatic Form? has been answered at many points as we proceeded. It will be seen that in the

central area a very high degree of planful programming has been undertaken and largely realized. The Thurstones' work in abilities had the best qualities of research "vision" and of steady persistence in step after step necessary to link findings in a coherent body of theory and checked fact. The work of Guilford and his associates, on creative ability, has had this same monumental quality, though not so far advanced; whereas Eysenck and the London group have followed a visible, broad pattern, even if at times moving too fast to check, consolidate, and improve factor techniques per se. Again, the present writer's laboratory started over twelve years ago a program of simultaneous factoring in the three media of personality observation—behavior *in situ* (rating), questionnaire, and objective, situational tests. This involved cross-media factoring, developmental studies factoring at four different age levels, checking of functional unities found in R technique by P technique, determining of obliquities accurately enough to explore second-order factor relations, and improving statistical significance tests for the various modes of checking hypotheses used in these new realms.

All these aspects of the program have been brought to fruition in some degree of empirical research. Yet among the tragedies of current research organization we find, first, that foundations have not realized the administrative demands, and demands on computing resources, necessary if multivariate research programs are to reach effectiveness, and secondly, that the centers in which such research proceeds can be counted (in the world) on the fingers of one hand. Hence the scientific peer groups necessary to check, criticize, and disseminate knowledge among graduate students lag far behind the number desirable for development of a healthy convergence and articulation in a scientific system. In spite of this, and to a degree unrealized in a depressing number of university teaching departments, there has been achieved a considerable fraction of the programmatic research necessary for examining the integration of these concepts and for articulating different empirical domains.

The final editorial category for systematic presentation—a Look to the Future in Terms of Long-range Strategy of Development—has also been considered at each stage of our exposition, but some quite specific summaries of more urgent research needs may now conclude this essay. In the first place, in a strategy which envisages advance through work in far more departments than happen to be equipped with electronic computers and advanced statistical arts, it is necessary to *supply* good factor measures, that can confidently be employed anywhere in univariate research, involving factors but not factor analysis. At present, the O-A batteries [28] are the best available stopgap, but there is crying need for factor confirmation and intensification, i.e., for development

of more factor saturated tests, as Sells proposes, for all eighteen general personality factors (in objective tests), and as carried out for one factor, recently, by Scheier (the anxiety factor, U.I. 24). Although the present writer, from his immersion in the field of objective personality test development, would judge that there is evidence, beyond that summarizable in statistical P values, for confidence in the 18 dimensions stated, many psychologists adopt a scepticism ultimately based on a belief that nothing so tenuous as a mathematical factor can be psychologically real. It would be advantageous to psychology if such skeptics would go explicitly on record with alternative statements, expressing their beliefs on the form of the psychological patterns concerned, and set to work on checking studies. For much valuable manipulative univariate experimental work could proceed with the advent of more saturated, if not more definite, measurement of these factors. Unfortunately ratings and, to a lesser degree, questionnaire factors are less widely experimentally applicable and their relations to the objective test factors, in half the cases, remain to be discovered.

One of the first needed experimental clarifications is a sorting of factors into those of environmental and those of genetic origin. As argued in more detail elsewhere [28], a great saving of research effort would result if, initially, research were directed to this end as soon as factors are validly measurable. For there is no point in entering upon any explanation of a pattern in terms of rival learning theories if *no* learning theory is going to account for the pattern. The recent genetic studies of Beloff, Blewett, Cattell, Eysenck, Prell, and Stice [see summary in 28] already indicate that most of the variance in intelligence, schizothymia, and *Parmia vs. Threchia* is hereditary, whereas most of that in *Surgency vs. Desurgency*, *Premisia vs. Harria*, and *superego strength* is environmental.

Secondly, more extensive and intensive studies are needed of the natural life course of factors. This involves both factorings at different cross-sectional levels to determine changes of *pattern* with age, as in the work of Coan (see [28]), Gruen [37, 38], Hofstaetter [64], Peterson (see [28]), Thurstone [90, 92], Woodrow [101]—and theoretically in Ferguson [52] and Ahmavaara [1]—which has shown both continuity and significant trend, and simple measures on the age trends of the single scores for personality factors, such as have hitherto been made only for intelligence. The initial results already show them to have very distinctive life courses [28]. For example, in our culture, *Surgency* declines steeply between twenty and thirty years, whereas *ego strength* rises steadily through most of the life course. Both these developments would help check on hypotheses about the factors *per se* and also help enrich the conceptions about factor structures in general.

Perhaps the greatest discrepancy between the pioneer promise of the system and the necessary massive research reinforcement lies in the area of dynamic structuring and the calculus of conflict. The possibilities for putting most existing kinds of clinical hypothesis to a quantitative test (after rephrasing in accordance with the more objective structural indications) are obvious and manifold—and neglected. The implications for motivational research, however, are very relevant also in the experimental and physiological study of human and animal motive, which could benefit from more reliable measurement of drive strengths, permitting the emergence of laws of learning and conflict, at present drowned in the huge error variance (and lost also in the conceptual vagueness of experimental measurement, where it concerns motivation strength).

No intelligent view of current research strategy can overlook the necessity for enlisting the help of mathematical statisticians in solving and anticipating the problems which arise in this field as the use of the model and the exactitude of its experimental testing increase. "Anticipating" is a superfluous luxury at the moment, for there is still a backlog of problems either unsolved or insufficiently solved. In particular it is necessary to discover a statistical test for completeness of factor extraction, to make an improvement on Bargmann's test [7] of significance of simple structure, to solve the parallel proportional profiles equations for factor rotation for the oblique case [29], to develop a parametric test, beyond the Cattell-Baggaley s index [28], for deciding when one factor study confirms another, and to improve on the Oblimax analytical rotation to simple structure, now programmed for computers, owing to the excellent work of Saunders and Dickman, but still imperfect. In this part of the program the mathematician must be persuaded to help the psychologist to psychological goals, not to impose rigid but inappropriate mathematical perfection, as was done with orthogonal axes. A word on models below will enlarge on this.

A realistic summary of research strategy must include graduate teaching. It is an unfortunate fact, seldom commented upon, that the demand in various large-scale government and industrial concerns for Ph.D.'s competent in multivariate methods has been so great that the normal supply of personnel to university teaching posts has been cut off. A vicious circle is thus set up in universities in which the area is insufficiently taught and a still more insufficient supply of teachers is generated to teach it.

Although the multivariate analytical approach, after half a century of rapid growth, is at the point of presenting psychologists with the first definite set of quantitative findings on personality and motivation structure, based on a rich harvest of concepts which are susceptible to

accurate experimental checks, the checks are being made all too slowly and inadequately. The possible supersession of rough clinical notions by replicable experimental evidence on dynamic structure could generate a new phase in clinical psychology and make possible a wide surge of manipulative experiment on the genetic, physiological, and learning laws governing causes and consequences of this personality structure—experiment which could not before be profitably undertaken. The incompleteness of the work on factor analytic structuring itself thus continues to present a “bottleneck” in the flow of objective personality research.

Doubtless as more universities recognize the basic logical connection of multivariate and clinical research, clinical resources will help to solve the present research supply problem by training a new type of researcher in clinical and personality areas, simultaneously competent in clinical observation and multivariate experimental designs and calculations. Meanwhile few such graduate students are produced, and fewer surmount the temptation of the market place, so that such adequately trained persons constitute perhaps not one in ten of the people struggling with the complex problems of clinical and personality research. The clinical practice area will continue to be the largest source of good data for personality research. Consequently, any practical forecast of how soon one may expect the predicted broad development of new laws based on structured personality measurement, rising beyond the level of the present clinical, and general, nonquantitative theory (based on perception by the naked eye alone), depends on two matters outside science per se, and difficult to estimate, namely: (1) the amount of lag in teaching departments in switching to new training goals for clinicians, counsellors, and other major sources of researchers in personality, and (2) the growth of the social and economic organization of a more complex type of research institution, involving coordination of clinics, electronic computers, centers for basic personality research, machinery for assembling larger samples of subjects (from more comparable and controlled populations), research committees to appraise factor matching and factor batteries, etc. This larger organization becomes a necessity for effective work in multivariate research (though never really needed in univariate work) because there is a far greater tendency for any apparently local problem in multivariate research actually to involve the whole area, i.e., to involve simultaneously many variables and many types of subject.

To the editorial request that each contributor conclude by extending reference to “Barriers Blocking *General* Theoretical Advance in Psychology” the present writer would add, in addition to the difficulties in the socioeconomic organization of research and in graduate in-

struction, the psychologist's own preoccupation with theory. It needs no very piercing clinical eye to see that psychology suffers from a sense of inferiority—as it compares its theoretical development with that of the physical sciences. A fair number of psychologists have reacted to this by importing pretentious theories from outside, though they have no natural organic relation to psychological data. Some grotesque productions have ensued; these merely distract psychology students who are interested in theory (not too numerous, incidentally) from the theory that is growing more modestly out of local laws and newly observed, peculiar regularities in quantitative psychological research data.

A slight regression of psychology toward reattachment to its step-mother, philosophy, is evident in the recent profusion of mathematical models. Like any other machine, the model must be the servant of the psychologist, not his master. Speculation on models far beyond any psychological exemplification or means of checking by data is no service to psychological theory. The playground for indulging mathematical models is infinite, but as with trial and error in biological mutations, the percentage proving adapted to reality will be very small. Consequently, the theorist should exercise some choice from the start, in terms of adopting models suggested by psychological data, rather than those appealing to the mathematician. The oblique factor analytic model, central in the present psychological theory, would quickly have died of neglect if left to most mathematical statisticians, for it is beset with unpleasantly unsolvable issues, with assumptions which are often not exactly met, and with some properties as irrational as π . But the psychologist has developed it because he is an observer and a scientist first, and only secondarily a mathematical theorist.

APPENDIX: THE CONCEPTS OF VARIABLE DENSITY AND FACTOR ORDER

For most of the more specialized, complex theoretical or statistical developments touching the above outline of the field the reader can be, and has been, referred to publications elsewhere. Since at the moment of writing no source is available to deal with one vital specialized issue—variable sampling—it seems best to handle that here. The notions of *a total available population of variables*, and of *a means of taking a stratified sample* of it, underlie the concept of the *personality sphere* discussed earlier and are necessary to a true rationale for distinguishing *first- and higher-order factors* [28].

It is an assumption of factor analysis that no factor affects all variables of behavior and that boundaries can, therefore, be drawn in some conceptualized "behavioral space" (or area of variables) delimit-

ing the realm of action of any factor. This assumption is primarily involved in simple structure, but it is also involved, for example, in Guttman's "radex" theory of factor structure, and it also certainly seems to fit any empirically obtained factors, by practically any system of rotation.

First-order factors necessarily affect larger areas than specific factors and second-order factors than first-order. Thus in Fig. 3 the interrupted-

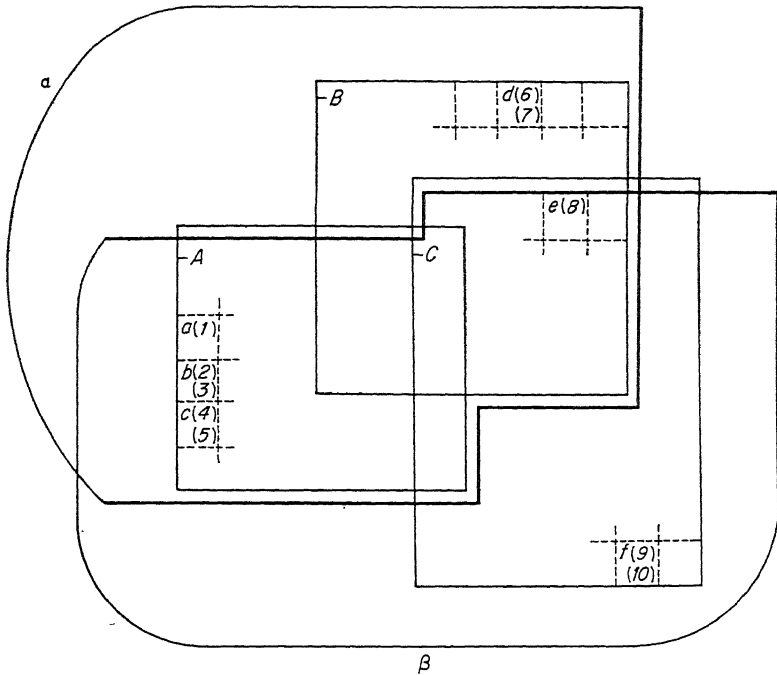


FIG. 3. Emergence of factors in relation to choice of variables. *After Cattell.*

line squares can be considered to represent specific factors, *a, b, c, d, e*, the continuous-line areas the first-order factors, *A, B*, and *C*, and the heavy continuous the second-order factors, Alpha and Beta.

Now it will be seen that if we factor variables 1, 2, 4, 6, 8, and 9, we shall first obtain primary factors *A, B*, and *C* (*B* and *C* both loading on 8) and specifics *a, b, c, d, e*, and *f*. On factoring *A* and *B* and *C* we shall obtain (providing there are enough other primaries to complete the definition) two second-order factors, Alpha and Beta. If, on the other hand, we had happened to begin with variables 1, 8, and 9 (plus such others analogously placed as are necessary to define two

factors), we should have reached factors Alpha and Beta directly, without realizing they are second order. Conversely, if it were possible, as it often is, to get two variables so similar as in the pairs 2 and 3, 4 and 5, 6 and 7, and 9 and 10, without actually being identical, our factoring would have given the specifics b , c , d , and f as first-order common factors.

Now, in the absence of any workable concept of "density of variable sampling," it certainly happens in research that we sometimes change scale in this way without knowing it. On the other hand, it is unlikely that we do frequently, and the most probable state of affairs is that we mix first- and second-order factors (or primary and specific, but pseudo-common) in a single research. For example, if one started a factorization with variables 1, 2, 3, 7, and 9 in the above diagram, factor A would be found by its loading of 1, 2, and 3. But unless 1, 2, or 3 contained some variance in B and C (which by definition they do not) factors B and C would not appear, since there is only one representative of each. On the other hand, the second-order factors Alpha and Beta would appear, to the confusion of matching efforts in the primary realm.

Much of the difficulty and disagreement in present research findings is probably owing to this source—quite apart from the related effect, as pointed out by Ahmavaara [1], that primary factors found at lower population ages may "fine out" into two or more distinct factors at higher ages. Careful records of factors found with different sets of variables, finally put together with every possible cross comparison, might do something to solve this difficulty. But no one has succeeded in so unraveling the tangle, and it is more likely that we shall succeed better by getting an independent concept of "density of representation" of variables, fixing this at a definite figure for all researches to be integrated. This concept implies the notions of both (a) a total population or area of possible variables and (b) the distance apart of any two variables.

Now either the personality sphere [14, 23] or the time sampling of human behavior [28] can give us the former, but where do we get the latter? The familiar notion of the distance apart of two variables in factor analytic space will not help, for this is a dependent value, and we have to compare this with the new, *independent* concept of distance, to check on whether our factor analysis is right. Further, the notion of a total area of human behavior is almost certainly going to depend on the assumption that items of behavior gathered according to a certain operational procedure are equidistant from one another. If so this vital difference will exist between the total sampled variable space and the

resultant factor analytic space: that whereas the variables are spaced with equal density throughout the former, they are very definitely concentrated largely in hyperplanes in the latter.

The search for a concept of degree of similarity (or distance) of variables must thus proceed outside, and independently of, the factor analytic model. The possible alternatives are a functional or a phenomenological basis. By the former is meant any system of classifying situations and responses which depends on the functional relations of these situations to man, i.e., on their relations to human functioning. By the latter is meant a classification supposedly depending on the real properties of the objects, detached as far as possible from human perception.

A number of functional classifications of variables according to similarity, which do not directly depend on correlational and factor analytic measures, can be suggested. For example, the likeness of situations might be measured in terms of the amount of learning transfer from one to another. Another possibility would be in terms of a threshold of discrimination in perception of these situations. For example, if as far as a person can *see*, two tests are really demanding the same thing of him, then we should not consider them one jnd apart. Or again, we might take the degree to which fatigue transfers from one type of reaction to another. Or yet again, we might take the frequency with which interest in one activity can be substituted stably for interest in the other.

There are undiscussed difficulties in most of these, such as the fact that the variables are not defined by stimulus-response but require also reference to the mode of scoring. But a more general objection is that *any* functional classification might prove to be in some degree related to any other, and therefore, to the factor analytic one. On the other hand, the phenomenological basis is open from the beginning to the criticism that human reactions to situations depend less on the real nature of the situation than on the personal or cultural history of conditioning to them. Consequently, the similarity of situations in any conceivable psychological sense is likely to have little relation to any possible index of phenomenological similarity. Thus, the similarity of variables can at present be envisaged only on a functionalistic basis. We should not abandon this avenue, despite the suspicion of its nonindependence of factor analysis, until the relation has been empirically examined. But this will take time, for though we have ample data on correlational closeness of variables we have virtually none on transfer of learning or fatigue, perceptual similarity, or motivational equivalence in relation to variables and factors.

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PSYCHOGENETIC STUDIES OF TWINS

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THE PLACE OF PSYCHOGENETICS IN SCIENCE¹

Psychology and the biological discipline of human genetics meet on the common ground of a concern with the variable dynamics of human behavior as exhibited in individuals or groups. The mutual interests of the two sciences lie in the very specific characteristics that cause men to strive and create, to maintain health or succumb to adversity, to choose a proper mate, to work, reproduce, and grow old, to die in harness or in the feeble shadows of retirement [48]. There are biological foundations for each of these functions, and all of them are genetically controlled.

Although a major objective of psychological research into personality

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is to appraise quantitative and qualitative differences in traits and attitudes from an "ethnocentric" standpoint [23], the aim of genetic studies is to search for the basic causes. The changing behavior patterns and adjustment problems of individuals and societies, and the various modes of adaptability to a multitude of technical advances of modern man depend on determining basic factors, which, in turn, follow certain laws.

Some of the underlying principles are elementary, others highly complex. A few are still the subject of much hypothetical controversy, if only because "the cleavage between natural and social science . . . is a cleavage between substance and action, body and soul, the objective and the subjective" [34]. Though the science of human genetics is less than sixty years old, and the twin-study method is the only quasi-experimental genetic procedure available in man (arranged by Nature rather than human ingenuity), a wealth of empirical data has been produced for man's study of himself and his origins [12].

Against this background of "the collective properties that describe the living" [34] and of "the myriad elements constituting the life cycle of a human organism" [1], an attempt will be made here to review those comparative results, methodological principles, and conceptual implications of twin studies which fall into the broad sector of *psychogenetics* (physiological, psychological, and psychiatric genetics). The research method to be described and critically evaluated has led to data having fundamental import for any theory of human behavior. Therefore, though the concern here differs from that of most contributions to the present study, the method under analysis has distinct significance for an assessment of the systematic status of contemporary psychology. Needless to say, however, the analysis of a *research method*, rather than a body of systematic statements, cannot be carried out in strict conformity to the rubrics of the discussion scheme suggested for these volumes.

It is worth emphasizing that the need to depart from the discussion outline in certain ways is no reflection on the relevance of psychogenetic twin data to the human sciences. The character and problematic objectives of the method used for generating such data are dictated by general questions basic to the human sciences. Although it is true that twin studies represent a highly specialized research method, which has born fruit in the garden of human genetics, the facts garnered there call for certain generalizations which must condition the content of any psychological theory of fundamental intent. In the long run, no theory of human behavior or personality can evade the detailed implications of psychogenetic knowledge with regard to such matters as basic problem definition, selection of variables and general causal "model," treatment of basic behavior "processes" (e.g., perception, learning, motivation),

and of development, individual and species differences, and many other issues. Obvious as these facts are, perhaps it is fair to recall that not too long ago it was not uncommon for psychological theorists to operate in something close to a genetic vacuum, and even today one sees occasional evidence of theorists attempting to settle the specifically genetic issues confronting them by fiat rather than reference to available knowledge.

Necessarily, refinement of the usefulness of twin studies in exploring the complexities of human behavior paralleled the gradual advance of *human genetics* toward a status of scientific respectability. This interdependent development sparked a growing awareness in all behavioral sciences of the excellent opportunities afforded by twins, revitalizing in turn the interest in the predominantly genetic aspects of those sciences. At the same time, however, it introduced into psychogenetics a great many controversial issues and procedural problems peculiar to disciplines beclouded by conflicting ideologies. Arising from the widespread conviction that scientific thinking ought to conform to political thinking, the tension in the atmosphere was increased by the notion that there is some basic conflict between religious tenets and the scientific principles of human genetics.

It will always be to the credit of the small and widely scattered phalanxes of twin researchers in many countries that they made a conscientious and sustained effort toward establishing psychogenetics as an ideologically unshackled discipline within the behavioral sciences.

GENERAL METHODOLOGICAL PRINCIPLES

Historically, it is of interest that long before medicine developed into a full-fledged science, artists and scholars focused attention on twin births and attempted to find an explanation for this phenomenon [33]. Ancient mythology contains many references to twin divinities. The Babylonians and Assyrians introduced twins in astronomy, thus giving rise to innumerable legends and horoscopes. Hippocrates believed twins were conceived by the division of the sperm into two parts, with each part penetrating one of the two uterine horns. Cicero commented on what Diogenes had to say about twins and the astrologers who, then as now, insisted that temperament is determined by the influence of the stars. Aristotle and Empedocles expressed the idea that double monstrosities might originate from a phenomenon of codevelopment (partial fusion), and Galen thought that excess heat in the uterus might split the sperm, thus originating two or more formations. During the many centuries dominated by the Arabic and Salernitan schools of medicine, "writers merely reiterated the classic ideas about twins" [33].

Although Viardel observed in 1671 that uniovular twins were always of the same sex, it was not until the second half of the nineteenth century that the phenomenon of twinning ceased to be regarded as a biological whim which aroused either a sense of alarm or idle curiosity. It was Sir Francis Galton who had the foresight to recognize the usefulness of the lives of twins as a research tool in the service of science. His two treatises, *The History of Twins as a Criterion of the Relative Powers of Nature and Nurture* (1876) and *Inquiries into Human Faculty and Its Development* (1883), were classical contributions to the implements of psychogenetics.

Since the rediscovery of Mendel's ingenious theories regarding the "unblending" behavior of stable genetic units in organic inheritance (1900), investigators in many countries have availed themselves with increasing frequency and better techniques of the unique opportunities presented by the regular occurrence of two genetically different types of twins—those derived from one fertilized ovum, and those derived from two fertilized ova. Whereas one-egg twins are always of the same sex, two-egg twins may be of the same or of opposite sex.

In the original version of the twin-study method, the comparison of observable similarities and dissimilarities in the histories of genetically similar or dissimilar genotypes is limited to twin subjects. This procedure requires access to a representative series of one-egg and two-egg twins, of either or different sex, presenting evidence of a diagnostically well-defined trait to which the investigative principles of the proband method [97] can be applied.

In another version, observational or experimental data are obtained from a few well-selected pairs of one-egg twins whose aptitudes, physiological reactions, or adjustive patterns can be compared under different life conditions or in response to different methods of planned management. This procedure has been used by numerous investigators, especially by Gesell and Thompson [36], and is called the co-twin-control method.

In a third version called the twin-family method [49], the collection of comparative data is extended to complete sibships of twin index cases and their parents. The six dissimilar sibship groups compared in this manner are one-egg twins, two-egg twins of the same sex, two-egg twins of opposite sex, full sibs, half-sibs, and step-sibs. This procedure makes it possible to combine the study of twins with the investigative principles of the census, proband, and sibling methods, as well as with special pedigree studies, thereby affording an excellent opportunity to investigate intrafamily variations with a minimum of uncontrolled variables.

The advantages of such a combined procedure are most apparent in the study of traits which present complex sampling problems and require

comparisons in both cross-sectional and longitudinal directions. In fact, so broad is the scope of the twin-family method that it fulfills nearly all the requirements specified by Cattell [15] for the use of the multiple variance method in investigations of functionally, but not necessarily genetically, unitary traits falling into the normal range of personality development. In his scheme, measurable test data are obtained from five different populations, in pairs: (1) a sample population of one-egg twins in their own families, (2) siblings in their own families, (3) siblings with each member of the pair in a different family, (4) unrelated persons in pairs in the same families, and (5) unrelated persons in different families.

DETERMINATION OF ZYGOSITY

For determining the zygosity of same-sex twins, the present method of choice is a refined version of the *similarity method*, originally developed by two veteran twin researchers, Siemens [77] and Von Verschuer [92]. The fetal-membrane method is no longer in use, since it is now known that not all one-egg pairs are born with only one placenta.

In applying the modern similarity method, the comparison of such usually variable physical characteristics as facial features, dental specifications, ear lobe form, and pigmentation of hair and eyes is supplemented by a careful analysis of fingerprints and blood group data, the most reliable criteria for distinguishing one-egg and two-egg twins. Other morphological traits or metric characters cannot be relied upon per se, especially in the presence of a grossly pathological condition in one member of a pair. If dermatoglyphic and hematological data are indecisive in a scientifically important case, it may be advisable to resort to reciprocal skin grafts [70]. Full-thickness homografts are not successful in two-egg twins, although initial takes may last three to four weeks.

In the hematological analysis, it may be borne in mind that a given pair of twins cannot be monozygotic if the blood groups are different. If the blood groups are the same, however, the twins may or may not be monozygotic. Procedural accuracy requires, of course, that in same-sex pairs found to be similar with respect to the major ABO and Rh factors, blood typing is continued until a difference appears or until all available antisera (M-N, S-s, Duffy, Kell, Lutheran, and so forth) have been tried.

The main disadvantage of hematological procedures is that they are rather expensive and depend on the availability of both twins. There are clinically important traits which, by their very nature, take the research subject out of the reach of laboratories.

The dermatoglyphic analysis rests upon the fact that fingerprints conform to one of three basic types, each of which is largely determined

by heredity: whorls, loops, and arches. Whenever possible, the analysis should be extended to both qualitative and quantitative aspects, although the qualitative analysis may often suffice for practical purposes. By means of quantitative procedures, however, dermatoglyphics alone guarantee a maximum degree of reliability.

Of the three quantifying measures used, the simplest one is the sum of the homolateral ridge-count differences [30], a difference of more than 40 being strongly suggestive of dizygoty. In Wendt's individual pattern score or *Musterwert* [98]—a measuring device of almost equal simplicity—each pattern is classified according to certain objective criteria and scored from one to seven. A difference of more than five in the twins' total score is strongly indicative of dizygoty.

Of at least the same diagnostic value are the scores obtained with Slater's discriminant function [81], the most complex test devised. In this method, whorls provide two counts each, loops one, and arches none. The following five characteristics are calculated from the number of ridges intervening between the core of a whorl and its triradii to one and the other side, or between the tip of the innermost ridge of a loop and the triradius:

1. The difference in total count between right and left sides, both members of the twin pair being taken together, expressed as a proportion of the summed total counts.

2. The difference in total count between one twin and the other, both right and left sides being taken together, as a proportion of the summed total counts.

3. The correlation coefficient between right and left sides, pairing digit with digit, radial counts against radial counts, ulnar against ulnar, both twins being taken.

4. The correlation coefficient between twins, pairing digit with digit, the right hand of one twin with the right hand of the other, and left with left.

5. The crossed correlation coefficient between twins, proceeding as above, but taking the right hand of one twin with the left hand of the other and vice versa.

According to this method, a diagnosis of dizygoty is indicated by scores over two, whereas a score below minus one is indicative of monozygoty.

In recent years, a great deal of work has been done to improve the statistical procedures, which are employed in the diagnosis of zygoty, by means of multiallele systems such as the blood groups. The choice of the method depends on the manner in which the genetic system is used [87]. The most common area of application is the assessment of dizygoty in twin samples, since same-sex twins with the same blood

groups may be dizygotic. If the chances of two particular twin partners being dizygotic are to be estimated individually, the amount of hematologically verifiable information may be expected to vary considerably from family to family [69].

The probability of monozygosity for concordant twins can be calculated either with or without reference to the actual phenotypes which the concordance involves [29]. When the zygoty of a particular set of twins is under consideration, the appropriate procedure is to calculate the probability of monozygosity with reference to the phenotypes involved. However, when the given probability quotient is to be obtained for a random pair of twins or when the adequacy for diagnostic purposes of a proposed series of genetic phenotypes is the issue in question, it has been suggested that the probability of total concordance be calculated without reference to the actual phenotypes. The appropriate formulas have been rearranged by Sutton, Clark, and Schull [87].

With current knowledge regarding the essentials of the similarity method so remarkably improved, serious doubt is cast upon the potential usefulness of twin data published without clearly substantiated zygoty classifications.

ASCERTAINMENT AND ANALYSIS OF TWIN SAMPLES

The scientific value of comparative studies, which are based on an unrepresentative or improperly analyzed series of a few sets of multi-zygotes, is even more questionable.

It will always be possible, for instance, to find some one-egg twins who are distinguished by discordance as to a well-known or etiologically obscure trait of predominantly gene-specific origin. Apart from the expected occurrence of phenocopies (nonhereditary variation usually produced by a clearly defined mutant), it is worth remembering, however, that a genetically determined trait may be neither symmetrical in its phenotypic expression nor completely penetrant [2, 3, 21, 55]. Just as it is erroneous to ascribe an observed lack of penetrance of a certain gene effect to the action of environmental factors alone, so would it be a mistake to doubt the primary randomness of developmental processes or of nonadaptive right-left asymmetries in embryonic development, which take place on a biological level where at every moment multiple influences tend to randomize the sequence of events.

From a genetic standpoint, it is fully accepted that "two individuals of the same species develop according to a common design only insofar as gene-controlled mechanisms reproduce the same conditions within and around the embryo" [3]; and no geneticist believes, with respect to traits observed in one-egg twins, that symmetry and concordance are en-

tirely owing to genotypic similarity or to the effect of single genes. The significant influence of the embryonic environment on traits which are not completely controlled by heredity has been demonstrated by Wright's study of polydactyly [102], and the important effect of various combinations of modifying genes on the expression of traits produced by a single mutant gene is equally well established.

In other words, individual differences between one-egg twin partners are no precise measure of environmentally produced variations, nor is it likely that exact quantitative values will ever be assignable to the relative contributions of genetic and nongenetic factors in the production of these differences. It has even been suggested by Allen [3] that any estimate of penetrance based on observations in one-egg twins should be regarded "as a probable overestimate, or as the upper limit of the range in which the true penetrance may lie."

Regardless of the fact that similar reservations apply to estimated expectancy rates for two-egg twins (concordance as to a given type of morbidity), it is apparent that an unrepresentative increase over the average difference between dizygotic twin partners is no indication of the exact contribution of genetic influences even in relatively comparable environments. Two-egg twins are also closely related, so that it should not be very difficult to find a few pairs displaying virtually the same degree of concordance as one-egg twins. In fact, if in a study of intellectual or motivational similarities the search for twins of either type is restricted to pairs attending the same class in a certain college, or if a comparison of biological health and survival values is made only in complete same-sex pairs who have survived to the age of ninety-five years, one should not be too surprised to obtain smaller intrapair differences in two-egg rather than one-egg twins, at least once in a while. On the whole, it is to be expected that observable differences between two-egg twins of the same sex will somehow depend on the extent of dissimilarities between their parents. In a random-mating human population, the given intrapair differences will be more highly correlated with parental than with grandparental differences [5].

With an understanding of the fact that generalized conclusions cannot be drawn from observations made in single pairs or in an unrepresentative series of pairs, the importance of adequate sampling procedures with complete ascertainment of twin index cases (rather than pairs) in a certain district or group of institutions becomes axiomatic. The sampling methods to be used are essentially the same for twins and nontwins, but the establishment of the twinning attribute in a given part of a population requires a systematic screening procedure, including careful examination of official birth records wherever possible.

According to Allen [3], the most useful evidence of unbiased sampling

in a twin study is provided by an approximate agreement between a twin sample and the parent population, either with respect to the proportion of opposite-sex pairs or, when known, of one-egg pairs. Roughly speaking, the population twin rate is 2 per cent, and the proportion of twins from opposite-sex pairs is about one-third of all twins. Any series of twins differing significantly from the parent population in either of these two characteristics can be safely regarded as selected and unrepresentative. It may be reemphasized, however, that the given statistics are to be based on twin individuals rather than pairs, since even random and representative samples may deviate considerably from the parent population in statistical estimates based on pairs.

In the United States, the precise twin rate is 2.19 per cent of all babies born since 1928. This rate is reduced to about 1.9 per cent by excess twin mortality within the first year of life, whereas the 2:1 ratio of same-sex to opposite-sex pairs observed at birth remains virtually unchanged in all age groups. General mortality is higher in males than in females, an increase that is apt to have some effect on the proportion of male twins in an adult sample. However, the reduction of male twins is not sufficiently pronounced to necessitate modification of the 1:1 sex ratio in estimating the proportion of one-egg pairs by means of Weinberg's differential method [3].

After the first year of life, there is no significant difference between the mortality rates of twins and nontwins, so that the proportion of twin individuals in the population is assumed to remain nearly the same at all ages. However, Allen has called attention to the fact that as pairs are broken by mortality and migration, the number of intact pairs is reduced at successive ages, thus making it necessary to analyze twin data in terms of individuals rather than of twin pairs. Among people who survive to an advanced age, the relative frequency of pairs represented by at least one twin may be nearly twice as great as at birth.

Another point to be considered in the analysis of twin family samples, in relation to specific pathological traits and their variations in different periods of time or life as well as in different ethnic or socioeconomic settings, is the need for consistently corrected morbidity risk figures (expectancy rates as obtained by the Weinberg method). In clinical investigations, expectancy rates are more valuable than the usual prevalence statistics favored by public health authorities. According to Strömgren [84], the disease expectancy is "the risk of becoming ill during one's lifetime, if one lives long enough to pass the period of risk" (the time during which the disease may develop).

Once again, the statistics describing such a sample are to be computed from twin index cases (proband) rather than from twin pairs, if some or many pairs are represented by a single index case, and if

morbidity expectancy rates for various groups of siblings and co-twins are to be compared within the sample (twin-family method). The same method of computation is required for estimating penetrance and concordance rates. In the latter case, however, it should not be overlooked that the number of index cases from concordant pairs is to be halved in order to correct for the twofold representation of concordant pairs in the sample [3]. Since the concordance rate is usually understood to provide the directly established proportion of pairs with two affected partners, if ascertainment of affected twins is complete for the population, it is evident that the number of concordant pairs is one-half the number of cases observed in these pairs.

In most instances, of course, differences between one-egg and two-egg groups of twins will have the same statistical significance whether evaluated in terms of concordance or morbidity expectancy. For the use of concordance rates obtained under conditions of incomplete ascertainment of twins affected by a pathological trait, various corrective formulas have been devised by Allen [3].

In the analysis of normal personality variations in twin samples, the best-known statistical technique employed in estimates of genetic components is Holzinger's h^2 : the variance of the dizygotic twins minus the variance of the monozygotic twins, divided by the variance of the dizygotic twins [67]. In order to establish the significance of the h^2 values, an F test may be used for the ratio of the dizygotic over the monozygotic variance.

More recently, Cattell and associates [16] introduced a multiple variance analysis design as a refined method for analyzing "dimensions of personality which have been established by factor analytic investigations upon personality responses in rating data, questionnaire data, and objective tests." The twelve primary personality factors measured were obtained on the Junior Personality Questionnaire Test and included three factors which provided evidence for predominantly genetic determination: general intelligence, cyclothymia vs. schizothymia, and adventurous cyclothymia vs. submissiveness. Four factors assigned equal roles to heredity and environment, although "heredity predominated between families" (energetic conformity, dominance, socialized morale, and impatient dominance). The predominantly environmentally determined personality factors consisted of tender-mindedness, general neuroticism, surgency-desurgency, will control, and somatic anxiety. A final report on the results of this promising study has not yet been published.

As a general principle for the analysis of normal personality traits, it may be worth mentioning that the given twin data should be expressed in terms of varying degrees of intrapair similarity or dissimilarity, rather than in terms of concordance or discordance. Twins may be concordant

or discordant as to rheumatic heart disease, but not as to the normal shape or color of their hearts.

PROCEDURAL LIMITATIONS AND ADVANTAGES

Regarding the procedural potentialities of twin studies, research workers in psychological genetics, not to mention their critics, would do well to remember that, like any other scientific procedure, the twin-study method has its limitations and its very specific advantages. It is a mistake to overstrain the merits of the method by letting it bear the burden of proof in extravagant attempts to measure multitudinous variables in "the dilemma of mind-body dualism" [65].

It is equally inappropriate to belittle the value of twin studies either because of some inherent imperfections in their applicability, or because of an unexpected lack of success in overexpanded investigations concerned with "concepts of absolute or ultimate causes" [65]. In the humble words of Carl Lotus Becker, "the significance of man is that he is insignificant and is aware of it."

In general terms, *the limitations* of the twin-study method can be placed in three categories:

1. Imperfections of the research species.
2. Imperfections of pluridisciplinary research workers, research methods, and research teams.
3. Imperfections of quantifying methods for measuring meaningful personality differences in genetically similar or dissimilar phenotypes.

As to the first group of limitations, it cannot be helped that, like every other human research subject, human twins enjoy more sacred rights, a longer life span, and more intricate systems of organization and regulation than any species of laboratory animal. Of course, they cannot be kept in cages, nor can they be separated before they are born. Forced to exist in crowded quarters during important stages of embryonic development, they may carry some genes which are sensitive to asymmetrical cytoplasmic influences in this prenatal period [22]. Also, they are prone to prematurity [51] and birth trauma [4] and seem to have a preference for "non-white" mothers, who do not belong to "the lower socio-economic segments of the population" [61].

Following the ordeal of being born, twins enter a world in which parents rear their own children, thereby depriving similar as well as dissimilar twin partners of the chance of benefiting from, or being observed in, entirely different cultures. Instead, some twins will be subject to superstitious beliefs, fierce parental pride or bewilderment, the hazards of educational laxity, easily mistaken identity, or being thought of in terms

of behavioral opposites, and the potential effects of a modified form of sibling rivalry.

For instance, the marked similarity of school performance ratings in one-egg twins has been explained by Husén [41] on the basis of such incidental factors as going to and from school together, or being mistaken for each other by teachers. Zazzo [103] ascribed a considerable part of an observed IQ deficit (9 IQ units) in a European twin population to a language retardation resulting from the twins' preference for using a "secret language" with concomitant social isolation.

In fact, Bauer [11] believes that one-egg twins can be regarded as psychologically incomplete individuals "sharing an ego" and projecting each other's inner life in their manifest behavior. On the other hand, Burlingham [14] has expressed the opinion that twins have "a more acute rivalry to cope with than is the case with ordinary siblings." Compared with the rivalry among siblings, that between twins was found by this investigator to start at an earlier age, to be more pronounced "because" of the necessity of competing on an equal footing, and to culminate more frequently in early mutual death wishes.

Other potentially disadvantageous aspects of twin development have been seen in a weakened relationship with the parents, as a corollary of an intensified identification process between the twins, in a bewildered parental attitude toward two children who are so alike, and by erroneous analogy with freemartins in cattle, in the purported sterility of one of identical twin brothers [50]. According to Burlingham's theory the identification mechanism tends to preserve a marked degree of similarity between twin partners that would otherwise gradually yield to significant differences in behavior.

Fortunately, most of these interpretive inferences regarding the precariousness of a twin's childhood, adjustment, or reproductive capacity are far too gloomy. Once a twin has survived his first year of life without evidence of organic damage, he is virtually certain to be undistinguished from single-born individuals, even to the extent of having a complete ego of his own. There is no evidence of premature babies being more likely than full-term infants to develop a psychosis, nor are there any statistical indications that infections, emotional disturbances, or other tangible disabilities are more prevalent in twins than in the general population.

On the contrary, twins are known to vary as much in their personalities, intellectual abilities, and stress symptom thresholds as do single-born people, and there is no reason to believe that they are less healthy, less longevous, or less selective in regard to their own potential formula of adjustment. In childhood they seem to be as capable as many other children of working out fairly adequate ways of dealing with difficulties

in their home milieu and interpersonal relationships [45]. As adults they show the expected variations in marital and reproductive histories, and when tested psychometrically in far-advanced age, they produce mean scores which are entirely "within the limits of normal expectation" [50].

Since even Burlingham reported that the early differentiation of roles, said to divide one-egg twins into one active and one passive partner, was found to be "determined by the bodily strength of the children and to change according to changes in their relative health and development" [14], it cannot be assumed "that with regard to intelligence or general vitality or any other aspect of biological development, a twin derived from only one-half of a fertilized ovum might tend to be inferior either to a two-egg twin or to the average single-born person" [50].

It is reasonable to say, therefore, that the main disadvantage of twins as research specimens lies in certain imperfections which characterize human beings, human societies, and human vicissitudes in general. By the same token, the willingness of twins to serve as research subjects in a cooperative spirit somehow depends on establishing and maintaining personalized relations with them. This requirement calls for a substantial degree of empathy, humbleness, and sincerity on the part of the investigator.

The common denominator for the second set of twin-study limitations—imperfections consistent with pluridisciplinary research workers, research methods, and research teams—is the fact that twin researchers are human, too. They cannot expect to be either more longevous or much more versatile than their research subjects, and they always risk frustration in the conduct of studies which cut across the customary borders of individual disciplines.

Since they cannot hope to qualify as experts in every discipline dealing with the structural, physiological, or psychological aspects of personality development in normal and pathological constellations, gemellogists must learn to be satisfied with fractional answers to pluridimensional problems. On this level, however, they are easily misled into describing observed temporary and perhaps reversible dissimilarities between twin partners in the antithetical setting of absolute dichotomies, or in mystifying terms borrowed from transcendental schools of thought.

Ideally speaking, individual twin research workers, depending on their professional qualifications, should have little trouble delineating their tasks according to whether the variations studied fall into the normal or pathological ranges of variability. Unfortunately, the dividing lines are seldom clearly drawn, and there are not too many research workers who are willing to anticipate limitations in their investigative capacities.

The alternative is the formation of interdisciplinary research teams

for conducting twin-study projects which are broad as well as continuous in a longitudinal scheme. However, apart from being expensive and difficult to maintain over a prolonged period of time, such an organization may prove susceptible to professional rivalries and the introjection of incompatible biases and predicative jargons. So long as there is still much intradisciplinary disunity in most of the behavioral sciences, it would seem advisable to bear in mind that there is a point of diminishing returns in the prospects of pluridisciplinary projects.

On the genetic side, two divergent philosophies have emerged—the statistical (static) and the physiological (dynamic) points of view. The statistical philosophy has been described as hyperatomism and hyperselectionism [37] and is suspected of interpreting every generalized set of facts by the introduction of more and more units for statistical treatment. This pattern may lead to serious consequences by requiring “astronomical numbers of modifiers and a similar number of tiny but specific adaptations.” In order to explain all phenomena which appear to be gene-controlled, more and more genes are introduced in the form of modifier systems built up by selection.

The dynamic approach is preferred by Goldschmidt’s school, as well as the writer. Although it accepts the basically statistical tenets of genetics, the main objective is seen in an understanding of behavioral phenomena in terms of gene-specific molecular processes and developmental systems, with all their interaction, embryonic regulation, and integration. In this frame of reference, the concept of Mendelian heredity (with or without simple segregation ratios) becomes more or less synonymous with “chromosomal heredity.” Pertinent environmental factors which mold, and the formative elements which secure behavioral malleability on the human level are viewed as “end-products of the same evolutionary process,” and are likened to “the two sides of a coin, defying analysis as independent variables” [45].

On the psychological side, there is an even more perplexing division into schools, each of which rejects some of the fundamental standards of classification accepted by the others [82]. The emotional tone of this dispute has been compared by Slater to that which was rampant in “the days of debate between allopaths and homoeopaths over a century ago.” This country has also had its share of the spurious nature-nurture battle for supremacy, the unfortunate effects of which have been commented upon by many writers.

Especially relevant here are Skinner’s reflections on science as “a continuous and often a disorderly and accidental process” and on the scientist as “the product of a unique history” who may be “more concerned with his success as a scientist than with his subject matter” and may therefore aspire to assume “the role of a roving ambassador” [78].

In this context, psychiatry is presented as "a field in which behavior is customarily described . . . indirectly." Hence, psychologists are cautioned to put "the older statistical and theoretical techniques in their proper perspective" and awaken to the possibility that there may be direct observation of behavioral processes.

Agreeing with Rogers's notion that "science is not an impersonal something, but simply a person living subjectively another phase of himself . . . and that the knowledge gained through scientific method . . . is a matter of subjective choice dependent upon the values which have personal meaning for me" [71], Skinner is opposed to any attempt "to fit all scientists into a single mold." From a gemellological standpoint [45], it is also imperative for workers in the behavioral sciences to discontinue the practice of using a two-valued system of conceptualization in dealing with problems of personality development.

So long as belief in heredity as an essential determinant of variable behavior patterns is equated by some workers with a fatalistic distrust of man's perfectibility, it will be difficult to form pluridisciplinary research teams unhampered by rigidly codified schemes of dichotomous absolutes. Twin studies foster the hope that environmental variables are not the only ones which can be controlled by man. However, research workers are required who are not afraid of delving into the dimly lit strata of man's bipolar existence. Only as a team learning to avail itself of the opportunity to break new ground with the aid of new research methods, can these workers come to understand basic behavior patterns in terms of physiochemical or molecular processes powered by genic elements.

Of course, the research tools employed in the conduct of comparative twin studies cannot possibly be less imperfect than the research workers using them. Since these studies are largely concerned with problems at the very beginning of an intricate chain of cause and effect [82], their usefulness depends on an unbiased collection of clinical, demographic, and psychometric data. Therefore, it follows that most of the imperfections of pluridisciplinary twin research workers are also inherent in their research methods. Relatively few workers in the behavioral sciences are able to accept the virtual inseparability of genetic and nongenetic components of personality and the fact that most test devices for measuring personality differences have proved refractory to standardization [9, 18, 47].

Thus, for the sake of respectability a gene-specific effect on human behavior is acknowledged by some euphemizing analogy, or by keeping it in what may be called the "etcetera" category of contemporary psychology and psychiatry. The tendency to regard accounts of gene-controlled phenomena as the work of a devil's advocate persists [45]. The temptation to avoid labeling genetic factors as such is most likely to arise when psychometric attempts are made to assign quantitative values

to the relative contributions of genetic and environmental factors in producing individual differences, or whenever it becomes necessary to distinguish between cause and motivation in human behavior.

It is ironic that the same factors producing imperfections in the twin-study method should also be listed among the principles specified as *procedural advantages*. Nevertheless, both the humanness of twins as research specimens, and the need for employing interdisciplinary research teams are assets as well as disadvantages of the method. Rabbits are out of place when it comes to differentiating between the symptomatologies of schizophrenia and manic-depressive psychosis through the behavior of monozygotic individuals; and the best way for members of different disciplines to learn to respect each other's work is to give them a chance to work together.

Compared with other techniques for studying normal or abnormal variations in human subjects, families, and populations, the twin-study method has the following specific advantages:

1. Except for traits which are peculiar to twins or significantly altered in twins, the method constitutes an excellent sampling procedure for the investigation of variations displayed by different genotypes in a controlled environment, or by a constant genotype under the influence of different environmental conditions.

2. In the study of traits which require close personal contact with the research subject and access to all strata of a given population, the method provides an inconspicuous approach to families whose private affairs might not otherwise be open to study.

3. Likewise, the method facilitates the conduct of combined cross-sectional and longitudinal investigations. Such investigations are indispensable in pathological conditions where information is needed not only as to the selection pressures bearing upon affected persons, but also as to variations related to age of onset, duration, or severity of clinical symptoms, expected distribution between the sexes, or the differential aspects of reproductivity, responsiveness to treatment, and other survival values [43].

4. In conjunction with the statistical principles of the proband and sibling methods (Weinberg), a twin study covering an entire district or state represents the most economical substitute for a total population survey requiring personal interviews and the application of controlled test procedures.

SERIAL TWIN DATA ON INTELLECTUAL AND PERSONALITY VARIATIONS

In reviewing the part twin studies have played in analyzing variations in intellectual abilities and personality potentials, no attempt will be

made to achieve bibliographical completeness. The report is focused on the results of fairly recent investigations in which a sizable series of twins was used. Pertinent data have been considered primarily from a genetic viewpoint on the clinical side, and from the standpoint of their potential psychological significance on the genetic side. The role of non-genetic influences on the psychometric, psychodynamic, and pathoplastic aspects of personality differentiation is dealt with elsewhere in this volume. Needless to say, full appreciation of these influences is not indicative of loyalty to any one school of training.

General understanding of the interaction of heredity and environment is greatly enhanced by a dynamic concept of constitutional, intellectual, and characterological phenomena based on the solid foundations of genetic principles [45]. Since the organism is both active and reactive, the importance of genic elements in the organization of behavior patterns rests on the interdependence of organic structure and psychological function throughout the life of the individual. There is no behavior without an organism, no organism without a genotype, and no physiological adaptedness without continuous and fully integrated gene activity.

Of course, in order to maintain his present evolutionary level, man must be both conditionable by culture and impressible by education. The ability to learn from others and to profit from experience is determined by the genotype; cultural values and opportunities have to be acquired by each individual through communication with his group. Broadly formulated, then, a person's phenotype may be defined as the visible expression of his malleability by environmental influences, and his genotype as determining his norm of reaction to the total range of possible environments during his lifetime. The implication here is that every gene-controlled mode of activity requires an operational area in which to unfold [9, 23, 47, 65].

In the area of normal personality variations, the earliest twin studies were limited to a descriptive account of the histories of interesting twin pairs, without looking at the backdrop of the total genetic and environmental variation observed in a population. More recent data have been based on serial studies in which the development and performance of each twin were compared with those of his partner. Early pilot studies were those by Galton [32] in 1883 and Thorndike [89] in 1905. They were followed by the work of Von Verschuer [92], Lange [56], Wingfield [101], Herrman and Hogben [40], Graewe [38], and Gedda [33] in Europe, and by that of Rosanoff and Orr [72], Gesell [35], Merriman [64], Lauterbach [57], Newman and associates [67], and Burks [13] in the United States.

On the heels of progress made in the procedures of clinical classifica-

tion and zygosity determination, psychiatric twin data of more than historical interest were presented by Luxenburger [62] in 1930, and by Rosanoff et al. [74] in 1934. Our New York State studies were organized in 1936 [43, 8]. They were matched by Essen-Möller [26] in Sweden (1941) and by Slater [80] in England (1951) with series of fairly comparable size.

In Galton's study [32], subjective estimates of intelligence and zygosity were used. Thus he observed that ordinary environmental differences were not sufficient to make "similar" twins unlike, whereas "dissimilar" twins were not found to become more alike under the influence of similar surroundings.

Thorndike's series [89] consisted of 50 pairs unclassified according to zygosity. His working hypothesis was that higher intrapair correlations would be obtained in "trained" rather than in "untrained" functions, if the training itself were responsible for similarities in scholastic achievements and comparable intellectual functions. Evidence for a primarily genetic determination of variations in mental abilities was seen in the finding that resemblances between twins changed neither with age nor with training.

In the studies of Merriman [64] and Lauterbach [57], data obtained by the Stanford-Binet, Army Beta, and National Intelligence Test, as well as intelligence estimates by teachers, were evaluated in 100 and 200 pairs. The observed resemblance in IQ was of the same order of magnitude for male pairs (0.877 ± 0.30) and female pairs (0.857 ± 0.029), and older twins were not found to be more alike than younger ones. Wingfield [101] introduced various procedural refinements (using 102 pairs in the age group seven to fifteen), but obtained essentially the same results as the earlier investigators.

Herrman and Hogben [40] compared only very similar one-egg twins (65 pairs) with very dissimilar two-egg twins of the same sex (96 pairs), employing the Otis Advanced (Form A). Apparently, the observed one-egg correlation (0.86 ± 0.04) was somewhat too high, and that for the same-sexed two-egg series too low.

Like Newman, Freeman, and Holzinger [67], Burks [13] was especially interested in the development of one-egg twins separated in early childhood. Her observations on four pairs of this type were interpreted as indicating the significance of both genetic and nongenetic factors in shaping the life histories of genetically alike partners.

In the Chicago study, 100 nonseparated, same-sexed pairs (50 one-egg, 50 two-egg) of school age (eight to eighteen years) were compared with 19 one-egg sets (aged eleven to fifty-nine) who had been separated early in life. The Stanford-Binet correlation for one-egg twins reared apart was 0.77, about midway between those for two-egg twins reared

together (0.63) and one-egg twins reared together (0.88). The investigators concluded that only extreme environmental differences tend to have an appreciable effect upon intelligence.

Generally corroborant data were reported by Thurstone et al. [90] for a sample of adolescent pairs (48 one-egg, 55 two-egg), by Baroff [10] for a series of 40 one-egg and two-egg pairs with high-grade mental retardation, and by Feingold [28] and the writer [46] for a series of 127 same-sexed senescent pairs (mean age sixty-nine and seven-tenths years) studied longitudinally over a period of eight years.

The extensive battery of Thurstone and associates consisted of tests chiefly measuring primary mental abilities, personality, and psychomotor function. The analysis raised almost as many questions as it answered, because of inconsistencies in motor function scores for the two hands and the failure of reasoning and mathematical faculties to distinguish the two zygosity groups. Nevertheless, the study substantiated the assumption of an important genetic component in those abilities differentiating one-egg and two-egg pairs. Especially on some of the visual, verbal, and motor tests, two-egg twins displayed marked intrapair differences with significantly increased frequency. The decisiveness of this finding was confirmed by the preliminary data of Vandenberg [91] reported in 1955.

In Baroff's investigation of intelligence as measured by mental age, one-egg twins proved to be significantly more similar than two-egg twins, despite the fact that this institutional series included only pairs concordant as to mental retardation. In the one-egg group, the degree of similarity in mental age remained unaltered by the duration of institutionalization (relatively constant environment), whereas two-egg twins showed increasing disparities. In the author's opinion, genetically unlike persons in a similar environment are likely to become increasingly dissimilar in the symptomatology of an inherited type of mental defect.

Feingold's impressive data were collected in conjunction with the senescent twin population study organized by this writer and his associates in 1945 [28, 46, 50]. The main purpose of the project was to investigate intrafamily variations in aging patterns in both cross-sectional and longitudinal directions. The total sample consisted of 2,536 senescent twin index cases in New York State (sixty years of age and over), including a series of 1,557 index pairs whose zygosity was sufficiently established to be useful for comparative longevity analysis. The total number of one-egg and two-egg pairs, observed with respect to health status, intellectual performance, and length of life, approximated a 1:2 ratio (518:1039) and was in accordance with statistical expectation. At the beginning of 1956, after eleven years of observation, 516

twin subjects were still alive, including 179 pairs where both members survived.

The psychometric study was planned in such a way as to provide comparable test scores of same-sexed twins on a longitudinal basis. The 240 test cases chosen for this purpose in 1947 had to meet the requirements of being white, literate, native-born, noninstitutionalized, and apparently free of mental and physical illness. Of this sample, 36 complete pairs and 7 single survivors were retested with the same battery

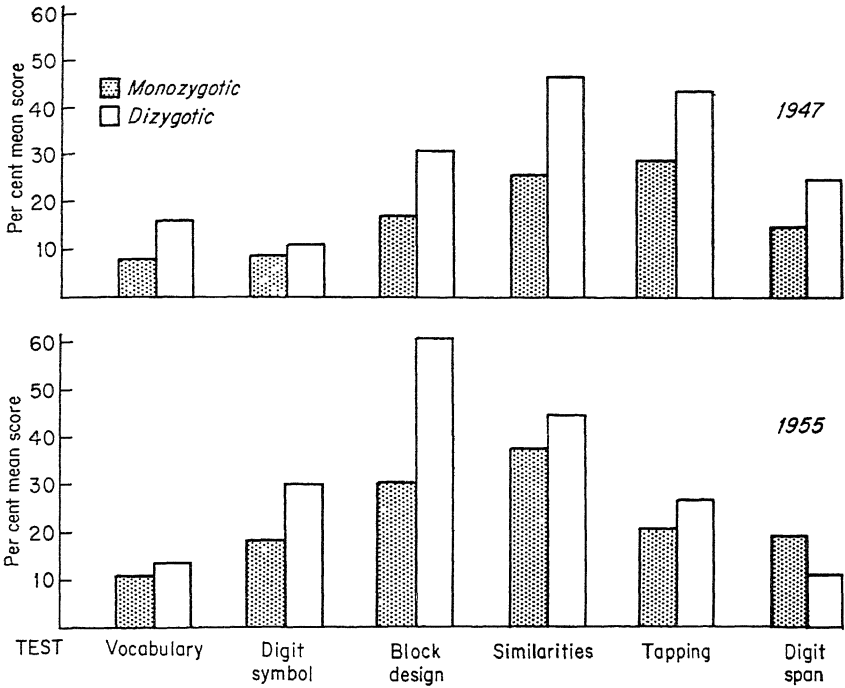


FIG. 1. Comparative mean intrapair differences in test scores (1947 and 1955).

in 1955, after a mean interval of 7.8 years. The battery consisted of four subtests taken from the Wechsler-Bellevue Scale I (digit span, similarities, block design, digit symbol), the vocabulary list of the Stanford-Binet 1916, and a paper-and-pencil tapping test. At the time of the retest in 1955, the age of the survivors ranged from sixty-eight to eighty-seven years, with a mean age of seventy-four and one-half years.

The results of the first test round showed that the mean intrapair differences in test scores measuring various intellectual abilities were consistently smaller in one-egg than in two-egg pairs (Fig. 1). The difference between the two zygosity groups was significant at the .01 level of confidence for the vocabulary, digit symbol, and tapping tests. In

accordance with Zazzo's finding [103] that the difference between the intelligence quotients of twins and nontwins tends to decrease with increasing chronological age, no evidence was obtained in support of a difference between the test performances of aging twins (one-egg or two-egg) and comparable single-born persons. There was a difference between male and female test scores, indicating that certain intellectual changes in the period of senescence are observable in males at an earlier age than in females. On the whole, the test data clearly revealed that gene-specific intellectual differences persist into a well-advanced age.

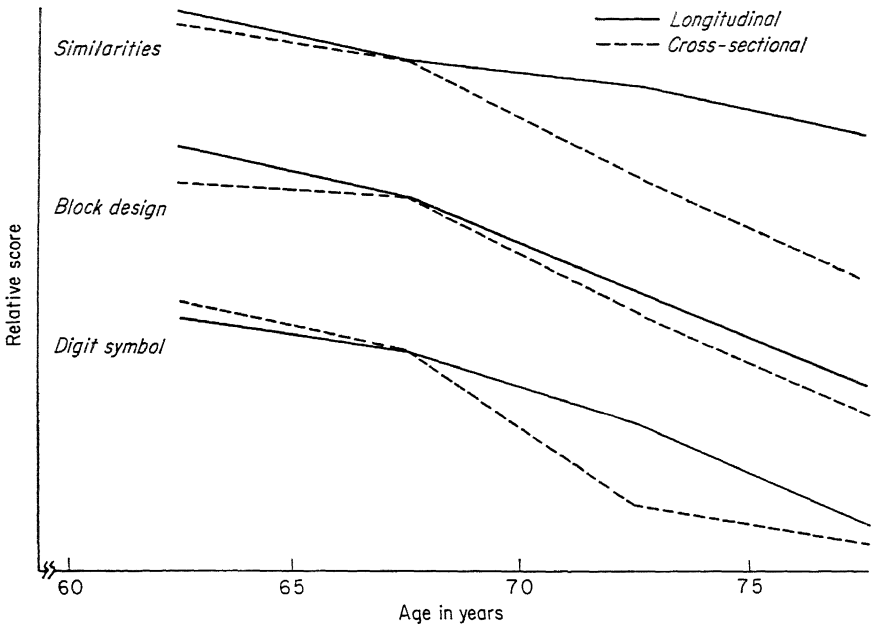


FIG. 2. Trends of intellectual decline in senescence (longitudinal and cross-sectional test data).

Although the retest series [46] was numerically too small to show statistically significant differences between zygosity groups, it was still apparent in five out of six tests that the mean intrapair differences tend to be greater in two-egg than in one-egg pairs. The digit span test data were the exception.

As to the longitudinal trends revealed by the testable survivors, the test results showed a consistent although slight decrement in intellectual abilities during senescence. By and large, this finding was in agreement with the trend observed in cross-sectional investigations, but the slope of the decline in the longitudinal study (Fig. 2) was smaller than that expected on the basis of survey data.

Another interesting observation was that the retested twin pairs scored higher on the original tests than did the total sample tested previously. This disparity seemed to indicate a relationship between test score level and survival potential. However, without corroboration by data from larger samples, the implications of this hypothesis could not be regarded as conclusive. The same reservation had to be made for the finding that two-egg twin partners who are most similar in test scores in the senescent period may have the best chances of surviving together.

TABLE 1. BIENNIAL MEAN INTRAPAIR LIFE SPAN DIFFERENCES IN SAME-SEX TWIN PAIRS OVER AGE 60 (BOTH DECEASED)

	Year of analysis	Number of index pairs	Intrapair life span differences expressed in months		
			Male	Female	Total
One-egg pairs	1948	32	47.6	29.4	36.9
	1950	68	42.9	31.2	36.7
	1952	76	40.7*	30.7*	35.7*
	1954	78	40.7*	31.6*	36.0*
	1956†	104	49.9	33.9*	41.9*
Two-egg pairs‡	1948	36	89.1	61.3	78.3
	1950	70	79.1	63.2	71.8
	1952	86	79.1*	69.5*	73.7*
	1954	102	69.5*	79.1*	74.6*
	1956†	110	69.2	75.3*	72.5*

* Significant at 1 per cent level.

† Preliminary data.

‡ All opposite-sex pairs over age 60:106.0 months.

How genetic factors determine the ordinary length of life and other general health and survival values has been shown by comparing the life spans of those twin pairs where both partners died of verified natural causes after the age of sixty years (Table 1). In all the biennial estimates made since the beginning of the study, the mean intrapair life span difference has been smaller in one-egg than in two-egg pairs. The present total mean difference (1956) varies from 41.9 months in the one-egg group to 72.5 months in the two-egg group of the same sex. The differences between the two zygosity groups have been statistically significant ($p \leq .01$) in the last three analyses, although there is an expected disparity between the contributions made by the two sexes to the total difference, probably owing to the shorter life span of the male. The present difference between the mean intrapair life spans of the two

male groups is close to 20 months, but it does not reach the level of statistical significance if only males are considered.

In general, as to normal personality variation it can be said that gene-specific derivations range from physical, coordinative, physiognomic, and temperamental characteristics to intellectual abilities, affective regulations, and special talents [39, 43, 90, 91]. In between are sex maturation patterns, variations in antibody production, the capacity for longevity, and the ingredients for sustained tolerance of physiological or psychological stress, a highly essential prerequisite for a well-balanced personality [45, 86]. Except for one-egg twins, it is apparent that each individual has his own threshold of adaptability to different types of stress, and his own pattern of stress symptom formation.

Consistent similarity in the composition of these personality components is not observed in the absence of genotypic identicalness. Two-egg twins of the same sex tend to differ as much in their personalities as any siblings reared together or apart. Only one-egg twins retain basic similarities in appearance and general personality traits despite pronounced differences in life experience.

This principle is not refuted by the fact that a spiral-like development toward marked behavioral dissimilarity (chronic alcoholism, delinquency, suicide) may sometimes result from a seemingly insignificant difference in the original adjustive patterns of one-egg twins [43, 45, 81]. Extreme disparities of this kind are the exception rather than the rule.

SERIAL TWIN DATA ON PSYCHOPATHOLOGICAL VARIATIONS

In considering the contributions made in the area of psychopathological variations by means of serial twin data, it may be helpful to bear the following points in mind: from a genetic viewpoint, the dividing line between a normal state of adjustment and those minor forms of ill health commonly referred to as psychoneurotic is not regarded as static, nor as less vaguely defined than that between normal and subnormal intelligence. A deviant behavior pattern is not presumed to be the result of a simple genotype-phenotype interplay, reducible to an aggregate of well-delineated causes and effects, nor is it merely thought of as the concomitant of a fixed congenital aberration, or as a self-limiting error in homeostasis, or as just an unfortunate episode in adjustment [68, 96].

Genetically, human behavior of any variety is viewed as an extremely complex and continuous chain of events in the individual's adaptive history. It is axiomatic, of course, that even the finest genetic endowment can go astray, either because of an unusual combination of adverse circumstances (intrinsic or extrinsic) or because of prolonged abuse.

An example is the tendency to suicidal acts, which was long thought to be due either to an inherited degree of unfitness in general personality structure or to a special reaction type distinguished by introjective aggressiveness. Neither theory, however, has been supported by our data on a series of 18 one-egg and 21 two-egg pairs of twins, one of whom had committed suicide [43]. With but one exception, all have remained discordant. In short, suicide is one of the few phenomena unlikely to occur in both twins even under similar conditions of maladjustment and privation.

The high-tension state released by a suicidal mechanism, in the form of a self-destructive trigger reaction to adverse life conditions (compulsive or twilight-state type of short-circuit reaction under stress), apparently depends on unusual and not easily duplicated constellations of motivational factors. Although two twin partners may both commit suicide, it will only be by chance and without direct relation to each other. Thus, concordance will be extremely rare even in one-egg twins.

As to psychoneurotic reaction potentials (outside the field of criminality), Eysenck and Prell [27] had the courage to join the small group of investigators who availed themselves of the opportunities afforded by the twin-study method. In line with their findings in a series of 25 one-egg and 25 two-egg pairs, they classified "the neurotic personality factor" as a biological and largely gene-specific entity, estimating the genetic contribution to this "neurotic unit predisposition" as 80 per cent.

Not quite so specific is Slater's [80, 82] interpretation of the neurotic symptoms, observed in a series of 9 one-egg and 43 two-egg pairs, as exaggerations of polygenically determined personality variants, less closely related to a given type of stress than to the basic personality. Despite "almost identical personality," seven of the nine one-egg pairs failed to present concordant psychoneurotic histories, as against 15 concordant pairs in the two-egg group. Therefore, critical deviations in a person's career were assumed to be due to relatively chance occurrences, such as the personality of the chosen marital partner. "One twin might suffer a mischance which would lead to a vicious circle of ill-health, social failure, hardship, discouragement and increased ill-health, while the other totally escaped." According to this theory, there are graded constitutional vulnerabilities in more than one dimension, so that "the man who breaks down with a neurotic illness is likely to be handicapped not by one constitutional weakness of severe degree, but with a number of minor weaknesses."

However, Shields's study [76] of 62 same-sexed pairs between the ages of twelve and fifteen years (36 one-egg, 26 two-egg) provided evidence for one-egg twins (69 per cent) being twice as likely as two-egg twins (31 per cent) to have the same degree of adjustive difficulty. With

each child rated "on a four-point scale of psychiatric maladjustment," twins of either zygosity had no higher incidence of neurotic adjustment problems than single-born controls, but male twins and nontwins far exceeded their female counterparts in presenting some difficulty in adjustment. Little more than one-half of the total group of English school children investigated (four South London areas) were classified as non-neurotic. Nongenetic explanations for the observed differences between one-egg and two-egg twins were rejected, perhaps somewhat summarily, especially in regard to the frequent similarities in type and severity of neurotic behavior patterns.

One of the highest one-egg concordance rates reported has been that for homosexual behavior in the adult male [43], although all concordant twin partners in this series denied any mutuality in overt sex relations. Nevertheless, 44 one-egg pairs yielded a nearly perfect concordance rate, with the index cases standing at least midway on the homosexuality scale applied, and with pronounced similarity in the role taken by twin partners in their individual sex activities. In the two-egg group (51 pairs), nearly 60 per cent of the co-twins of predominantly or exclusively homosexual index cases showed no evidence of overt homosexual behavior at any age, and only 11.5 per cent were given homosexuality ratings of five or six on Kinsey's scheme. The likeliest genetic explanation for these findings would seem to be a gene-controlled disarrangement in the balance between male and female maturation patterns, resulting in a shift toward an alternative minus variant in the integrative process of psychosexual maturation.

Another condition with a well-established one-egg concordance rate of close to 100 per cent (6 pairs) is an entirely different defect of more obvious organicity, namely, mongolism, the relationship of which to maternal age is regarded as fully substantiated [7]. Since the corresponding rate for two-egg co-twins (23 pairs) does not seem to exceed that of their later-born siblings (approximately 4 per cent), the search for the etiological factor in mongolism has been narrowed down by twin data to a more or less permanent change in the mother's endocrine or reproductive system. Apparently, the noxious influence during a mongoloid pregnancy is not transient, but acts on a genetically predisposed embryo, or upon the ovum, or upon the embryo before the earliest stage when twinning occurs by division.

Serial twin studies have also aided in investigating the etiology of two other organic syndromes, cerebral palsy [4, 52, 89] and convulsive disease [17, 58, 59, 60]. As to the former condition, the data of Allen (60 twin cases) and Thums (90 pairs) have indicated that twins are rarely concordant. There is a high rate of stillbirth or neonatal death in the co-twins of cerebral palsy cases, but no evidence for a specific genetic

susceptibility to prenatal or natal injury. Apparently, many twins with cerebral palsy are survivors of adversities which proved fatal to their twin partners. The circumstances most likely to affect both twins of a pair include nonspecific maternal and genetic factors, as well as prematurity per se, but probably not mechanical trauma during birth.

The most extensive analysis of epileptic twin pairs (30 one-egg, 130 two-egg) is that of Conrad [17], with concordance rates of 66.6 and 3.1 per cent, respectively. Unless plainly repudiated, the results of this study represent strong evidence for the genetic origin of true convulsive disease.

According to Lennox et al. [58, 59, 60], cerebral dysrhythmia is an electroencephalographic expression of the epileptic genotype, assumed to be the result of a dominant gene by the Boston group, and of polygenic factors by Alström [43]. In epileptic twins, 25 per cent of two-egg and 100 per cent of one-egg pairs have been found to be equally dysrhythmic, despite marked dissimilarities in clinical symptoms.

In the area of criminal behavior [53, 56, 74, 80, 85], there is still an emphatic need for well-planned cross-sectional and longitudinal twin data. Based on the findings of Kranz, Lange, and others [43], criminality concordance rates vary only from 14 per cent in opposite-sexed pairs to 54 and 66 per cent in same-sexed two-egg and one-egg pairs, respectively. This distribution indicates that both family milieu and basic personality traits play important parts in shaping the habitual criminal. Therefore, the trend toward similar criminal behavior in two-egg pairs may stem largely from the effect of unfavorable environmental influences.

Measured by the same yardstick, concordance in one-egg pairs can often be expected to extend to specific personality features likely to lead to a criminal career (brutality, ruthlessness, predatoriness, irresponsibility), rather than to the kind of crime perpetrated. With criminality itself determined in many cases by constellational circumstances, discordance may occur even in one-egg pairs, where one partner "manages to keep within the bounds of law, while the other having once taken a criminal step remains outside the law and does not find his way back" [80].

As to behavior disorders that are not sufficiently explained on a situational or experiential basis, the list of conditions for which detailed twin data are now available is headed by the schizophrenic and manic-depressive types of psychosis [43, 45]. Since these two disorders do not occur interchangeably in the same twin pairs, they are assumed to be genotypically specific. The potentialities for a cyclic psychosis are probably associated with a subtle disturbance in a neurohormonal control mechanism which ordinarily protects a person from having harmful extremes of emotional responses. The concordance rates of two-egg and

one-egg twins (52 and 23 pairs, respectively) vary from 26.3 to 95.7 per cent.

Although the tendency to exceed the normal range of mood vacillation apparently requires the mutative effect of a single dominant gene (with a tendency to incomplete penetrance), the metabolic deficiency in a potentially schizophrenic person seems to be the result of a recessive unit factor. Varying clinical expressions of the disordered behavior pattern associated with the ensuing type of vulnerability to stressful experiences are probably produced by a number of modifying genes. Generally speaking, these variations depend on the type and degree of constitutional defense reactions that can be mobilized against the main biochemical (enzymatic) dysfunction. The concordance rates for two-egg and one-egg twins (based on a series of 953 pairs) are 14.5 and 86.2 per cent, respectively.

Involutional melancholia and other nonperiodic forms of depressive behavior in the involutional and senile periods have been shown by our twin data (62 one-egg, 142 two-egg pairs) to be unrelated to the manic-depressive group of disorders. There is an indirect link with the schizophrenic genotype through certain forms of emotional instability characteristic of schizoid personality traits. Other symptoms of maladjustment in the senescent period may arise either from gene-specific metabolic dysfunctions peculiar to the senium, or from graded differences in general health and survival values.

Twin studies have helped materially to focus attention on numerous obscurities in the etiology of all these disorders. Growing insight into the cellular, structural, and metabolic aspects of personality organization will gradually unfold a keener and more profound understanding of human behavior.

PROSPECTS FOR FUTURE TWIN RESEARCH

Although the research data gathered by means of twin studies are invaluable, a great deal of work has yet to be done, particularly in the behavioral sciences. Admittedly, progress in psychogenetics has been slow, and it may not be much accelerated in the near future.

Apart from a long delay in developing biologically oriented definitions and classifications, only relatively few research organizations have specialized in this area. Longitudinal twin family investigations are time-consuming, expensive, and often destined to be narrow in scope. In many instances, they may prove only that gene-specific determiners are essential in the etiology of a normal or deviant behavior pattern [6]. What they may never be able to explain, however, without considerable help from other disciplines, are the basic questions as to which genetic factors

and how many are involved, what their biochemical actions are, and how they interact with other genetic factors and with the environment.

Here a word of caution to workers in genetics seems indicated in regard to the current tendency (Allen) to assume the operation of a gene-controlled variation whenever some anatomical or chemical phenomenon is found in a population giving evidence of a behavioral deviation. With each new report of a possible organic correlate of a certain type of mental disorder, it would be well to bear in mind that should one of these findings stand up under scrutiny, it may turn out to be the consequence of a patient's disturbed behavior, rather than its gene-specific cause.

Of course, there is a tendency to oversimplify on the other side, too. What is sometimes overlooked in the formulation of purely psychodynamic theories is the fact that man is selective in the development of his own formula of adjustment. For instance, there is no simple relationship between a good home and normal behavior, any more than between a poor home and mental disorder. In the absence of genotypic identicalness, even pronounced similarities in physical and cultural environments, including having the same mother and father, fail to produce similar personalities with any degree of consistency.

The general belief that the behavior patterns of one-egg twins resemble each other chiefly because of unusual similarity in their early environments, both prenatal and postnatal, has yet to be substantiated. If confirmed by well-controlled twin studies, this knowledge will serve to strengthen any correctly formulated genetic hypothesis, either concerning normal behavior variations or specific types of mental disorder. In fact, it is possible that a disordered behavior pattern may result more immediately from some primary gene effect than can be assumed for a correlated anatomical defect.

Even if comparative twin studies concentrating on the search for biochemical correlates of basic personality variations have no spectacular success in the very near future, there can be no doubt about their potential value for the understanding of human behavior. Important leads may be obtained by a series of well-planned investigations focused on psychogenetic features that tend to be relatively constant and provide evidence of both homogeneity within families and a high concordance rate in one-egg twins. Most useful, also, will be any study (Allen) that succeeds in separating genetic and nongenetic components of personality development by tests for genetic linkage with blood groups and other easily identified genes.

Obviously, every one of these investigations would require a well-coordinated interdisciplinary research team, as well as the application of flexible research techniques lending themselves to optimal use of large

numbers of twin subjects and their families. In planning such projects, it would be advisable to concentrate adequate facilities and personnel in a few strategically situated research centers. Instead of encouraging needless duplication of studies that may be limited in scope and procurable support, it would be well to devote all regional resources to the organization of a few broad projects, and the training of men qualified to handle them.

Above all, more than demonstrating that hereditary elements play an important part in specific behavior variations, the main objective of psychogenetic twin studies should be to demonstrate precisely how this action takes place.

SCHEMATIC RECAPITULATION

The purpose of this contribution has been to determine the place of psychogenetic concepts in the over-all theoretical scheme of psychology as a branch of the human sciences.

The relevance of genetic data in the uses of psychology is based on the indispensability of this segment of knowledge in the understanding of every human function.

It has been shown that twin studies as a research tool are essential in demonstrating that heredity plays a vital role in potentializing all basic functions necessary for the establishment of health and the patterning of normal behavior.

In reviewing the body of information acquired by means of the twin-study method, the material was organized as follows:

General methodological principles. Twin studies are based on the regular occurrence of two genetically different types of twins (one-egg and two-egg). They are applicable in three different versions: (1) the twin-study method proper, (2) the co-twin-control method, (3) the twin-family method.

Determination of zygosity. The most reliable criteria in the comparative scheme of the modern similarity method are dermatoglyphic and hematological data. Reciprocal skin grafts can be used if these tests are indecisive in differentiating same-sex twins.

Ascertainment and analysis of twin samples. Adequate sampling procedures are important because generalized conclusions should not be drawn from observations made in single pairs or in an unrepresentative series of pairs. The statistics describing such a sample are computed from twin index cases rather than twin pairs. In the analysis of normal personality traits, twin data are expressed in terms of varying degrees of intrapair similarity or dissimilarity, rather than in terms of concordance or discordance (as used in comparing differences in morbidity risks).

Procedural limitations and advantages. The limitations are (1) imperfections of the research species, (2) imperfections of pluridisciplinary research workers, research methods, and research teams, (3) imperfections of quantifying methods for measuring meaningful personality differences in genetically similar or dissimilar phenotypes.

The advantages are (1) the humanness of the research subjects, (2) the interdisciplinary nature of the method, (3) its effectiveness as a sampling procedure and an economical substitute for a total population survey, (4) the facilitation of combined cross-sectional and longitudinal studies in a family setting.

Serial twin data on intellectual and personality variations. Two-egg twins of the same sex tend to differ as much in their personalities and behavior patterns as any siblings reared together or apart. Consistent similarity in basic personality traits is found only in one-egg twins and in them is not erased even by different environments.

Serial twin data on psychopathological variations. Differences between the two zygosity groups have been found in the concordance rates of the following conditions: psychoneurotic reaction potentials, male homosexuality, mongolism, convulsive disease, schizophrenia, manic-depressive psychosis, and involuntional psychosis.

Prospects for future twin research. Twin studies are expected to make further important contributions to the identification of the nature and action of genetic components in normal and abnormal behavior patterns.

In conclusion, it may be said that the techniques of psychogenetics establish cohesion between two coordinate sciences, psychology and human genetics. These two sciences belong together and have to pull together in advancing the understanding of behavioral variations in man.

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A. PERSPECTIVE ON SOCIAL PSYCHOLOGY

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INTRODUCTION

The data and problems of social psychology have barely been identified, and the questions that have been studied in the recent period refer to only a small part of the field. In these circumstances it would be premature to propound a formal system of the phenomena in this region. In any case, I cannot lay claim to the possession of such a system. The task that faces the student is rather how to proceed during the first stages of exploration in a difficult and uncultivated territory.

Lack of strict knowledge does not, however, imply the absence of a theoretical direction. Indeed, the investigator in this area begins with a strong initial orientation. A long tradition of thinking about human nature precedes him. He brings to his subject matter the doctrines of man prevailing in his time and the notions derived from his own experience. He also draws upon the contributions of general psychology. These sources of observation and of conceptions, which precede investigation and guide it, constitute a kind of theory, which might be called presystematic.

Social psychology still works largely with borrowed conceptions that have not been sharply tested on its own grounds. It has not yet achieved an independent outlook on its data and problems. In what follows I propose to examine a few important assumptions and to trace the effects they have exerted. My particular theme will be the relation that has prevailed and that should obtain between general and social psychology.

No one will question that social and general psychology have much to do with each other. But the relation between them is not simple. A few historical remarks may serve to introduce this discussion.

The main aim of social psychology, which is to further a theory of human nature, has a long history. But the way in which it proposed to go about realizing this aim was unprecedented. The new social psychology was committed to seek for answers by means of the methods of science, of controlled observation, where possible of experimentation, and to thus bridge the gap between our understanding of natural and social events. The idea of such a discipline was the culmination of a series of great changes in thinking about nature and society.

At the same time, this movement was in part a reaction against the narrowness of a general psychology which found no place in its scheme for some of the most essential properties of men. The scientific psychology from which it sprang restricted its observations to the relations between an individual and an environment that strictly excluded other persons; it was not concerned with relations between persons or between persons and groups. The movement toward a social psychology represented an insistence that these major and neglected parts of human psychology be taken seriously. It stood for the belief that no psychology can be complete that fails to look directly at man as a social being.

There were a number of reasons for this restriction in the scope of psychology during the first fifty years of its existence. Investigators had no techniques for the exact investigation of psychosocial phenomena; and like most humans, they preferred safe and tested procedures. Probably they also feared the complexity of social events, and saw little hope of studying them in the manner that their conception of science demanded. The belief that this area was outside the range of experimental analysis discouraged further interest.¹

It is noteworthy that the new social psychology did not start with a commanding discovery which could guide thinking and investigation, and furnish an answer to the preceding doubts. It was rather the expression of a hope that the procedures of observation and experimentation were not limited to selected phenomena within our field, and of a determination to demonstrate this faith in practice.

Let us now consider the ways in which the relations between general and social psychology have been conceived. Since there has been little explicit discussion of this question, it will be necessary to infer the relevant views from existing trends of investigation and theory. At this point one finds quite diverse emphases.

1. There are those, both within and outside social psychology, who

¹ Consider, for example, the position that Wundt espoused in the *Völkerpsychologie*.

hold that it is an applied discipline, that it, involves little more than the application of some of the laws of "nonsocial" psychology to more complex data. There are two main grounds for this position. First, it is clear that the same principles of human functioning are equally at work in the social and nonsocial settings. The principles of learning or of motivation discovered by the procedures of strictly individual investigation must also be valid under social conditions. Few, if any, will dispute this thought. The other ground of this position is less often stated openly. This is the belief that all principles of psychological functioning will be discovered in the nonsocial setting. This belief follows from the further assumption that the basic data of psychology are those that concern the most elementary phenomena, and that more extended phenomena are complications of these. This was the view of Hull [5], who held that social and moral data can be derived from a knowledge of basic learning principles. It is also the position that Skinner [10] maintains. It seems to follow that social psychology (and perhaps also the other social disciplines) has no basic theoretical problems of its own. In apparent support of this belief is the further observation that the term "social" does not designate a particular psychological function, such as memory or perception, but rather includes all. On these grounds the following division of labor is recommended: Let general psychology discover the principles, and let social psychology extrapolate them. This has continued to be a leading emphasis within social psychology itself.

There is much to be done in the way of relating what is known in general psychology to social data. But the second assumption simply denies, with not even an attempt at proof, that the urgent problem is to advance our meager understanding of the complex cognitive and emotional operations on which social events rest. An enterprise that starts on such a precarious footing is not likely to rise above its source, or move toward a coherent body of knowledge. Furthermore, will not those who are attracted to psychology prefer the challenge of building the foundations to the work of premature and dubious application?

2. There is also a more pragmatic trend in social psychology. In every area of study there is much to be done in the way of observation and description, and of establishing particular formulations, without the necessity of facing immediately their relations to more fundamental propositions, in the present instance to those of general psychology. From the standpoint of practice there is much to be said for this direct approach. Work done under such auspices can be useful; surely we can profit from increased knowledge of shifts in public opinion, of interrelations among opinions, of the distribution of prejudice, of relations between leaders and followers. In the long range, however, it reveals a weakness. It assumes that the study of social-psychological events poses

no problems of basic clarification, that it is enough to have innumerable questions one can ask about social behavior and experience and to possess rules of method that secure the objectivity of one's findings. The danger of this position is that it creates a technology before there is a science.

3. Another view, and one that I favor, may be stated as follows. Social psychology is not an applied discipline. Its task is to contribute to a theory of the psychological functions. This cannot be accomplished only by studying individuals in exclusively individual settings; it requires also the direct investigation of happenings between persons, or the extension of observation beyond the limits that experimental psychology had traditionally imposed. We cannot have a tenable theory of emotions or motives if we do not study those that refer directly to persons; no procedure of extrapolation will suffice for this purpose. It would be most unusual if we had discovered the key to the central properties of men without having to go to the trouble of studying them in the only setting in which they can be observed. I hold that social psychology is part and parcel of the enterprise of general psychology.

It follows that social psychology is under obligation to make its own contribution to the persistent problems of general psychology. This statement surely does not describe what has happened during recent decades, and may appear to be an expression of an unrealistic hope. Let us, however, push the conclusion to its limits. The paradox of this conclusion is that it demands an independent, critical examination of the basic issues of psychology in the light of the data of social behavior and experience. It asserts that if the need for a social psychology can be traced to the lack of an adequate base in general psychology, the gap cannot be remedied by a wholly derivative discipline.

Much that has happened in this field is, I would say, the result of a failure to take this possibility seriously. Although social psychology was partly a revolt against the existing order, it nevertheless grew up in the shadow of general psychology, from which it borrowed its concepts and procedures. It has adopted existing formulations about the operations of motives, emotions, thinking, and learning, which were derived mainly from the study of strictly individual, indeed non-human, behavior, and has systematically confined its investigations within the prevailing frame of concepts. It will be my contention that this dependence has been responsible for the neglect of some central questions and for a limited horizon.

These comments follow from my estimation of the achievements of social psychology, especially during the recent decades which were marked by an unusual expansion of interest and activity. I come away with two distinct impressions. First, one must record a number of gains. There has been a sharpening of problems, a growth of techniques; and some additions to the body of knowledge have been secured. An opti-

mistic assessment of the situation is therefore not difficult to reach. One may admit that the advances have been limited, but this is not unusual at certain stages of a science. Above all, many will be inclined to stress the power inherent in empirical procedures to replace vague generalities with tested knowledge. "Better a minute truth than a grand half-truth" expresses fairly well the prevailing spirit. The second impression goes in a quite different, indeed disturbing, direction. There is something puzzling about today's social psychology. Much careful and conscientious work is going on, but it is hard to avoid the conclusion that it has not been fruitful in fundamental conceptions. It has not produced many instances of decisive research, and has not perceptibly deepened our knowledge of man, despite the very considerable effort that has undeniably been expended.

In what follows I have chosen to discuss in this light the adequacy of the steps we have taken in studying a few problems that have been our chief concern.

A QUESTION OF PERSPECTIVE

Let me begin with a very general point. Each discipline possesses its special spirit, which consists in a particular way of viewing its data. The study of man as a social being also requires its own perspective, which must start from some conception, however tentative, of what it is to be human. The subject who sits for our portrait, *Homo sapiens*, is, to be sure, only dimly visible to us, and we will probably not produce a good likeness of him soon. But even a first sketch requires some apprehension of his dimensions. These would have to include as a minimum that he possesses unusual intellectual powers, that he can act with reference to ideas and ideals of right and wrong, even when he violates them; that he has a need to surround himself with objects that are attractive. To realize that these are part of the "human minimum" is essential as a point of departure for thinking about man.

Today there seems to be little evidence of this awareness. The question, what it is to be human, which should be of more than passing concern, has virtually disappeared from discussion. At the same time, the man of social psychology turns out to be a quite dwarflike creature. One would not often suspect that we were talking of an organism capable of keeping or betraying faith with others, in whose history religious beliefs have played quite a part, who can cry out for justice. It is hard to escape the conclusion that primitive notions of what it is to be human have guided thinking and investigation.

It is not my intention to say that social psychology should have prompt answers to the most difficult questions. I am suggesting that a

certain broadmindedness about human ways is necessary for thinking of the required scope. We cannot be true to a fragment of man if we are not true, at least in a rudimentary way, to man himself. The consequence of this neglect is aimlessness in investigation and fragmentation of knowledge. The danger of ignoring relevant matters is that less relevant matters usurp their place. Actually many of us today do have a point of departure which follows the curious doctrine that man is directly descended from the white rat. A limited perspective can have the effect of trivializing a subject. Those who deepen our understanding help us, as a rule, to see more in a given region than we had suspected to be there; in social psychology one often has the uncomfortable feeling of being invited to see less than we thought was before us. One wonders whether a failure to appreciate the relevance of history and literature, of art and religion, to human affairs is the prerequisite for the pursuit of social psychology.

We will now abandon general statements to consider how social psychology has dealt with several particular problems, and the role that general psychological theory has played. I have chosen three problems for illustration, although others might have served equally well.

THE CONTROVERSY BETWEEN INDIVIDUAL AND GROUP PSYCHOLOGY

Let us consider the fate of a problem in motivation which belongs at the very center of the discipline. It is of considerable consequence for any social psychology to establish the grounds of concern for the welfare of other persons or groups, and how these are related to the concern individuals feel for their own welfare. What has happened to this problem? It has virtually disappeared or been interpreted out of existence, one suspects for no better reason than that it has not attracted the interest of general psychology, which could offer little guidance at this point just because it had excluded the relevant phenomena from view.

It may be well to trace the course of thinking about this question historically, since it is connected with an important set of problems that came to a head when the first steps toward a social psychology were taken. It is not my intention to examine the evidence, but rather to ask what circumstances decided the way in which social psychology went about defining its task.

Our story begins with the controversy centering around the conflicting claims of individual psychology and of group psychology. The issues entering into this well-known discussion have not, I believe, been fully understood. This is mainly because some of the best-known proponents of a group psychology were driven to the expedient of postulating

a group mind which, as every proper social psychologist today has learned, is a mystical notion. Actually this was a relatively superficial feature of the dispute; lying just underneath the surface were the serious issues, still very much alive today.

Why was the group mind thesis put forward by able men? It started with a serious problem—with the clarification of group characteristics and group membership. It had its roots in a formulation by no means strange today, namely, that one cannot understand an individual by studying him solely as an individual; one must see him in his group relations. Thinkers like McDougall were especially impressed by the phenomenon of group spirit. Members of organized groups, they held, were guided in their sentiments and actions by the idea of the group. That is to say, when acting as group members, they were orienting themselves to a reality vastly more powerful than themselves, and one that was able to command their devotion and interest. Since McDougall and others saw no way of deriving these great social forces from the properties of individuals taken separately, or from the aggregation of individual characteristics, they felt compelled to postulate a group mind.

From within psychology there came a sharp response to the group mind doctrine. It was clear that there can be no psychology other than that of the individual. What, then, is social psychology? The answer was stated most clearly in the 1920s by F. H. Allport.

Allport went far beyond the rejection of a group mind and the assertion that social psychology is the study of individual behavior (and experience). This first step only cleared the way for the second, and far more important, formulation—that concerning the nature of individual behavior and experience. There is, to begin with, the following account of

. . . the essential formula for behavior: . . . (1) Some need is present in the organism, such as the necessity of withdrawing from weapons injuring the body, or the need to obtain food, or to secure a mate. (2) The organism acts: it behaves in such a manner as to satisfy the need [1, p. 1].

This statement cleared the way for the characterization of the subject matter of social psychology:

Social behavior comprises the stimulations and reactions arising between an individual and . . . his fellows. . . . The significance of social behavior is exactly the same as that of nonsocial, namely, the correction of the individual's biological maladjustment to his environment. . . . In and through others many of our most urgent wants are fulfilled; and our behavior toward them is based on the same fundamental needs as our re-

actions toward all objects, social or non-social. It is the satisfaction of these needs and the adaptation of the individual to his whole environment which constitute the guiding principles of his interactions with his fellow men [1, pp. 3-4].

It was this formulation (representative of the then dominant general psychology) that won the day. If one abstracts from details, it still expresses the spirit and embodies the assumptions of virtually all contemporary social psychology. For this reason alone it deserves the closest examination.

The main point may be restated as follows: all that a person does, all that he feels and thinks is determined by the tendency to gain satisfaction for *his* needs. Allport was thus asserting a basic proposition about human motives: the egocentric proposition. It is here, in this characterization of what an "individual" is, that we must see the root of Allport's disagreement with group mind theorists. The latter were seeking a way to express what they sensed to be an essential feature of social life—the capacity of individuals under some circumstances to transcend their own particular interests and to act in the interest of their group. The current general psychology saw this as a false problem for two closely connected reasons. It denied the reality of groups on elementaristic grounds, and could only conclude that the idea of the group was an illusion of individuals. But, in addition, it defined at the outset an all-inclusive property of human motives in such a way as to require a re-interpretation of all group sentiments in terms of self-centered motives.

We can see that there was indeed a big difference between this psychology of the individual and the group psychology that some thinkers considered necessary for a complete account of human behavior.

This psychology of the individual also defined in a particular way the scope of social psychology. The latter was to be one small corner of psychology which, instead of studying the usual stimuli—weights, lights, sounds—dealt with social stimuli. The other person, too, is a stimulus. But if "the individual in the crowd behaves just as he would behave alone, *only more so*" [1, p. 295]; if it was the "individual citizen" who stormed the Bastille, one could only conclude that social phenomena were not of major theoretical interest.

As often happens in the history of thought, conflicting doctrines may be at one in the most important assumptions. This was the case with the group mind and the individualistic formulations. The problem they were facing was that of reconciling an apparent antinomy: that men are social beings and that they lead an ultimately private existence. Both failed to describe the process that overcomes this paradox and that is responsible for group phenomena at the psychological level. Social action requires

that the individual participant be capable of representing to himself the situation that includes himself and others. These individual representations contain, in cases of full-fledged interaction, a reference to the fact that the others also possess a corresponding view of the situation. These similar and mutually relevant representations in individuals provide the equivalent of what group mind theorists sought and individual psychologists denied. [See 2, chaps. 5, 10, 11 for a fuller account.]

In short, social action in humans rests on an unusual kind of part-whole relation, in which the structure of group conditions is (at least in part) represented in the individual, who can only in this way become a participant. Consequently a group does not need a central headquarters, and a social act is not reducible to the model of action between person and thing. Neither of the contending doctrines saw this solution.

In the recent period, some social psychologists have come to adopt the view that psychosocial events are based on such similar and mutually relevant representations in individuals. They have accepted it mainly, however, with reference to the cognitive side of our functioning, while retaining intact an exclusively egocentric conception of motives. This procedure fails to draw the full consequences of the formulation. If the representations by individuals of their relation to others are to issue in action that is not chaotic, they must have mutual reference in some degree. Is this possible if each person as a rule sees a given situation solely from the standpoint of his needs? The hypothesis we are forced to consider is that mutually relevant fields are not consistent either with a purely egocentric account of cognitive or of motivational events.

An error in thinking and in psychological analysis made it appear that there is no alternative to the egocentric formulation. To be sure, mothers have been known to starve in order to feed their children, and persons have endangered their lives for others. There are, then, actions which at least appear to be quite the opposite of self-centered. But contrary data are weak reeds against winds of doctrine. The ready answer stood at hand that the need to help others is egotistic because one enjoys it; it is egotistic to enjoy one's unegotistic action. The error of this too clever argument lies in the refusal to face and explore an intelligible alternative, in the failure to admit as a legitimate possibility that under certain conditions the place of egocentric needs in the individual's brain is not functionally at the center, but that egocentric needs may themselves be localized in the brain as dependent parts of a wider situation.

The issue is, of course, a factual one; conceivably the most seemingly disinterested action may be the work of calculation and self-interest. Admittedly a decision about this question is difficult, perhaps mainly because much of human behavior is a function of both kinds of vectors.

But science does not justify dogmatism when a problem is beset with difficulties.

The point I wish to make is that social psychology has passively accepted a prevailing view, of the greatest consequence to its subject matter, without taking the trouble to consider observations and distinctions which should be the starting point for thinking. It ignored observation in favor of a theory that replaced observation. It is true that if the secret police knock on a door in the dead of night, the neighbors may be alarmed for themselves. Is it equally clear that they have no concern for the victim? Where is the stringent evidence that a sense of injustice at the mistreatment of another plays no role at all?²

An error about a fundamental proposition such as we have been discussing, one which concerns the possible kinds of relations between persons, is bound to have consequences. If group events require that persons should feel and act as part of their group, and not solely as the center of happenings, then the error is of the same magnitude as that of a doctrine that would deny the self-centered tendencies of persons and describe society as an exercise in altruism.

This omission has been responsible for a limited and lackluster treatment of central topics. To it we should trace the accounts of group belonging as a kind of business transaction obeying the motivational principles of a watered-down Hobbes; one hardly finds an inkling in these studies that being in a group can be either an enjoyable or a responsible experience. It must be credited also with the formulations about attitudes that give the lion's share to their opportunism, and with the neglect of those attitudinal forces that take possession of the person, including the part we call his self. It has dampened concern with those values that appear to be determined chiefly by objective requirements.

A reexamination of a range of problems in the light of this issue could prove a challenging task. One may safely say that if social psychology is to make progress, it must take into account the vectors that make it possible for persons to think and care and work for others. It will need to find a place for the capacity of persons to relate to the needs of a situation so that they become the needs of the person; it will have to acknowledge that the desire to play one's part meaningfully may at

² It might avoid misunderstanding of what has been said to add that I have not questioned the power of egocentric needs. The preceding discussion might become more palatable if it were pointed out that action determined by the interests of others is not necessarily wise, nor are its consequences unflinchingly beneficial. Indeed, actions in the interests of one's immediate group may be aggressive and destructive to outsiders. With these remarks I may perhaps be exonerated from defending a "soft" doctrine.

times be strong, and that it may even make sense to speak of an individual's desire for the realization of a better society.

Before leaving this topic two further remarks may be in order.

The first concerns a reformulation of the content of mutually relevant fields in the light of this discussion. One dimension of the consensus they produce has to do with the intellectual assessment of a given situation: There can be no concerted action between persons unless they have cognitively structured the given conditions in somewhat similar ways. (Included in the given conditions are the actions and intentions of the participants themselves. There must be a similar way of understanding both the material properties of the environment and the psychological properties of the participants.) But social action cannot get started on the basis of an intellectual appreciation of such data alone; there must also be a degree of affective consensus with respect to the aims and needs of the participants. The traditional view describes this second aspect of consensus as a concurrence of ego-centered orientations. The alternative here discussed is that the need or goal of one person can, given certain conditions, arouse forces in another person toward fulfilling them, without exclusive reference to the latter's "own" needs. This relation to another, when it is mutual and known to be such, seems to me to be an indispensable condition of mutual trust and of group coherence.

The next remark concerns the relation between the mutually relevant fields of individuals and a total group process. It follows from what has been said that group events lack the solid monolithic structure which they phenomenally give us. A group is not a single physical system; it does not possess the kind of unity that belongs to a thing or an individual. For example, the "body of medical knowledge" does not have a single locus; it is distributed among many individuals and includes what is to be found in libraries and hospitals. Phenomenally we objectify group events to a high degree; it would almost be right to say that there are group minds, but that they exist in individuals, and that there are as many group minds as individuals in a group. Also, a group event includes more than the psychological activities of its members. In addition to the environment—natural and social—and the activities of its participants, it includes the structure of initiated events and the regularities these exhibit, whether or not they are known to the participants. The study of these regularities is the problem of other social disciplines, such as anthropology and sociology.

At this point it seems best to stress the distinction between such a total group process and the psychological components of it. Recently, Sears has suggested that psychologists who study social behavior and personality may be at fault in limiting their view to a single individual,

and proposed that we take the dyad as the minimum unit of action. "A dyadic unit," he says, "is essential if there is to be any conceptualization of the *relationships* between people . . ." [9, p. 478]. One can only welcome an effort to repair the individualism of an earlier period, and to arrive at an equivalent of group realities in a behavioristic way. At this point it is necessary to enter a reservation in favor of a more individual and more complex approach. Here one should refer to three dyadic structures. One is the inclusive sociological formation; it is right to say that we must keep it in view if we are to follow the course of action which individuals jointly produce. But it is necessary to distinguish between this inclusive structure and the contributions to it by each of the participants. At this time, I know no way of describing the psychological and sociological happenings within a single conceptual formulation.

THE DATA OF SOCIAL PSYCHOLOGY

The place of experience in human social psychology has been settled in a purely practical way. It is not possible, as a rule, to conduct investigation in social psychology without including a reference to the experiences of persons. The investigator must, for example, take into account what the person under observation is saying; and such utterances have to be treated in terms of their meaning, not as auditory waves, or sounds, or "verbal behavior." One can hardly take a step in this region without involving the subject's ideas, feelings, and intentions. We do this when we observe people exchanging gifts, engaging in an economic transaction, being hurt by criticism, or taking part in a ritual. The sense of these actions would disappear the moment we subtracted from our description the presumed mental operations that they imply. This requirement to include mental happenings in an account of human activities, one which the social disciplines generally must observe, should have spurred an examination of the systematic properties of experience and their relations to action. Instead we find that the situation has been accepted half-heartedly, and that its implications have not been explored with care.

To see how the problem of experience arises in this area, let us consider how we follow the actions of persons. The first observation we make is that persons invariably describe the doings of others (and their own doings) in *psychological* terms. We say that a person sees, hears, prefers, demands. This is also the way we describe happenings between persons; thus we say that one person helped another, or distrusted him. These are the ways in which we order the actions of persons whom we observe to be living and conscious.

An organism that relates itself to the environment in this manner is

observed to act in it in a special way. Heider [4] has pointed out that we observe persons to produce effects intentionally. They relate themselves to the environment by wanting, by being interested, by liking, by understanding. In the case of persons, a cause is not merely a preceding state of affairs; it is a state of affairs as known or understood by the actor. An effect is not merely a later state of affairs; persons make things happen, or intend them. The movements of persons thus gain the status of actions.

With these is connected the most significant property of persons: that we experience them as capable of responding to *us*. They alone can understand our thoughts and feel our needs. Therefore they become the adequate objects of praise and blame. It is only to beings having these properties that we can relate ourselves by cooperation and competition, by affection and hatred, by admiration and envy. It is in these terms that we follow the actions of a friend, the happenings in a play of Sophocles or in the life of a primitive society. Events of this kind form much of the content of the mutually relevant fields of persons discussed earlier.

From the standpoint of a powerful tradition there is something suspect about these everyday observations. The main charge is that they do not speak the language of science. They refer, of course, to what the other person does, but they are not simply descriptions of the movements he carries out; they are not simply statements of the geometrical displacements of persons and things. At this point the temper of one theory in general psychology recommends the wholesale dismissal of the layman's concepts and language when we turn to investigation. His accounts are, it is said, contaminated by the inclusion of subjective conditions that are not observable because they are not describable in terms of physical operations. This formulation, although it has not originated in social psychology and would, if taken seriously, drastically curb further inquiry in this field, has nevertheless left a strong impress upon it.

The following illustration may clarify the point at issue and the difficulties it raises. Among his prescriptions for a psychological utopia, Skinner includes the training of children to tolerate frustration, and proposes an ingenious procedure [10]. He would occasionally have the children in his utopia come to their meals, but delay their eating for a few minutes while they watch some delicious specialties that had been prepared for them. Gradually he would extend the period of deprivation, the intention being to instill self-control without injurious consequences. As Skinner describes the procedure, it is exclusively an affair of timing responses to given physical conditions. One may be permitted to wonder whether the children, however carefully reared, might not take a different view of the proceedings. Are they not likely to wonder what their caretakers are up to? And will not the outcome depend on the answers

the caretakers give? If it should come into the children's heads that the caretakers are malicious, it might go ill with the effects of the scheduling. On the other hand, if the children trusted their mentors, and particularly, if they credited the caretakers with the meritorious motive of instilling self-control without injurious consequences, the discipline might prove more successful. The elimination of any reference to these internal events amounts to a failure to describe the relevant conditions with any adequacy.

The problem arises whenever we refer to action between persons. A determined effort to treat the relation of frustration to aggression in non-experiential terms could not avoid defining frustration as damage attributed to a particular instigator [3]. In a recent discussion, while again insisting that we give priority in psychological investigation to action, on the ground that it alone is public, Sears uses the following example: “. . . if a child wants to be kissed good-night, his mother must lean toward him affectionately and kiss him. He, in turn, must slip his arms around her neck and lift his face to her receptively” [9, p. 480]. This sentence is surely not an unadulterated description of geometrical displacements; it does not supply the kinematics of affection, or even of slipping, lifting, or leaning.

The sources of disagreement about the place of experience in psychological investigation are too deep-seated to be dealt with summarily. We will consider only a few points most relevant to this discussion. In the first place, there are certain misconceptions to be noted. It is often asserted that actions are public but experiences are private, and that therefore the latter have no place in science. Surely there is an error here. The observation of actions is part of the observer's experience. Indeed, the same writers who make the first assertion as a rule subscribe to the second. There is thus no ground for calling actions objective and experience in general subjective. This confusion has been discussed by Köhler [7]; it is not necessary to repeat it in full.

There is another, seemingly more substantial reason for the difference in status accorded to behavior and experience. We can, it is asserted, arrive at a high degree of consensus about behavior, but not about our respective experiences. (In the light of the preceding point, this assertion claims that some kinds of experience produce consensus superior to others.) In particular, the conclusion is drawn that the positions and displacements of objects in space provide the only dependable consensus.

This conclusion will not withstand scrutiny. There is often, indeed, excellent consensus about events which, according to the preceding view, are unobservable or incommunicable. The size of an afterimage, or the experience of a causal connection, can be described with a lawfulness that permits the study of their dependence on inner and outer conditions.

This suffices to qualify the data of experience as data of science. Instead of pursuing this fruitful direction, the physicalistic doctrine has attempted to demonstrate that the data of experience can be treated as verbal behavior. It can be shown, though, that the occurrence of an experience is not the occurrence of a verbal response.

It is not probable that the preceding formal arguments are actually responsible for the efforts to eliminate all reference to experience from human investigation. To locate the sources of difficulty we must look elsewhere. Perhaps the most decisive assumption is that the data of experience are not functionally connected with, and provide no help toward understanding, other concurrent events in the individual. This belief is contrary to what we know about the relations of mental and physical events. The physicalistic program also derives from the elementaristic assumption that the properties of action can be exhaustively described in terms of component movements. Were this the case, it might indeed follow that the data of experience have a limited place. But human actions are extended spatio-temporal events having a definite form, and we cannot describe them without reference to goals, and to means related to goals. These characteristics of actions are lost from view when we concentrate on their most minute components one at a time, just as we lose the quality of a form or a melody when we attend only to its smallest components. It has been convincingly shown that the most consistently behavioristic procedures do not actually deal with stimulus and response in these elementaristic terms [6]. Behaviorism must and does include action; it grants in practice all that is needed when it speaks of "running toward a goal," or of "pushing" and "pulling."

What is the relation of the distinction we have tried to draw between movement and extended action to the data of experience? First, the data of experience point to, and thus help identify, the conditions in the environment to which we are responsive. Second, the data of experience provide hints concerning the internal events that steer action.

Those who dream of an objectivistic social psychology fail to realize that such a program can be pursued only if the data of experience are taken into account openly. We are today far from able to describe the most obvious and the most significant social acts except in the language of direct experience. What are the event-sequences corresponding to such data as "the mother praised the child," or "the boy refused to heed the teacher"? And how much more difficult is it to describe the actions of "keeping a promise" or "telling the truth"? Not only are we at a loss to report adequately the actual sequences of such events; there is often no fixed set of actions corresponding to them from occasion to occasion. How, then, could we go about locating and identifying the relevant action patterns unless we were guided to them by the distinctions of

direct experience? Even if we succeeded in such a description, it would remain a foreign language until it was translated back into the terms we ordinarily employ. At this point the categories of the layman are actually in advance of those that formal psychology today has at its command. He has, without the benefit of a psychological education, identified some of the conditions and consequences of action. To be sure, these categories are descriptive, not explanatory. Also, everyday thinking identifies them in a shorthand, summary manner, which must be replaced with far more detailed description. But to counsel their abandonment is to give up the prospect of social understanding, and to bar the very advance toward which we aim.

Throughout this discussion we have noted the prevalence of the assumption that one can move directly from a few selected notions, derived mainly from the study of lower organisms, to an account of human actions, and that the latter require no concepts appropriate to them. Actually, concepts such as conditioning, stimulus generalization, extinction, response strength, secondary reinforcement, and reinforcement itself have as a rule been extrapolated to social settings without a serious effort to demonstrate their relevance under the new conditions. In this passage the terms lose the relatively clear sense they initially have. The extrapolations become largely verbal; we are not the wiser when the translation has been accomplished. This procedure, instead of increasing objectivity, often conceals distinctions long familiar to ordinary observation. It discourages the exploration of those differences between persons and things, between living and dead, that are at the center of the subject. It creates the curious presumption that hardly anything new remains to be discovered in a field that has barely been studied.

The conclusion we have reached could have been arrived at more simply. Every field of inquiry must begin with the phenomena that everyday experience reveals, and with the distinctions it contains. Further inquiry may modify our understanding of them, but the phenomena themselves will never be displaced. In social psychology the phenomena with which we begin are qualitatively diverse and the description of them prior to formal investigation is consequently of particular importance. Let us, for the purpose of this discussion, assume that concepts such as "role," or "internalizing of values," have a place in social psychology. They must then be shown to apply to the ways in which the actors, who are often innocent of these notions, see their situation. The latter act in terms of conceptions and emotions peculiar to them—in terms of envy and trust, hope and suspicion. The concepts must be relevant to this world of appearances, which are among the indispensable data of the field. Those who avoid this initial phase of investigation run the danger

of placing themselves in the position of the hero in Greek mythology who was shorn of his power the moment he lost contact with mother earth.

Having said this, it is necessary to add that a psychology based on phenomenal data alone must remain incomplete. The latter are always part of a wider field of events within the individual; any order they may reveal will be partial unless completed by a more comprehensive knowledge of psychological functioning. We need, therefore, an objective psychology that will account for the structure of experience. It also follows that the examination of experience should not become either an aimless or an endless occupation. It should strive to issue in inductive inquiry and, where possible, experimentation.

These conclusions should not hide the difficulties that face investigation in social psychology. In one area of psychology, that of perception, the reliance on phenomenal data has proceeded fruitfully. Such investigation possesses one indisputable advantage: phenomenal events are studied in their dependence on stimulus conditions which are describable in terms of well-understood physical operations, and in relation to internal processes that are also described in terms of natural science categories. This advantage deserts us in most parts of social psychology. Here we must abandon, at least for the foreseeable future, the yardsticks of physics, and describe both the stimulus conditions and the effects they produce in psychological terms. Since the dimensions of these events are frequently complex and only vaguely known, the prospect of discovering clear functional relations may arouse skepticism. It would be misleading to minimize the difficulties, but it would also be premature to prejudge the outcome. This is a challenge social psychology must accept.

THE STUDY OF SOCIAL INFLUENCES

The final problem in illustration of my theme concerns the study of social influences. Social psychology recognized early the importance of this area and has energetically investigated it.

My comments at this point follow directly from what has preceded. Social influences differ from other conditions—such as heat and cold, light and dark—mainly in this respect: they are experienced to have their source in persons. They refer to the purposes, attitudes, and thoughts of others. It follows that we cannot talk sensibly about the effects of social conditions without specifying their cognitive and emotional content. One responds differently to the same action, depending on whether it is judged to be friendly or unfriendly, deliberate or accidental, serious or frivolous. The first consequence of this observation is

to underscore the fact that a nonpsychological definition of the stimulus conditions and their effects may leave the essentials out of account.

In an effort to bring order into this area investigators have leaned toward a simple and seemingly comprehensive conception of social influences. It has two main properties. First, it assimilates all group influences to the construct of constraint or pressure, and all their consequences to the construct of conformity. Second, it applies a general proposition about the operation of rewards and punishments to account for the observed effects.

There is good reason to be skeptical of the assumption that all instances of social determination are of the same kind, differing only in detail. In particular, it is a serious error to equate social determination generally with constraint or pressure. I am more impressed by the need to discriminate the kinds of social influence, and will attempt a few remarks concerning this point.

1. One of the great effects of social experience is to produce consensus about considerable reaches of the environment. We discover in the course of action that many of the reports of others are validated in our experience, and conversely. It is on this foundation that action between persons occurs. This form of consensus is largely the product of observation and judgment. By no stretch of the imagination can it be equated to the operations of constraint and conformity.

It is a curious fact that contemporary social psychology, which is not inclined to underestimate the effects of social conditions, has seen no problem here. The assumption has been that the individual's unaided experiences suffice to validate the basic features of the physical environment, and that social effects enter only at the point where he must take the reports of others on trust. This assumption (which leads necessarily to a subjectivistic account of social determination) ignores what may be the crucial point, namely, that we accept the reports of others in lieu of direct experience only because we have at other times received the most direct proof of the validity of their reports, that only on this basis do we extend the area of consensus into what is not directly perceptible.

2. There is another, and quite different, range of operations to which it makes no sense to apply the notions of constraint and conformity. The actions of persons exert emotional effects upon us. I do not know what it would mean to say that one is afraid or envious out of social pressure.

3. Each social order confronts its members with a selected portion of physical and social data. The most decisive feature of this selectivity is that it presents conditions lacking in perceptible alternatives. There is no alternative to the language of one's group, to the kinship relations it practices, to the diet that nourishes it, to the arts it supports. The field

of the individual is, especially in a relatively closed society, in large measure circumscribed by what is included in the given cultural setting.

These conditions produce a kind of socially generated reality, which is as much part of the environment as topography and climate. They of necessity shape the individual's expectations, needs, and character, often perhaps irreversibly. The consequences are more fundamental than those generally dealt with by empirical investigation which has been concerned mostly with modifications of already formed views and needs. From the psychological standpoint the significant feature of these conditions is their monopolistic character, or the absence of known alternatives. The responsiveness to such conditions may be fundamentally different from what is nowadays called conformity.

4. We finally come to constraint and conformity proper. These highlight another aspect of social determination. They refer to conditions that create a conflict between tendencies in the person and the forces extending from the social field. They differ from the instances mentioned previously in that they present an issue and involve a choice among alternatives. As soon as this is the case, the happenings are a function of the conflicting alternatives. The problems in this region concern the operations of conformity *and* independence, not of conformity alone.³

Current thinking has assigned a particular interpretation to the constraint-conformity operations. It relies on a general proposition about rewards and punishments, derived from observations of lower organisms, and silently assumes the egocentric axiom which was discussed earlier. It also presupposes that constraint and conformity are psychologically homogeneous. Actually there is a crying need to discriminate among different kinds of constraint and conformity.

Prior to formal investigation we can enumerate a few.

1. There is a range of constraints that persons accept because they seem just, even if not pleasurable. If so, it follows that the study of social influences cannot be pursued at some crucial points in the absence of a psychology of ethics. Current thinking and investigation has ignored this fundamental basis of social discipline. The belief that the vectors present in the experiences of right and wrong are merely the reflections of social influences, a view that is not wholly intelligible, has silenced questions that should be raised.

2. Action in line with social demands, even if their rightness is in doubt, is, of course, a fact of considerable importance. It is customary to refer here to the role of expediency, but it may be more illuminating

³The forms of social determination described above need not, of course, occur in separation. They may be, and probably often are, relevant to the same set of events.

to consider the varieties of motivation that are implicated. (*a.*) Fear of consequences, the favorite formula for the explanation of social sanctions, is a potent force. But it may be necessary to strip it of its simplicity. It is pertinent to ask why the anticipation of punishment is effective in some circumstances, not in others, with some persons and not others. (*b.*) Loyalty to the group. A worker may be convinced that a call to a strike is unwise, but will lay down his tools because he believes that the welfare of his union will be best served by his acquiescence. This quite human and powerful attitude seems not to have found credence in our psychology. (*c.*) Another potent source of conformity is indifference, the failure to see an issue, and the pressure of other concerns which are presumed to be of higher importance. (*d.*) One may even conform in order to exploit others for one's own ends.

The preceding examples have not included those effects that involve a change of evaluation of the given data. They have left out of account the power of social conditions to alter established judgments and convictions. Again it is unlikely that a single abstract formula will do justice to operations that range from simple cognitive inference to the most complicated changes of emotions and attitudes. Inquiry will need to find a place for (1) the properties of narrowed mental fields, (2) the sources of respect for authority in matters remote from immediate experience, and (3) the dependence of distortions in feeling and thinking on the need to preserve cherished personal and group bonds. Since our understanding of these matters is limited, qualitative observation has a place of importance.

In this region, too, a certain breadth of view is not out of place. Thinking and investigation have concentrated almost obsessively on conformity in its most sterile forms. Observation of human affairs, as well as psychological considerations, can correct this one-sidedness. The individual participates in social life by means of his capacities to think and feel, by including within his view the situation of the group. Individuals stand in a relation to their group milieu wholly different from that of a cell to an organ, or an organ to the body. They will never be free of group constraints, but they are potentially capable of questioning the most established beliefs. To be sure, if one takes a sufficiently narrow—or overextended—view, the majority of mankind appears throughout history as an inert mass swinging heavily with the social tides. The notions of imitation and conformity then seem to fit most aptly. But it is the contribution of psychological thinking to take up a position that is neither too near nor too far from its subject matter. We may agree that the greater part of mankind takes most of its ideas and beliefs at second hand, that the reasons for most of the things men do is that others have done them. But the psychologist will not miss seeing that in some corner

of our lives we are at times capable of taking a fresh view, and that the aspiration to become oneself is also part of the human attitude.

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INDIVIDUAL SYSTEMS OF ORIENTATION

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INTRODUCTION

This paper* attempts to explore, both theoretically and empirically, some of the consequences of a single postulate. In its most general form,

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the postulate is to the effect that there are lawful interdependencies among certain classes of beliefs and attitudes held by the same individual. Proceeding from distinctions between two classes of objects of attitudes (persons as communicators, and objects of communication), and between "own" attitudes and those attributed to other persons, it hypothesizes that certain kinds of combinations of such attitudes and beliefs are psychologically unstable, tending to induce psychological events that result in more stable combinations. A more precise wording of the proposition must await further clarification of terms.

I know of no better way to indicate the kinds of problems that I have found illuminated by the approach here outlined than to trace its natural history, autobiographically.

In one of my earliest investigations [35] I found, not very surprisingly, a very considerable degree of within-family homogeneity in attitudes toward political and religious issues. What I had not anticipated—after all, my training had been in psychology, not in sociology—was the finding that the variations in degree of within-family similarity could only be accounted for in terms of differential impacts upon different families of common influences, which seemed to be of institutional nature. It seemed likely that individual differences within families whose members were subjected to common institutional impacts represented some sort of compromise adaptation to family norms, on the one hand, and to personal attitudes toward family members, on the other.

In a later and more ambitious study [31] I found some confirmation for these suspicions. In tracing the development of attitudes toward public issues of the entire student population of a small college, the influence of group norms was very clearly revealed, but as so often happens, it was by careful examination of variations on the common theme, and of outright exceptions to it, that the most significant findings emerged. As a general principle, attitude change in the direction of the prevailing norms was most pronounced on the part of those who participated most actively in college affairs. But change and persistence in attitudes toward public issues could be accounted for in all cases only in the light of attitudes toward persons and groups. Somehow the two kinds of attitudes were associated. Stimulated in particular by discussions with Professor M. Sherif, I published an account [33] of these findings, subsequent to the original monograph, in terms of positive and negative reference groups.

No findings had been more illuminating, in this investigation, than those derived from subjects' estimates of the attitudes of various persons and groups toward the same issues concerning which their own attitudes had been frequently expressed. The degree and direction of distortion that appeared in these estimates appeared to be a particularly sensitive

indicator of the psychological processes by which approach-avoidance tendencies toward persons or groups influenced acceptance or rejection of the positions attributed to those persons or groups. Conversely, changes in attitudes toward persons and groups often accompanied changes in attitudes toward public issues, under conditions suggesting that the former was the dependent variable.

From such considerations it was not much of a theoretical leap to speculate along the following lines. Human beings are constantly dependent upon each other, not only in direct ways and for consummatory purposes (like helping and loving) but also indirectly, as sources of information about other objects in the world. One's own eyes and ears are never enough; one's own experience is almost always too limited, and one's own observations often in error. Socialized¹ human individuals are as rewarding to each other in the latter sense—i.e., as communicators, as suppliers of new information, and as confirmers or correctors of old—as in the former. Favorable attitudes toward persons, as rewarders, are generated in both ways.

In so far as one person has been rewarded by another as communicator—i.e., rewarded in the sense of having found him a trustworthy informant—he is likely to experience conflict on discovering that his own attitudes toward some object are divergent from those of the trusted informant. In somewhat more general terms, it may be postulated that perceived discrepancy between own attitude and that of a trusted person or group is disturbing; it serves to upset a previously established equilibrium. The discoverer of such a discrepancy is likely to conclude that either his own attitude or that of the previously trustworthy informant is "wrong." Such a disturbance might be expected, as in the case of other equilibrium disturbances, to engender some form of equilibrium-restoring behavior—for example, changing one's own attitude toward the object, obtaining further information from other sources, or modifying one's trust in the informant.

My thinking along these lines was considerably aided by Professor F. Heider's published work [13, 14] on "balance," and later by that of my colleagues Dorwin Cartwright and Frank Harary [6], along similar lines. Meanwhile the experimental and theoretical contributions of Professor L. Festinger and his students [see especially 10, 11] helped me to put into perspective the interrelated notions of perceived discrepancy in attitude and communicative behavior.

As a result of these and other influences I have ventured into some-

¹ Here, and throughout this paper, I shall use the term "socialized" to refer to humans old enough and otherwise able to communicate "normally" and to have "internalized" the norms of groups of which they are members sufficiently not to be considered gross deviants.

what wider explorations concerning social interaction, viewed from the point of view of both groups and individuals. I have come to see social interaction in communicative terms, in the sense that it involves almost exclusively the exchange of information rather than of energy. I view individual participation in social interaction as a virtually lifelong process which includes the following subprocesses:

1. Cognitive and cathectic predispositions (attitudes, or orientations, as I shall later call them) are acquired, interdependently, toward persons and toward objects of communication with those persons.

2. Simultaneously, beliefs are acquired concerning the attitudes of fellow-communicators toward objects of communication with them.

3. With regard to specific persons (or groups) and specific objects (or classes of objects) of communication, attitudes and beliefs about others' attitudes come to function interdependently as a system having equilibrium properties.

4. Communicative exchange is initiated by individuals under conditions of system disequilibrium.

5. System modifications tending toward restored equilibrium follow such communicative exchanges.

6. The new state of equilibrium tends to persist until it is disturbed by the receipt of new information (by direct, sensory experience with the object, by communication with others, or by processes of memory, reasoning, fantasy, etc.), following which there is renewed instigation to communicative exchange.

In an immediate sense, this paper attempts little more than a systematic formulation of the processes by which human beings develop attitudes toward other persons and toward objects of joint relevance to themselves and to those persons. I believe that the formulation takes into account a wider range of phenomena, and brings them more parsimoniously within a single framework, than have my own previous attempts in this direction. In a more inclusive sense, I have some hope that the approach here presented has improved my own understanding of the peculiarly human aspects of social interaction in all its forms. Perhaps a similar formulation, in collective rather than in individual terms but resting upon the same general notions, can even be applied to the understanding of group phenomena at their own level—but that is a different story, more appropriately told elsewhere. For the present, I need only note that “systems of orientation,” to a fuller description of which I now turn, may be regarded as intraindividual representors of the objective interactional systems in which individuals are psychologically involved at any given moment.

At the outset I stated my intention of exploring the consequences of a single postulate, a formal statement of which depends upon terms yet

to be presented. That postulate is to the effect that forces toward states of equilibrium within individual systems of orientation determine both existing attitudes toward two classes of objects and the behaviors by which further information concerning those objects is obtained and evaluated—and by which, therefore, those attitudes are maintained or changed. The consequence of taking this proposition seriously is the promising possibility of developing an inclusive theory which, at one level, accounts for the development of multiple attitudes on the part of single individuals and, at another level, accounts for the communicative behavior of which social interaction among humans so largely consists.

CONCEPTS ESSENTIAL TO THE FORMULATION

All the concepts described below refer, in a sense, to independent, intervening, or dependent variables, since each of them refers to something conceived as varying in degree, and since each of them, hypothetically, either contributes to the variance of one or more of the others or results from such variance. For two reasons, however, I have preferred to present this formulation in terms of systematic rather than empirical variables. First, the heart of the formulation lies in hypothetical relationships of the several variables to the construct "strain," which is in no sense an empirical variable. Since, as I assume, an empirical variable is meaningless apart from a proposition in which it is paired with at least one other empirical variable, either as dependent or as independent, it would be inappropriate to present this formulation as a set of hypothetical relationships among empirical variables. Second, I have chosen to emphasize "system properties" rather than the single variables which contribute to them, and consequently none of the variables has an enduring status either as independent or as dependent. According to some of the specific propositions to be presented, a change in one system variable is likely (under certain conditions) to be followed by a specified change in another system variable, but according to others a change in the second is a precondition for a change in the first. The variables corresponding to the following concepts are therefore presented as formal ones, which may or may not be subject to operational definition, but whose hypothetical effects are empirically testable.

The fact that I have chosen to emphasize the systematic nature of these construct variables will not, I trust, lead the reader to conclude that the present formulation has not led to testable hypotheses. The facts are quite the reverse, as I shall attempt to show, after a fuller description of the central concepts, and of the relationships among the systemic variables to which they correspond.

Orientation. It seems to me necessary to assume that human individuals, in interacting with one another, develop cognitive and

cathectic habits of relating themselves both to each other and to the world of objects with which they deal in common. So dependent are human beings upon one another, and so recurrent are the requirements for adaptation to the objects which they face in common that, given human capacity for learning, the development of habitual and anticipatory adaptations to one's fellows and to common objects is inevitable. Since it is the cognitive and the cathectic aspects of these habitual adaptations that are of interest in the present formulation, the term "attitude," which commonly has precisely such meanings, would serve to describe them, except for one consideration. It is crucial for the present formulation to distinguish between two kinds of objects of attitudes: persons as fellow communicators, and objects of communication (including persons). I have therefore preferred to use "orientation" inclusively, as referring to both kinds of objects of attitudes, reserving for "attitude" the latter meaning only and labeling the former as "attraction."

Conceptually, an orientation may be defined (in its most general sense) as that existing organization of the psychological processes of an organism which affects its subsequent behavior with regard to a discriminable object or class of objects.² By long usage, however—doubtless

²This resembles fairly closely the "standard" definitions of "attitude." Cf. Kretch and Crutchfield: "an enduring organization of motivational, emotional, perceptual, and cognitive processes with respect to some aspect of the individual's world"; or Newcomb: "predisposition to perform, perceive, think and feel in relation to" an object; or G. W. Allport: "a mental and neural state of readiness, organized through experience, exerting a directive or dynamic influence upon the individual's response to all objects and situations with which it is related." The inclusion in the definition of the notion of persistence over time is in one sense very awkward, since if the behavior from which an attitude is inferred changes over time, it becomes necessary to assume that the attitude defined as "enduring" has not literally endured, but has changed. Nevertheless, there would be no need of the concept of attitude if the time factor were not taken into account, because without it the concept would refer only to momentary determinants of behavior.

The source of the dilemma lies in the temptation to assume that object-oriented behavior is determined exclusively by attitudes, overlooking the fact that there are also immediate situational determinants. Any given instance of object-oriented behavior is a resultant of attitudinal (presituational, residual from previous experience) and of immediate (situational) determinants. Instead of "enduring," therefore, I have used the term "existing," intending to suggest (*a*) that attitudes consist of the presituational determinants of any given instance of behavior with regard to a specified object, or class of objects; and (*b*) that if behavior in a given instance is not as would have been predicted from knowledge of presituational attitude, it is not necessary to assume that the "persistent" attitude has changed (as known only *post hoc*) but only that new, situation-induced influences have been introduced. These new influences, from the present point of view, may subsequently modify the attitude, but these determinants of the specific instance of behavior are not to be included among the attitudinal determinants of that behavior.

because of the ways in which the concept has in fact been operationalized—its meaning has been pretty much limited to “strength” and “directionality” or sign (i.e., approach-avoidance) as aspects of the “existing organization . . . which affects . . . subsequent behavior.” The narrower conceptual definition would thus become “that existing organization of the psychological processes of any organism which affects the direction and strength of its subsequent behavior with regard to a discriminable object or class of objects.”

Orientations are here categorized in two ways: according to the role of the object of orientation in the communicative process (attitudes and attractions), and according to psychological aspect (cognitions and cathexes). *Attitudes* are conceptually defined as orientations toward any object viewed as object of communication—that is, about which information may be transmitted and/or received.³ Attractions (which, like attitudes, may have either plus or minus sign), analogously, are conceptually defined as orientations toward cocommunicators—specifically, toward the source of a message, on the part of the receiver (actual or potential⁴), or toward the recipient, on the part of the transmitter.

This distinction, however simple it may appear conceptually, has as an empirical counterpart an important class of borderline orientations; namely, those in which the object of communication is either the source or the recipient of the message. There are many instances in which it is possible to distinguish empirically between persons as objects of communication and as sources or recipients of communication about something other than themselves. Thus, the transmitter of the message “Lincoln was a wonderful man” is not the object but only the author of the communication. But if he transmits the message, “I am hungry,” he is both the author and object of the communication. In cases of the latter kind it is empirically difficult to distinguish between the orientations of the recipient of the message to the transmitter qua transmitter and qua object of communication, i.e., between the receiver’s attitude and his attraction toward the transmitter. This difficulty is not merely one of operations, but seems to rest upon the solid empirical facts of psychological generalization; that is, persons frequently do not in fact keep

³ As implied by the phrase “and/or,” I shall not limit the term “communication” to those instances of message transmission in which *both* encoding and decoding occur. Though it is possible to treat messages which are not, in the literal sense, encoded (because they are unwittingly transmitted) as signs rather than as messages, I shall consider the occurrence of such events as communicative phenomena, provided they are receivable and decodable messages. This seems to be consistent with Miller’s definition: “‘Information’ is used to refer to the *occurrence* of one out of a set of alternative discriminative stimuli” [28; my italics].

⁴ Henceforth, in the interests of brevity, I shall not repeat the phrase “actual or potential,” which will be taken for granted.

distinct their orientations toward communicators and toward the same persons as objects of communication. Hence, although the conceptual distinction is clear enough, it is not always possible to make the empirical distinction.

Orientations have been categorized, secondly, as having both cathectic and cognitive aspects. The former refer to approach-avoidance tendencies; cathectic orientations have the conceptual properties of sign and strength. It is convenient (and traditional) to regard sign and strength as varying along a single continuum, from maximally positive to maximally negative approach tendencies.

Cognitive orientations (or, more exactly, the cognitive aspects of orientations) have to do with the ordering, or structuring, of attributes as cognized "in" the object of the orientation. For present purposes it is not assumed that orientations vary in respect to degree of "ordering of attributes," but only in respect to the relative salience of specified attributes. Such attributes, which may themselves be regarded as sub-objects of orientation—i.e., aspects of the "whole" object—may also have cathectic value, but if so it is the phenomenal ordering, or relative salience, of these attributes, and not any central tendency of their cathectic values, which is the important property of cognitive orientations, for my purposes. It is *discrepancies* among cognitive orientations—in particular on the part of different individuals toward the same object—and not an individual's cognitive orientation toward an object, that constitute a system variable. Cognitive orientations toward different objects, or toward the same object by different persons, are comparable only in so far as the same attributes are ordered. They are not comparable if only the attributes peculiar to each object of orientation, or if only those recognized by a given person, are taken into account.

Operationally, cathectic orientations (whether attitudes or attractions) are ordinarily defined in terms of verbal responses from which sign and strength are inferred; they may also, of course, be inferred from non-verbal behavior. Any of the "standard" procedures of sociometric or attitude measurement may be employed. Cognitive orientations may be operationalized from verbal responses like checking the presence or absence of attributes of objects, or rank-ordering attributes on a continuum of salience. I have found Gough's adjective check lists [12] useful for the study of cognitive aspects of persons.

Judged (or, less accurately, perceived) orientations of others refer to any of the above categories of orientations, as attributed to another person, or, for certain purposes, to a group. As will subsequently appear, it is the relationships between subjects' own orientations and those which they attribute to others, toward the same objects, that are crucial for the present formulation, rather than either kind alone. For the sake of com-

parability, therefore, it is important that judgments of others' orientations be obtained via the same instruments by which "own" orientations are indicated.

Systems of orientation. The fundamental postulate upon which the present formulation rests is that all of the foregoing kinds of orientations, and of judged orientations of others, on the part of the same individual (assuming that all of them may be said to exist) are dynamically interdependent. (The argument upon which this postulate rests is presented in the following section.) The totality of these orientations is therefore regarded as having system properties, in the sense that a change in any one of them, under certain hypothetical conditions, induces change in one or more of the others. Orientations and judged orientations of others are regarded as elements in such systems, rather than as variables of primary significance in their own right, although

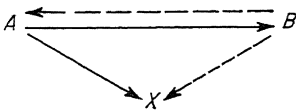


FIG. 1. Schematic representation of orientations included in the phenomenal system of person *A*, as he co-oriens toward person *B* and toward object of communication *X*. (Arrows point toward the person or object of orientation; broken lines refer to orientation of person *B* as perceived by person *A*, and solid lines to person *A*'s own orientation.)

important propositions may be derived in which orientations appear as independent or as dependent variables.

Figure 1 presents a schematic illustration of the system of orientations of individual *A* with respect to individual *B* and object *X*; the arrows refer to both cognitive and cathectic aspects of orientations. *A* is said to be co-orienting toward *B* and *X* when all the orientations represented by the arrows in the figure are phenomenally present in *A*.

I have previously [30] indicated my reasons for believing that co-orientation is the rule rather than the exception. On the part of socialized humans, that is (with the possible exception of utterly private objects), orientations toward objects are never unaccompanied by orientations toward other persons who are assumed also to have orientations toward them, and orientations toward persons are never unaccompanied by orientations toward objects toward which they are assumed also to have orientations.

There are other orientation variables which function primarily as parameters of system strain. *Importance* refers to the valence aspects of attitudes, or to degree of cathectic orientation toward an object, regardless of sign. It is most readily operationalized in terms of an intensity measure, or in terms of degree of positive or negative attitude. *Object relevance* refers to the degree of joint dependence of two or more com-

municators upon a specific object of communication, as judged by one of them. Or, alternatively, it is the strength of forces acting upon an individual to co-orient toward a specified object and toward a specified other person. It is operationalized from verbal responses concerning the importance of the object to the self-other relationship, or concerning the closeness or the relative frequency with which the respondent associates the object with the other person. Thus a child would be a highly relevant object to each of its devoted parents, provided that each of them assumes that the other shares his or her concern for the child. The business affairs of a man who considers that his business is "not the business" of his wife, and that she is indifferent to them, would not, from his point of view, be an object of joint relevance to both.

Certain distinguishable forms of attraction (liking, respect, trust) are described below, in context; fuller statements about them do not seem necessary meanwhile.

System strain. The nature and the conditions of interdependence among orientations and judged orientations of others depend upon the hypothetical intervention of an additional construct—that of "strain." In an earlier formulation [30] I referred to this construct as "strain toward symmetry"—a phrase whose suitability derived only from a certain graphic presentation of the elements of systems of orientation. The phrase is not an altogether happy one, but in any event it refers to a hypothetical state of psychological tension occurring under certain conditions of judged discrepancy between own and another's attitude toward the same object. As outlined in greater detail below, systems tend to move from states of greater to lesser strain, or toward "balance" [cf. 13, 6]. The mechanism by which this is accomplished is that of change in one or more of the system elements (orientations and judged orientations of others). This commonly but not necessarily occurs following communicative behavior, to which system strain is hypothetically an instigator.

Strain is regarded as corresponding to a state of tension [in its Lewinian sense; cf. 24, chap. 1] induced by the judged state of the cocommunicator's orientations in relation to one's own. The source of such tension may be (1) perceived discrepancy of self-other orientations and/or (2) uncertainty as to the other's orientations. The distinction is necessary because either may occur without the other, and their consequences may be very different. (In everyday terms, one may need to know another's orientations without in the least caring how they correspond to one's own—e.g., "How will you take your tea?"; or one may know with great certainty that another's orientations are different from one's own and be greatly concerned about the discrepancy.) The hypothetical conditions under which both discrepancy strain and un-

certainty strain are aroused are discussed below. Meanwhile, it need only be said that it is not assumed that either perceived discrepancy or uncertainty, *per se*, necessarily involves strain.⁵

Amount of strain, as a system variable, is postulated to vary with (1) degree of perceived discrepancy, (2) sign and degree of attraction, (3) importance of the object of communication, (4) certainty ("committedness") of own orientation, and (5) object relevance. The nature of these functional relationships is discussed below.

System-strain variables are categorized as accompanying either positive or negative attraction. This distinction is an important one, theoretically, since the manner in which attraction variables are presumed to interact with other variables to produce a given amount of strain varies with sign of attraction. It is convenient, therefore, to speak of positive and negative strain, as varying with the sign of attraction toward the cocommunicator. The further distinction between cognitive and cathectic strain corresponds to phenomenal discrepancies between the cognitive or cathectic aspects of orientations. Thus, for example, positive cathectic strain would be said to characterize any state of a system of orientations in which attraction to the cocommunicator is positive and in which there is perceived discrepancy between own and other's cathectic orientations to a relevant object of communication.

Communicative behavior. For present purposes, communicative behavior consists of the transmission and reception of information by human organisms. Information has been formally defined by Miller as "the occurrence of one out of a set of alternative discriminative stimuli" [28]. In the case of humans, at least, the latter are necessarily symbols (though not necessarily verbal ones). Hence, behaviorally speaking, communication, as I shall use the term, consists of sending and receiving symbols.

Communicated messages may be categorized (1) as consisting of either transmitted or received information, and (2) as to the content of the information. Since the central theoretical problem of the present systematic formulation has to do with the interdependence of communicative behavior and the orientations of the communicators, the primary content variables are the inferred orientations of the transmitter toward the referent of the symbols used in his communication.⁶ These are subcategorized, again, as either cathectic or cognitive. Inferences about the

⁵Unless "uncertainty" is specifically mentioned, "strain" will henceforth refer to "perceived discrepancy" and not to "uncertainty."

⁶For theoretical purposes, it is, of course, fruitless to categorize content in terms of objects of communication (symbol referents), since such categorizations would be phenotypic and not subject to theoretical generalization. Categorization in terms of inferred orientations of the transmitter has the common-sense justification that behavioral consequences for the receiver vary more directly with his inferences

transmitter's orientations may of course be made either by the "objective" observer or by the recipient of the message; such inferences are assumed to influence the recipient's subsequent behavior in relation to the transmitter or to the object of orientation, or both.

There are two systematic variables closely related to the notion of communicative behavior. One of these is "amount of information received" by a given individual from another given individual about a given object of communication. For obvious reasons it would be virtually impossible to apply the conventional measure (the logarithm of the number of alternatives) in the more or less "natural" situations in which investigations suggested by the present formulation could be carried out. My own practice, therefore, has been to use very crude indexes, like amount of time spent in discussing a given topic with a given individual. The other systematic variable, "instigation to communicate," refers to a hypothetical degree of motivation to transmit a message to, or to initiate a communicative exchange with, a given individual about a given object. Such a construct is necessary, as noted below, because there are many possible counterforces which result in inhibition of communications that would otherwise, presumably, be transmitted.

MAJOR INTERRELATIONS AMONG CONSTRUCTS

The general outlines of the relationships among orientations, system strain, and communicative behavior have already emerged. These relationships are conceptualized as properties of an intrapersonal system of orientations, and of perceived orientations of another person, on the part of a co-orienting person (i.e., one who is attending both to a cocommunicator and to an object of communication).

More explicit statements are now in order; at the broadest level of generality they will be formulated as inclusive postulates from which more specific, testable hypotheses may be derived.

1. Certain combinations of a person's orientations toward a specified object of communication and toward a cocommunicator, together with the latter's perceived orientations toward the same object of communication (all of which orientations are viewed as constituting a system), are

about the transmitter's orientations than with the "pure" content of the communication. It has the theoretical advantage, moreover, of making minimal assumptions (e.g., the message "I like apples" does not necessarily have the consequence for the receiver of the message that he believes that the transmitter likes apples, nor if he does, that thereafter he considers apples good), and thus leaving open the question of other parameters involved in the behavioral consequences for the receiver. Such a categorization has the disadvantage, of course, of necessitating very difficult operationalizations.

strain inducing. For example, if a student discovers that some of his beliefs about genes are at odds with those of his respected professor of genetics, or if he believes that a close friend differs with him about the virtues of a favorite political candidate, both systems of orientation would be characterized by discrepancy strain.

2. Instigation to communicative behavior is a learned response (not necessarily the only one), on the part of socialized humans, to such strain. The likelihood that such instigation will actually eventuate in communicative behavior (e.g., asking questions, trying to persuade the other to one's own point of view) varies with the strength of instigation, situational opportunity for communication, and the strength of opposing influences. Thus the student of genetics is most likely to seek further information from his professor if he is much disturbed by the discrepancy, if the professor is readily accessible, and if the student is not afraid, embarrassed, or otherwise reluctant to launch the communicative exchange to which he is instigated.

3. Following communicative behavior (transmitting and/or receiving information) on the part of one person vis-à-vis another, changes may occur within his system of orientations such that strain is reduced. The probability with which this occurs varies both with internal system variables (e.g., attraction toward the other, or degree of perceived discrepancy) and with external parameters (e.g., "competing" attraction toward other persons or groups). It is much more apt to occur, of course, following the receipt of information (especially concerning another's orientations) than following its transmission without feedback from the other person. If the same student has involved his friend in a discussion about the political candidate in question, his discrepancy strain might be reduced in any of the following ways: by being influenced to change his own attitudes toward the candidate; by becoming convinced that his friend's opinions had changed in the direction of his own; by concluding that his previous assumptions about his friend's opinions had been in error; by concluding that the matter was really of very little importance; or by concluding that his friend was so incompetent with respect to politics that his opinions did not matter. The first of these would be least likely to occur if the student's committedness to his own point of view was very strong; the last would be least likely to occur if his attraction toward his friend was very strong, and if there were many objects of importance to both of them which did not involve discrepancy strain.

4. The interdependence relationships within systems of orientation are such that, under conditions of system strain, changes in one or more of the component orientations or judged orientations may result in strain reduction apart from any overt communicative behavior. Suppose that the student prefers not to engage in what he fears may turn out to be an

unpleasant argument with his friend about politics. System strain is likely to be reduced intrapersonally, or autistically, in such ways as to have the same consequences as those noted above as following communication. That is, he may persuade himself that his own attitudes are too extreme; or that he must be in error about his friend; or that the matter is trivial, and should be shrugged off; or that his friend, though a fine fellow, must be considered a bit of an innocent in the area of politics. It is postulated, however, that these are substitutes for overt communication in the presence of counterforces to communicate, and not substitutes for the initial instigation to communicate.

5. The interdependence relationships within systems of orientation are such that alternative intrasystem changes may have equivalent effects upon strain. Eventually, that is, systems of orientation tend to revert toward equilibrium (i.e., minimal strain), whether the initial instigation

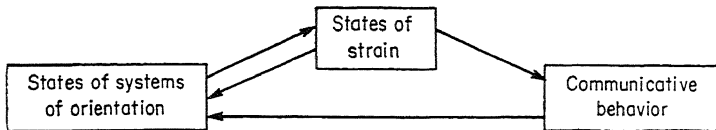


FIG. 2. Schematic illustration of relationships among essential concepts. Under certain conditions at the source of each arrow, certain changes hypothetically occur in the phenomena toward which the arrows point.

to communicate is expressed or inhibited, and whether one or another or some combination of the various alternatives is employed.

The first four of these statements of relationships among existing states of systems of orientation, communicative behavior, and changes in states of systems are schematically illustrated in Fig. 2. Each of the arrows indicates that "under certain conditions so-and-so occurs or is likely to occur."

There follow somewhat expanded explications of these five postulates. At the risk of repetitiveness, I have preferred to present all of them briefly before making the fuller statements, in order that each of the latter may be understood in the light of preliminary familiarity with all of them.

Certain states of orientation systems are strain inducing. The most unambiguous instance is the combination of positive attraction with perceived discrepancy of attitude toward a relevant object. Such a system state involves conflict or threat, and is, therefore, tension inducing, in one or more of the following ways:

1. In so far as positive attraction involves respect for the other's knowledgeability or expertness regarding the object of communication, an orientation which diverges from the other's may be in error. Hence

the greater the respect, and the greater the importance of not being in error, the greater the strain (degree of discrepancy being held constant).

2. In so far as positive attraction involves trust in the other's willingness to be helpful with respect to the object of communication, a divergent orientation may threaten motive satisfaction with regard to that object, since cognitive discrepancy might interfere with the communication through which help is to be given and received, and since cathectic discrepancy might lead to unwanted kinds of "help." Hence, given positive respect (i.e., perception of the other's ability to be helpful, without which trust becomes irrelevant), the greater the trust and the greater the object-relevance, the greater the strain of discrepancy (i.e., the greater the assurance that the other is both able and willing to help, and the greater the need of that help, the more threatening perceived discrepancy becomes).

3. In so far as positive attraction involves generalized liking for the other person (together with motivation to associate with him), a divergent orientation might threaten the personal relationship, either via overt conflict or (if the other's tolerance for discrepancy is thought to be low) via the threat of rejection by him. Hence the greater the liking, the stronger the person's own committedness, and the stronger the other's perceived committedness, the greater the strain, other things equal.

4. And finally, under all conditions of positive attraction, regardless of its particular components, sheer uncertainty as to whether or not discrepancy of orientation exists may be threatening; behind uncertainty there may be the possibility of any of the kinds of threats mentioned above. Beyond a certain point, presumably, known discrepancy may be more tolerable than uncertainty; and below the point where other strain-inducing conditions exist, uncertainty would not be threatening. Within these limits, the greater the uncertainty as to divergence of orientations the greater the strain, *ceteris paribus*.

In general, the conditions of strain induction are the same for negative as for positive attraction. Such differences as there are between positive and negative strain are analogous to the differences between any instance of simple approach behavior and the same behavior as avoidance of a less attractive alternative.

An exact analysis of the conditions of negative strain presupposes an analysis of the conditions under which co-orientation toward a negatively attractive person (together with an object of communication) occurs. In particular, it is necessary to distinguish between those situations in which co-orientation occurs *because of* negative attraction toward the cocommunicator and those in which it occurs *in spite of* negative attraction. In the former case, it is perceived threat potential of the cocommunicator which induces co-orientation toward him and toward objects

related to his feared behaviors. Under these conditions, as with positive attraction, the greater the negative attraction, the greater the strain (i.e., the more dangerous the other person, the greater the importance of being able to see things as he sees them). Co-orientation may occur in spite of negative attraction either because of perceived object-relevance (e.g., toward a business client who is held in contempt and toward the transaction that must be completed with him), or because of conditions that, for our purposes, are extrasystematic (as in conversation about the weather with a bore who cannot be avoided). Under these conditions, as distinct from positive attraction, there is no reason to hypothesize that strain increases with degree of negative attraction, but as with positive attraction, strain is hypothesized to increase with object-relevance.

Certain differential predictions which are made for the several "varieties" (which really refer to "sources") of attraction correspond fairly closely to those just made for the two signs of attraction. *Respect* is conceptualized as "adience toward the cocommunicator stemming from his perceived power over the object of communication," including knowledgeability, skill, expertness, and ability to make decisions about it; respect is object-specific. No theoretical purpose appears to be served by conceptualizing respect as negative—i.e., less than no skill, expertness, etc. Its contributions to strain are, therefore, hypothetically the same as for other varieties of positive attraction. *Trust* is specific to the co-orienting person (i.e., to the self), just as respect is specific to the co-oriented object; it is conceptualized as "an individual's adience stemming from the cocommunicator's perceived favorableness toward that individual," including sincerity and helpfulness on the positive side and deceitfulness and hostility, on the negative. As suggested by the just preceding discussion of negative attraction induced by threat potential, degree of strain varies directly with strength of trust, regardless of sign.

Liking is conceptualized as "general, undifferentiated adience (or abience, in the negative case) toward the cocommunicator"; its sources are not specified. It may be presumed to be a generalized resultant of the system properties of respect and trust, and of properties attributed to the cocommunicator which, for present purposes, may be regarded as extrasystematic. As in the case of trust, strain is presumed to vary directly with degree of liking, regardless of sign.

Strain, so far, has referred to system states characterized by perceived discrepancy of orientations, as distinct from sheer uncertainty as to the other's orientations. The latter (uncertainty strain) without the former may hypothetically occur under the following conditions: (1) system states in which attraction is predominantly negative (dislike and/or mistrust); and (2) system states characterized by absence of

object-relevance (especially those in which cathectic orientations are regarded as matters of "taste"). The latter may be regarded, for present purposes, as trivial. As to the former, the conditions of uncertainty strain are presumably the same as those of discrepancy strain, as outlined in the preceding discussion of negative strain.

Instigation to communication is a learned response to strain. This proposition merely asserts that among socialized human beings a process of operant conditioning has taken place whereby (under the stated conditions) a threatening state of affairs (i.e., phenomenal strain, either of discrepancy or of uncertainty) leads to instrumental behavior directed toward the removal of that state of affairs. It is implicit in this assertion that communication is an instrumental behavior which socialized human beings have found rewarding because of its efficacy in removing or reducing the threat, i.e., by establishing or increasing perceived similarity and/or certainty. This proposition does not assert that instigation to communication is the only learned response to phenomenal strain, but only that in the life history of socialized humans it has been rewarded with sufficient regularity to have been dependably learned. Neither, of course, does the proposition say anything about the possibility that among other learned responses to strain may be instigations to behaviors which inhibit or prevent communicative behavior.

Instigations to initiate communication may occur under any of the following states of system strain (in all of which one or more varieties of positive attraction are assumed to exist, momentarily at least):

1. The cocommunicator is perceived to "possess" an item of information which the person himself lacks and wants (e.g., "What time is it?").
2. The cocommunicator is perceived as lacking an item of information which the person himself "possesses" and wants the other to have (e.g., the information that he needs to borrow a dollar).
3. The person wants to confirm a tentative observation (e.g., "Did you hear that noise?").

All of these are instances of cognitive discrepancy, as distinguished from the following instances of cathectic discrepancy:

4. The cocommunicator is perceived as devaluing an object which the person himself values, or vice versa. Under these conditions communicative behavior (if the instigation is not inhibited) may take the form of attempting to persuade the other to one's own point of view (which, if successful, would be strain reducing). It may take the form of "exposing" oneself to persuasion by the other (whose success would be strain reducing). If there is a series of communicative exchanges, both of these kinds of communication may occur, often with some degree of

resulting compromise or rapprochement—which, again, would be strain reducing.

5. Finally, mere uncertainty as to cathectic discrepancy may serve as an instigator to inquiry or assertion, either of which may be instrumental to the reduction of uncertainty.

Under certain conditions strain reduction follows communication. This occurs most dependably, perhaps, in respect to cognitive discrepancy—i.e., following the exchanging of unevaluated information. The principal limiting conditions are those varieties of positive attraction which have been labeled “respect” and “trust.” In the case of cognitive strain reduction as a consequence of receiving information, it is attraction toward the other which is the limiting condition; in the case of transmitting information, strain reduction is most likely when the other is perceived as respecting and trusting oneself.

The conditions of cathectic strain reduction following communication are more complex. As everyone knows, a communicative exchange can easily lead to increased discrepancy, and often to increased strain. The limiting conditions here are of the following classes:

1. *Conditions of cognitive strain reduction.* Since cathexis presupposes cognition (objects are cathected not “as they are,” but as they are cognized), the conditions of respect and trust, as mentioned above in respect to cognitive discrepancy, are also operative in respect to cathectic discrepancy.

2. *Extrasystem conditions*—i.e., conditions external to the system of orientations under immediate scrutiny. Of particular importance is “committedness” to existing attitude, which may be an aspect of a more or less generalized personality characteristic, or which may be anchored in other systems of orientations, e.g., in “reference groups” of strong positive attraction. The stronger the existing attitude committedness, the less likely that strain reduction will occur via attitude change following communications. The implications of this are not necessarily that the amount of strain tolerated varies with committedness, but simply that other modes of strain reduction are more likely to be resorted to under conditions of strong committedness.

In general, whether with regard to cognitive or to cathectic discrepancy, the conditions under which communication is most likely to be followed by strain reduction are the same as those under which communication is most likely to follow phenomenal strain, as noted in the preceding section. This generalization follows from the assumption that, in so far as general conditions exist under which communication leads to strain reduction, under those conditions communicative behaviors are learned as instrumental to strain reduction.

Under certain conditions strain-reducing system change occurs in the absence of overt communicative behavior. There are alternative modes of reducing system strain. This and the preceding proposition, together, simply assert that phenomenal systems tend to shift from strain toward "balance." Thus the questions arise: "What constitutes balance?" "How is balance achieved by noncommunicative means?" "Under what conditions does this occur?"

1. Balance (which is most easily defined as the absence of strain) is most clearly present under conditions of exclusively positive attraction and perceived similarity of orientations. The most extreme form of strain would be found in a system including intense positive attraction and maximum perceived discrepancy of attitude toward an object of high relevance (e.g., the phenomenal system of either of two shipwrecked men on a raft would be one of maximum strain if his strong attraction to the other stems from perceived dependence upon him for safety, and if he disagrees completely with him as to the use of their rudder, the proper use of which he regards as essential to remaining afloat). Our problems, however, have to do with changes of degree of strain, rather than with the extreme points on the continuum, and so we turn to the question of how a given degree of strain may be reduced.

2. It has already been proposed that, *ceteris paribus*, strain increases and decreases with each of at least five orientation variables. There are two general classes of ways in which these variables, in turn, change: either by the receipt of information (whether via direct sensory experience or indirectly, via communication), or by autistic operations upon information previously received. The latter include rationalizations, memory losses, elaborations in fantasy, and other forms of cognitive "distortion." It is by such processes that strain may be reduced in the absence of communication.

The assumption that phenomenal systems tend to shift from greater toward lesser strain applies to both classes of changes in orientation, but nevertheless there is an important difference between the two. Changes in orientation resulting from the acquiring of new information are often strain increasing—i.e., upsetting to existing states of relative balance—but autistic changes in orientation are far more rarely so. Response to the receipt of information by "realistic" increase in strain is adaptive, in the primary sense of favoring a viable organism-to-environment relationship, since threatening events do occur in the environment. Autistic increases in strain may also be "realistically" adaptive, of course (e.g., subsequent recognition of previously unrecognized threat), but the principal adaptive function of autistic change seems to be the intraorganismic one of reducing strain. If so, it is presumably a con-

sequence of the fact that, whereas the environment is not constantly imposing immediate demands which require strain increase, the ever immediate demands of tension-relaxation are always operating.

3. Autistic strain reduction may be either supplementary to or substitutive for strain reduction following information exchange. The hypothetical conditions under which it is most likely to occur are, briefly, as follows. As a substitute for communication, the chief predisposing conditions are absence of opportunity for communication (as determined by extrasystem factors like physical inaccessibility); negative attraction (a system variable) such that opportunities for communication with the other person are avoided; and system states such that communication with the other person about the system-object is avoided, or limited. The last of these categories includes many possible combinations; for example, communication with a positively attractive person about an object of perceived discrepancy may be threatening to the attraction relationship; or the balance of another of the person's systems of orientations (including the same object but a different other person) may be threatened by "exposing" himself to influence by the cocommunicator (the phenomenon of conflicting reference groups), so that communication with him on this topic is avoided or restricted.

As supplementary to communication, autistic strain reduction is most likely to occur when the immediate effects of communication are most strain inducing. Many instances of "the psychopathology of everyday life" are illustrative—e.g., the "motivated misunderstanding" of what another has said, or the assumption that the other has perfectly understood the message which one intended to transmit but which one has, in fact, transmitted with some error or ambiguity.

Alternative intrasystem changes may have equivalent effects upon strain. This follows from the propositions according to which strain varies as a function of several system variables. Even though redundant, in this sense, such hypothetical substitutability merits a final note, since it provides the basic rationale for the use of strain as a hypothetical construct ("the little black box").

Hypothetically, strain may be reduced under any of the following conditions: (1) by reduction in the strength of attraction, (2) by reduction of object-relevance, (3) by reduction of perceived ("other's") object-relevance, (4) by reduction of importance of the object of communication, (5) by reduction of perceived ("others's") importance of the object of communication, (6) by changes in cathexis or in cognitive structuring of own attitudes, such that there is increased similarity with the other's perceived attitudes, (7) by changes in perceived attitudes (cathectic or cognitive) of the other, such that there is increased

similarity with own attitudes.⁷ Any of these changes, hypothetically, may occur with or without communication. And, in any given instance, once any one of these changes has occurred with strain-reducing effects, the probabilities that any of the others will occur are reduced.

The functional relationships among systematic variables that are of most interest are those that contribute to strain. As already noted, strain hypothetically increases with increase in any of the following: (1) object-relevance, (2) object importance, (3) strength of attraction toward the cocommunicator, (4) perceived discrepancy between own attitude and that attributed to him, and (5) committedness (usually stemming in part from extrasystem influences) to own existing attitude. These functional relationships are straightforward enough, but what about interrelationships among these five contributors to strain? And what about their effects upon strain in those frequent instances, empirically speaking, when a change in one of the five induces a change in one or more of the others?

Let us assume that, at a given moment and with respect to a given subject, a given cocommunicator, and a given object of communication, there have recently been no equilibrium-disturbing events and that the system of orientations is relatively free from strain. Increased strain results from some psychological events (receipt of new information, either by communication, or by direct sensory experience, or by autistic processes) which increases one of these five (and perhaps other) system variables. Suppose that, as a result of such an event, the individual's positive attraction toward the cocommunicator has increased; hypothetically then, other things equal, strain would be increased unless perceived similarity of attitude toward relevant objects is increased.⁸ Or, alternatively, suppose that a recent event has resulted in increased object-relevance; the predicted consequence would be the same—increase in perceived similarity of attitude. In either case, the degree of such change resulting from increase in one variable is limited by the existing degree of the other. That is, even a large increase in attraction will not very dependably increase perceived similarity if object-relevance is low, nor will a large increase in object-relevance, if attraction is weak. These interrelationships are schematically illustrated in Fig. 3. As suggested by the figure, these functional relationships are assumed to be monotonic and asymptotic. These assumptions are consistent with available empirical evidence, but it would be premature, on the basis of such evi-

⁷ With specific reference to strain of uncertainty, an eighth condition should be added: by the receipt of further appropriate information. It is not clear, however, that this condition is substitutable for those noted above.

⁸ Or, more accurately perhaps, thresholds for perceiving similarity will be lowered.

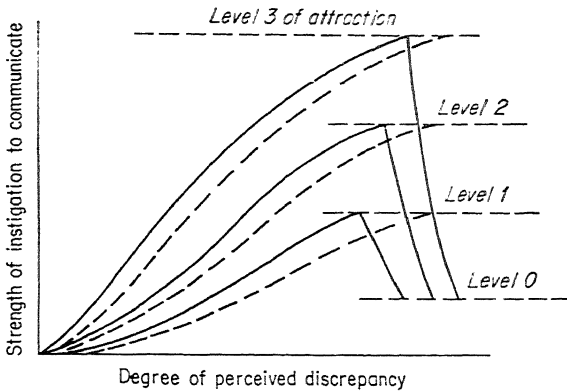


FIG. 3. Schematic illustration of hypothesized relationships among positive attraction, perceived discrepancy, object-relevance, and strength of instigation to communicate. Solid lines represent greater degrees, and broken lines lesser degrees, of object-relevance.

dence, to attempt to write equations for the assumed functions. (Object-relevance, in particular, has been only very inadequately measured.)

As another illustration of function forms, perceived similarity of orientation, viewed as an independent variable, is hypothetically related, via strain, to communication (or, more strictly, to instigation to communicate): specifically, strength of instigation increases monotonically with strain. The functional relationship may be described in a manner analogous to that suggested in Fig. 3, as shown in Fig. 4 (in which only positive attraction is considered, and in which the variable of "importance" is omitted).

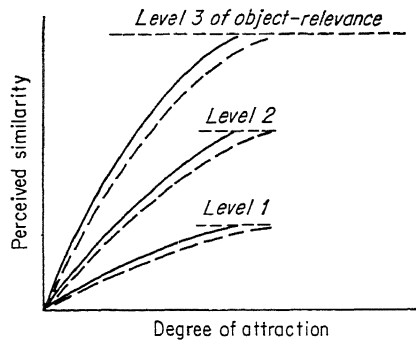


FIG. 4. Schematic illustration of hypothesized relationships among perceived similarity, object-relevance, attraction, and importance. Solid lines represent greater degrees, broken lines lesser degrees of importance.

Again (with one exception) the relationships are assumed to be monotonic. There is evidence to suggest that strain increases step-function-wise with perceived discrepancy—specifically, that less than a certain minimum of perceived discrepancy induces no strain, and more than a certain maximum leads to a reversal of attraction, and thereby to reduced strain.

As suggested by Fig. 4, the same degree of strain may be associated with many different combinations of attraction, perceived discrepancy,

and object-relevance. It is for this reason, of course, that the construct "strain" seems required; i.e., it is more parsimonious to relate the countless possible combinations of orientation variables, as independent, to the communication variables via the single construct of strain than to do so for each of the possible combinations, separately. The use of the strain construct, moreover, has the theoretical gain that significant propositions can be generated much better with than without it.

These samples of the assumed functional relationship are representative of the others. All of them, whether monotonic or characterized by step-function reversals, have a basis both in empirical evidence and in a theoretical rationale. All of them are subject to correction and/or refinement with the gathering of further data.

ALTERNATIVE FORMULATIONS

There appear to be only three comparable formulations which have been described in the literature, and the present one differs from each of them primarily in attempting greater comprehensiveness, in one way or another. Most nearly comparable in this respect is that of Homans [15], whose chief concern is to describe group properties in terms of hypothesized relationships among frequency of interaction, sentiment (which in actual usage, though not by formal definition, is equated with "liking"), activity, and (for certain purposes) group norms. Of these, only the first two (frequency of interaction and liking) are treated as empirical variables. At the group level, several of the propositions derived by Homans are closely equivalent to some of those which have appeared in these pages. But his makes no pretense to be a psychological system, since it is concerned only with the interdependence of group properties. There is no analysis of the intrapersonal processes by which, for example, frequency of interaction increases with personal liking, and vice versa. And, since attitude variables (as here defined) are not employed at all, there is no consideration of the interdependence between attitudes and attraction. Finally, perceived orientations of others, assumptions about which constitute one of the foundations of the present system, are not included at all in his system—not even, curiously enough, in his discussion of "norms," a norm being defined as "an idea in the mind of the members of a group . . . specifying what the members or other men should do . . . under given circumstances" [15, p. 123].

From these differences between the two formulations I conclude that, as might be expected in view of Homans's objectives, his system is less capable than is the present one of accounting for such empirical data as the following:

1. The observed covariation between attraction (including its forms other than "liking") and perceived similarity of attitude.

2. The fact that communication variables⁹—of content as well as of frequency—are a function, not straightforwardly of “liking,” but (a) of liking as interdependent with other variables, and (b) of negative as well as positive attraction, under some circumstances.

What I find most seriously lacking in Homans’s system is the absence of a set of “system parts” which are both psychologically meaningful and at the same time combinable into sociologically meaningful theoretical systems. In view of my own predilections for the kind of system-theory which makes concepts that are useful at one level of organization also available to adjacent levels of organization, I find necessary a system-theory which provides a psychological basis for the consensuses, both accurate and inaccurate, which are a required condition for group life. (Needless to say, there are many kinds of data at the group level which Homans’s formulation is far better equipped to handle than is the present one.)

Festinger’s systematic formulation [10] is strong precisely where Homans’s is weak. As indicated below, the variables of “pressure toward [group] uniformity,” perceived discrepancy, attraction to the group, expected success in influencing others toward agreement with oneself, and anchorage in other groups are hypothetically related, in systematic ways, to the “force to communicate.” The present formulation differs from Festinger’s (to which it owes much) primarily in that it includes communicative behavior of all kinds—receiving as well as sending messages, and nonpersuasive as well as persuasive messages—and in that it includes cognitive as well as cathectic orientations (“opinion” is the only term analogous to “orientation” that appears in his hypotheses). His system, therefore, makes no attempt to account for either the occurrence or the consequences of nonpersuasive communication; and, since his only equivalent for the notion of “strain” is “force to communicate,” his system does not account for “autistic” accommodations among orientations, other than the reduction of attraction with sufficient increase in perceived discrepancy. The two systems thus differ in this kind of comprehensiveness but, so far as I know, there is no instance in which opposite predictions would be derived from them.

Heider has developed a systematic formulation [13], much of which is still unpublished, which corresponds fairly closely to the “system of orientations” herein outlined, but which apparently does not attempt to account for either the occurrence or the consequences of communicative behavior. If Jordan’s [18] theoretical assumptions may be taken as representative of Heider’s, at least one instance has been reported [40]

⁹Homans concedes that the term “communication” is the virtual equivalent of “interaction,” as he uses the latter, provided there is no assumption that communication must be verbal.

in which, different predictions having been made from Heider's and from the present formulation, the latter would be supported.

EVIDENCE RELEVANT TO THE PRESENT FORMULATION

Nearly everyone who has concerned himself with the general problem of social influences upon attitude formation, persistence, and change has noted, in one way or another, that for some attitudes intragroup variance is less than intergroup variance [29]. This is particularly true for groups characterized by face-to-face interaction [31] and for those whose members (whether or not they interact in face-to-face manner) have some awareness of group membership [23]. It is also particularly true with regard to those attitude objects that are "group-relevant," i.e., are of common concern to group members, and, in some sense, of distinctive concern to the members of a given group [8]. Such findings, together with other related ones, suggest the very general conclusion that—in certain kinds of groups, at least, and with regard to relevant objects—attitudes are formed, persist, and change not just privately (i.e., "between" the individual and the object of his attitude) but also interpersonally (i.e., person-to-person influence has something to do with person-to-object attitudes). Attitudes toward group-relevant objects seem to be affected by some sort of intermember influence.

It has been almost as frequently observed that the same generalization applies to group members themselves, as objects of attitudes. Specifically, within-group variance on the part of a Polish-American society in Detroit, for example, toward members of various ethnic and religious groups is less than the variance of the same attitudes on the part of the total Detroit population. This is only to say, of course, that attitudes toward group members vary in accordance with the same principles as attitudes toward other kinds of objects. But data of this kind also yield another generalization: within-group attitudes toward "own" members tend to be more *favorable* than do between-group attitudes. This, of course, is common knowledge; such findings are often mentioned as illustrative of "ethnocentrism." Commonplace or not, the generalization, or some variant of it, takes an important place in the development of the present systematic formulation. Homans, in a systematic treatment which in some respects parallels this one, goes so far as to offer this hypothesis: "If the frequency of interaction between two or more persons increases, the degree of their liking for one another will increase" [15, p. 112]. His supporting data are drawn particularly from the Hawthorne studies [25, 38].

Another common-sense observation now becomes relevant. Given some freedom of choice, persons whose attitudes toward each other are

favorable tend to associate and to interact with each other. Homans takes note of this by adding "and vice versa" to the proposition just quoted. These two observations, together, suggest that *frequency of interaction and "liking" are reciprocally facilitative*. It is possible, however, that this circular effect is confounded by the homogeneity effects of within-group interaction. Indeed, it seems likely that a second circular effect parallels the first: *frequency of interaction and homogeneity of attitude are reciprocally facilitative*. This proposition, again, is supported by a good deal of everyday observation (e.g., "birds of a feather flock together"). Like the preceding one, however, it presupposes some degree of freedom of choice.

These considerations bring to mind another set of facts. In so far as persons associate and interact with one another selectively, on the basis of homogeneity of attitudes, the selection must be made on the basis of the judged or inferred attitudes of others, since attitudes, unlike feathers, are not unambiguously displayed. Illusory judgments as well as accurate ones may form the basis for selective association; notions like F. H. Allport's "impression of universality" [1] and Schanck's "pluralistic ignorance" [42] were developed to account for just this phenomenon. And so the proposition that homogeneity of attitude tends to facilitate interaction must be paralleled by another, to the effect that perceived homogeneity of attitude tends to facilitate interaction.¹⁰ Both propositions appear, on a common-sense basis, to be true; and (since illusions of this kind tend to be corrected by continued interaction) the former as well as the latter has predictive value. In any case, the facts seem to suggest a third kind of circular effect: *frequency of interaction and perceived homogeneity of attitude are reciprocally facilitative*.

These sets of common-sense observations may now be examined together. Interaction among group members seems to be reciprocally facilitative with favorable intermember attitudes, with "objective" homogeneity, and with perceived homogeneity of attitudes toward relevant objects. If so, interesting questions are raised about the possibility of interaction effects among the three kinds of attitude variables presumably associated with behavioral interaction. May it be that any one of them acts as a facilitating condition, or even a necessary condition, for one or both of the others?

Most of the "evidential grounds" so far cited have not been bodies of data gathered under controlled conditions, though such data are in fact available. The generalizations so far presented are actuarial-empirical;

¹⁰ The latter proposition can be operationalized either for individuals (persons tend to "choose" others whose attitudes are perceived as being like their own) or for collectivities ("voluntary" groups are characterized by relatively frequent judgments of intragroup homogeneity).

that is, under actually prevalent conditions they seem to be more often true than not. But the real problems begin with the investigation of qualifying conditions, for which purpose data more exactly gathered are required. Before turning to these, it may be helpful to present a schematic illustration of the conclusions and the questions to which the informal evidence has so far led. Figure 5 is autobiographically accurate; it represents a schematic summary to which I often turned at one period.

Samples of Relevant Evidence from Published Studies

The relevant evidence, from now on, has to do with the specific conditions under which the three kinds of circular effects predictably occur, and, in particular, evidence concerning relationships among the three

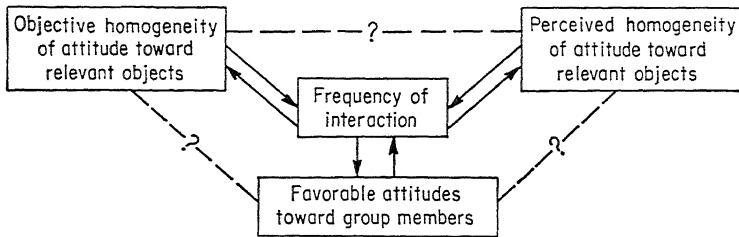


FIG. 5. Schematic representation of probable and possible relationships among four kinds of group variables, as inferred from informal evidence. The arrows signify functional relationships for which both empirical evidence and a theoretical rationale exist; the broken lines, with question marks, signify hypothesized relationships.

attitudinal variables. From this point on the analysis will again be psychological rather than sociological; i.e., individual rather than collective variables will be employed. Samples of evidence relevant to all of the kinds of relationships portrayed in Fig. 5 will be presented.

The interdependence of frequency of interaction with positive attraction toward other persons. Perhaps the strongest evidence from which frequency of interaction clearly emerges as the independent variable, in this relationship, has been provided by Festinger, Schachter, and Back [11, chap. 3]. They demonstrate that, in a university housing project whose occupants had originally been assigned living quarters on a strictly random basis, subsequent sociometric choices were closely related to measures of contiguity and contact. Closely parallel findings are reported by Deutsch and Collins [9], in a study of whites' attitudes toward Negroes in an interracial housing project where contiguity was determined by chance.

Not many studies are to be found in which attraction appears as the independent and frequency of interaction as the dependent variable;

perhaps the relationship has seemed too obvious. Newstetter, Feldstein, and Newcomb [37, chap. 12], however, have shown that, in a summer camp where boys were free at nearly all times to choose their associates, observed "compresence" varied closely with expressed personal liking. A mean correlation of .72 is reported, for seven groups of 30 boys each; since the reliability of the index of compresence is given as .84, it appears that most of the variance in frequency of interaction can be accounted for (under the conditions of this study) by personal attraction.

Available evidence does not indicate, however, that so large a proportion of the variance in personal attraction can be attributed to sheer frequency of interaction, as an independent variable. A significant proportion of the variance, in the situations obtaining in the available studies, must apparently be attributed to other factors, to a consideration of which I now turn.

The interdependence of frequency of interaction and objective similarity of attitudes. That the latter tends to increase with the former is suggested by many studies, not all of which have attempted to control for initial attitudes. Sims and Patrick [43], in a study of attitudes of Northern whites toward Negroes in a Southern university, show that their first-year students differed hardly at all from "typical" Northern students in Northern universities, thus apparently ruling out any important influence of selection. Their third- and fourth-year students differed hardly at all from Southern students in the same Southern university, and mean attitudes of second-year students were exactly halfway between those of freshmen and upperclassmen. The possibility that these highly significant differences may be inflated by self-elimination of upperclassman with deviant attitudes is not excluded; but similar results in an otherwise different kind of study reported by Newcomb [31] cannot be accounted for by student attrition.

The proportion of variance in similarity of attitude which can be attributed to frequency of interaction varies enormously, almost certainly, with a wide range of parameters. Chief among these, perhaps, is personal attraction, which, under conditions of freedom to choose, is known to vary closely with frequency of interaction. The evidence concerning attraction as related to attitude similarity is discussed below.

That existing similarity of attitudes tends to determine subsequent frequency of interaction is more frequently attested by everyday observation than by properly controlled studies.¹¹ It may be plausibly assumed

¹¹Partial analysis of a body of data recently gathered by the writer seems to provide support for this proposition. Frequency of observed association of men chosen as initial strangers to live in a student house under fraternity-like conditions is significantly related, several months later, to certain preacquaintance attitudes. (See *History and Prospects of the System in Mediating Research*, p. 416.)

that adequate data, if they existed, would show that some but by no means all of the variance in selective association among persons in new situations is determined by existing similarity of attitudes toward objects regarded as important. As noted in a preceding paragraph, a considerable part of this variance must be attributable to interpersonal attraction, a variable which itself is closely related to objective and/or to perceived similarity of attitude, as we shall see.

The interdependence of frequency of interaction and perceived similarity of attitudes. If frequency of interaction is considered as the independent variable, at least part of any consequent increase in objective similarity of attitudes must be presumed to occur via the intervening variable of "perceived attitude of others." Such data have not often been reported; in one of the few such studies, and perhaps the earliest [31], it is shown that with regard to a rather wide range of public issues upper-class students tended to view their own attitudes as more like those of other upperclassmen than like those of freshmen; freshmen, similarly, tended to regard themselves as most like their own classmates. Upperclassmen, of course had interacted much more frequently with each other than with freshmen, and freshmen somewhat more frequently with each other than with upperclassmen; it was the upperclassmen who saw most similarity with each other and most difference with the other group. Each group was more accurate in estimating the attitudes of its own than of the other group. Such findings suggest that most, though not all, of the variance in objective similarity of attitudes that is contributed by frequency of interaction is attributable to variance in perceived similarity.

No studies seem to have been made in which perceived attitudes of others can unambiguously be regarded as the independent variable, though at least one set of findings (see page 418) may be plausibly interpreted as showing that recent acquaintances tend to spend most time with those perceived as agreeing with them. With regard to a more specific kind of interaction, however—namely, persuasive communication—the findings quite clearly show an inverse relationship between frequency of interaction and perceived agreement; i.e., not surprisingly, persuasive communications are most frequently directed to those perceived as being in disagreement. But this finding, as noted below, varies with the communicator's attraction toward the person perceived as disagreeing with him.

In various ways, then, the evidence concerning circular effects between behavioral interaction and each of three attitudinal variables raises questions about relationships among the attitudinal variables. From the evidence already cited, to the effect that all three of them tend to be associated with frequent interaction (under certain conditions, at

least), it is predictable that all three of them will tend to be associated with each other. Again, samples of evidence relevant to the three kinds of relationship will be presented.

Personal attraction and objective similarity of attitude. Under conditions of voluntary association, and with regard to attitudes toward relevant objects, these two variables are proverbially associated. But exceptions, too, are proverbial; devoted spouses and best friends often disagree about matters of great relevance. By way of documented evidence, a study of a college community may be cited [31] in which attitudes toward certain public issues were shown to be highly relevant, for the community at large (though not necessarily for every pair of students). Among those more than 1.5 standard deviations below the mean of the total population (in the "approved" direction), friendship choices were given and received with from two to three times the chance expectancy; whereas among those equally extreme in the "disapproved" direction, friendship choices were exchanged with approximately chance frequency. For most of the latter group, as distinguished from most of the former, the attitude objects were not very relevant. But even among the former group, only some 25 per cent of all choices were exchanged among those attitudinally similar, by this criterion. These findings are consistent with others: comparatively little of the variance in observed personal attraction can be accounted for in terms of similarity of attitude toward any single object.

Personal attraction and perceived similarity of attitude. It seems altogether likely that much of the observed relationship between attraction and objective similarity of attitude is traceable to perceived similarity of attitude. This presupposes, of course, some degree of accuracy in judgments of similarity. As a matter of empirical fact, all the evidence that I have seen indicates that, within face-to-face groups (and in many other groups, too), two kinds of effects may be observed: "realistic" effects, which result in fairly accurate judgments; and "autistic" effects, as a result of which judgments are distorted by attraction—i.e., exaggerated estimates of similarity with others toward whom positive attraction is strong, or of discrepancy with others toward whom negative attraction is strong. The most striking instance of autistic effects known to me is shown in responses to a questionnaire dealing with the then recent dismissal of General MacArthur by President Truman [30]. On this issue, 48 of 48 self-designated "pro-Truman" subjects responding to the questionnaire attributed "pro-Truman" attitudes to "most of my closest friends"; whereas 34 of 36 "anti-Truman" respondents similarly attributed their own attitudes to their closest friends; 32 of 38 who were "pro-Truman" and 13 of 27 who were "anti-Truman" attributed "anti-Truman" attitudes to "most uninformed people." Although inaccuracies

of estimates are almost invariably in the "autistic" direction, they are rarely as extreme as this.

These responses, since they were obtained from subjects who had no way of knowing the correct answers, must be interpreted as showing the effects of attraction as the independent variable. Evidence which shows the same relation, with perceived similarity as the independent variable, does not abound. One of the clearest demonstrations is by Schachter [41], who placed a confederate, instructed to express disagreement, in each of several groups which were discussing a relevant issue. In all groups the confederate was sociometrically rejected at the end of the meeting, whereas in control groups (where the same confederates had been instructed to express agreement) they were not rejected. It must be stressed, however, that these findings come from a laboratory experiment in which the subjects were strangers who had discussed only one issue. In situations characterized by continued interaction, and by members' familiarity with each other's personalities and with each other's attitudes on many issues, it would be much more difficult to predict attraction from perceived similarity alone.

There is a small literature on "reference group" influences on attitudes which illumines the relationship between attraction and perceived similarity of attitudes. If we assume that attraction to members of one's own religious group, for example, tends to be positive, then experiments by Kelley and Volkart [20] and by Charters and Newcomb [7] may be so interpreted. Both experiments showed that Catholic students for whom Catholic membership had just been made "salient" made attitude responses more consistent with those presumably characteristic of most Catholics than did comparable groups for whom Catholic membership had not been made salient. (The former study found this result for high school but not for college students; the latter used university students only.)

Objective similarity and perceived similarity of attitude. That perceived similarity, as a dependent variable, may be facilitated by objective similarity, provided that there is opportunity for discovering that actual similarity exists, seems obvious. Many studies, employing the procedure of pretest, feedback concerning others' attitudes, and posttest [e.g., 17] have shown that the obvious does in fact occur—provided the source of information is considered trustworthy.

The relationship of perceived similarity, as an independent variable, to objective similarity is quite different. The perception of another's attitudes as like one's own is likely to increase their actual similarity only in an indirect sense—i.e., (1) if the perceived similarity is greater than the actual similarity; and (2) if the perceived similarity serves to in-

crease attraction to the other person, whose influence decreases the initial discrepancy. This effect may be forestalled, however, by the discovery that the previously perceived similarity was illusory.

Thus the evidence, particularly as presented in several studies by Festinger et al. [10], suggests that increase in objective similarity is facilitated by perceived discrepancy, rather than by perceived similarity, of attitude. This proposition, as Festinger is careful to note, presupposes positive attraction, and evidence like Back's [4] suggests that the effects vary directly with the degree of positive attraction.

The evidence so far presented may be summarized as follows:

1. Personal attraction, objective similarity, and perceived similarity of attitude all vary, under partially known and partially unknown conditions, with frequency of behavioral interaction.

2. Each of the three attitudinal variables covaries with each of the others, under partially known and partially unknown conditions.

3. The covariation between each of the attitudinal variables and interaction depends, in part at least, upon interrelationships among the attitudinal variables themselves.

4. The interrelationships among the attitudinal variables depend, in one way or another, upon some aspect of behavioral interaction. It is, of course, the evidence for multiple interdependencies which suggests the need for system analysis, as outlined in the preceding section.

The foregoing body of evidence has included very few distinctions among the many possible variables in terms of which behavioral interaction might be studied; frequency of selective ("voluntary") association, and of persuasive communication have been the principal ones so far. Both common sense and the available empirical data (see page 411) suggest that most of the variance in selective association, under conditions of "free" choice, can be accounted for by variance in attraction. Variance in communicative behavior, on the other hand, appears to have much more complex determinants. Viewed as a dependent variable, therefore, it is with regard to interaction as communication, and not merely as association ("doing something together"), that much of the variance is still unaccounted for. Viewed as an independent variable, moreover, the contribution of behavioral interaction is much more problematic with respect to communication than with respect to association, which may be regarded primarily as providing opportunity for communication to take place. The remaining samples of evidence, therefore, will have to do with communicative forms of behavioral interaction, and in particular with reference to the concept of "strain."

The work of Festinger and his associates [especially as summarized in 10] provides a body of experimental evidence that is directly rele-

vant.¹² Their findings are presented with the specific proviso that communications are considered to be those "which arise from pressures toward uniformity in a group [in which] the communicator hopes to influence the person he addresses in such a way as to reduce the discrepancy between them" [10, p. 6]. Their studies provide a good deal of support for the following propositions (their terminology is here "translated" into the language of the present paper): (1) frequency of communication varies directly with perceived discrepancy, with object-relevance, with attraction, and with expected success in changing the other's attitude; (2) attitude change following communication varies directly with "pressure toward uniformity" and with attraction, and inversely with anchorage in other person-relationships and with personality-determined committedness to existing attitude.

A smaller body of evidence from Heider and his associates [14, 16] suggests that "imbalance" is psychologically stressful. Specifically, Jordan [18] has shown that the combination of "liking" another person and of perceiving his attitudes as divergent from one's own is "unpleasant."

HISTORY AND PROSPECTS OF THE SYSTEM IN MEDIATING RESEARCH

Though the present formulation has borrowed heavily from others, together with their supporting empirical data, a considerable part of its empirical support has been found in research (my own, or that of my students) instigated by the demands of the developing formulation. These investigations have provided partial, or in some cases striking, confirmation for each of the following generalizations:

1. Following reports to subjects of the attitudes of attractive others (sometimes groups, sometimes individuals), those attitude changes which occur are predominantly such as to be strain reducing. Thus the greater the experimentally induced increase in self-other discrepancy, the greater the amount of attitude change and the more certainly change is in the direction of reducing discrepancy [22, 26, 46].

2. Attitude change, following experimentally induced increase in self-other discrepancy with attractive others, is less on the part of subjects who do than of those who do not anchor their preexperimental attitudes in agreement with other (extraexperimental) groups or individuals [46]. Strain is tolerated in the experimental situation because, presumably, to reduce it by attitude change would induce still greater strain in other, competing, systems of orientation.

¹² Since the relevant findings are drawn from a large number of quite diverse experiments, some of which have already been cited, further details are not presented here.

3. Inaccurate judgments of the attitudes of attractive others, whether individuals or groups, tend to be strain reducing—a process which may be labeled “autistic displacement of others’ attitudes toward agreement with oneself” [19, 22].

4. Tolerance for discrepancy—i.e., maximum discrepancy beyond which changes in the system of orientations are likely to occur—tends to be an individual constant, and is related to personality measures purporting to measure “conformity” [3].

5. Following the experimental presentation of information contradicting the previous assumptions about others’ attitudes, systems of orientation are very likely to be changed if the information is accepted, and unlikely if it is not accepted, a great majority of changes being strain reducing [26]. The least common kind of change in this study was in attraction; the most common was an extension of the range of acceptable attitude alternatives, equivalent to an extension of the area of “agreement.”

6. Accuracy of judging others’ attitudes varies directly with frequency of communication within the middle ranges of “liking,” and in the higher ranges of “trust” [27]. Apparently the extremes of “liking” introduce autistic distortions, and low degrees of “trust” make it difficult to evaluate information received.

7. The hypothesized effects of communication are facilitated by similarity of the cognitive structuring of the communicators [39]. According to this study by Runkel, students who were cognitively “colinear” with instructors received significantly higher quiz grades than others, regardless of the similarity between students’ and instructors’ cathectic attitudes; and among students living in the same house, attraction tended to increase more among “colinear” pairs than among others.

8. Finally, objective (as well as perceived) agreement tends to be associated with both positive attraction and with frequency of communication. Indices of the latter variable are derived in one case from self-reports of communication, in a large organization [27], and in the other [44] are inferred from the nature of membership or reference groups (e.g., “ten best friends” vs. “rich people”).

In so far as my own concerns have influenced these investigations, they tend to have moved from demonstrations that the propositions of central importance to the system find empirical support to inquiries concerning their limiting conditions, and concerning additional parameters that must be taken into account.

First among my present research concerns is the necessity of studying change over time on the part of interacting persons. This priority stems from my long-range objective of developing a “multiperson psy-

chology¹³ which, on the one hand, is faithful to the empirical facts concerning both orientations and communicative behavior of individuals and, on the other hand, can be applied to the attitudinal and behavioral relationships among persons in such a way as to be empirically faithful to the facts about groups. The best way of testing and extending the propositions required for such a systematic approach is to start, *de novo*, with a set of persons who have not yet developed, but predictably will develop, the kinds of relationships characteristic of group members. My current research, therefore, is being carried on among populations of subjects recruited as complete strangers to one another and simultaneously placed in a setting where, as a result of their joint responsibility for living and eating arrangements over a period of several months, orientations toward each other and toward many common objects will predictably develop and change over time, and where certain indices of communication are available. Data have been obtained before their acquaintance, and at weekly intervals during the entire period of their interaction. The adequate testing of the propositions of any such theory as the present one demands that propositions about change, as well as those about interrelationships at a given moment in time, be put to test.

I should like to add, parenthetically, that my interest in testing theoretical propositions in this kind of setting stems not from any prejudice against the "artificial" conditions of the laboratory, as distinguished from the "real" conditions of a "natural" situation, but rather from the necessity of creating the complete set of conditions necessary to test the propositions. I prefer to regard my present research setting as being, incidentally, somewhat more "lifelike" than those commonly available in short-term laboratory experiments, but as being essentially a long-term laboratory.

At any rate, the data obtained in this setting have provided both support and discouragement for the systematic approach outlined here. Chief among the latter kind of findings is the discovery that, in this setting, the predicted relationships among the basic variables are not in fact invariably found for the *total* population of *pairs* of persons. For example, it is hypothesized that, among pairs of persons, perceived agreement on relevant and important issues varies with attraction. The obtained correlations, though positive, are in many cases not statistically

¹³ By this phrase I mean to imply that the search for order and regularity in the behavior of multiperson systems may be quite as psychological in nature as the search in single-person systems, providing only that psychological variables are dealt with. Multiperson psychology is not a contradiction in terms, in the sense that multiperson physiology would be, because psychological events on the part of interacting persons affect each other with an intimacy and directness that physiological events on the part of different persons, apart from their psychological effects, do not.

significant. What does appear, however, is an extremely close relationship between these variables toward the extremes of positive attraction and perceived agreement. This fact, in conjunction with another one—to the effect that, after four months, estimates of the attitudes of closely associating persons tend to become very accurate—has an interesting consequence. Thus the highly cohesive subgroups which gradually develop tend to be characterized not only by very high perceived agreement (especially on generalized “values”) but also by very high actual agreement. With regard to generalized attitudes that do not change much, the consequence is that eventual subgroup formation can be pretty well predicted from preacquaintance agreement.¹⁴

Two considerations make it necessary to draw upon the system variable of perceived similarity to account for such findings. (1) Even the highest levels of attraction cannot be accounted for in terms of *actual* agreement *on first acquaintance*, when estimates are not very accurate. (2) At early stages of acquaintance, high estimated agreement predicts about as well to high attraction as it does several months later. This, together with a good deal of other evidence, provides very strong support for the following summary statement. The variable of perceived similarity is necessary, though not sufficient, to account for those interpersonal relationships which distinguish highly cohesive subgroups from all other possible subgroupings within a larger, face-to-face population.

This statement—which may not, of course, be invariably supported by future research—seems to me to point to the need for *some* kind of theory (not necessarily the present one) of systemlike structuring of individual orientations toward persons and toward common objects in the world of the orienting person and the person oriented to. I draw this conclusion simply because the potency of the variable of perceived agreement must itself be accounted for. This I have been unable to do, up till now at any rate, without taking into account all of the kinds of variables and constructs noted in this paper—including, in particular, the construct of strain, without which the dynamic interrelationships among the several elements in systems of orientation seem incomprehensible.

This has been a rough and perhaps premature attempt to systematize the conditions which I believe it necessary to take account of if one is

¹⁴ Statements in this and the following paragraph are based upon findings from a single, 17-man population; analysis of data from a second, similarly constituted group is not yet complete at this writing. No attempt has been made here to present complete results from the study here reported in part. Two very partial reports have been published, and a complete report will eventually be issued in the form of a monograph.

to understand how it happens that human beings selectively assort themselves into the more or less enduring associations which influence so much of their behavior. I should like to close with the reminder that, though it has sometimes been necessary for me to describe the phenomena of my concern in collective, or sociological, terms, I have tried to deal with systems of orientation in terms of individual-psychological variables and constructs. I shall be content if I have made it seem plausible that something like "individual systems of orientation" do seem to be operating in the world of persons and objects in which every socialized human lives. I shall *not* be content until systematic formulations have been developed that are more adequate than this one.

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A PRELIMINARY STATEMENT TO A THEORY OF ATTITUDE STRUCTURE AND CHANGE¹

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GENERAL APPROACH

One general theoretical approach to the problems of psychology zeros in on some limited aspect of behavior which occurs under restricted conditions but which permits precision in experimentation and the use of mathematical models. This approach can use a very simple model of man because it is dealing only with behavior which can be manipulated in the very limited conditions of the laboratory. Another approach to the problems of psychology is more concerned with accounting for the wide variance in behavior as it occurs characteristically in the real social world. Here interest begins with an attempt to identify the significant variables in social behavior and to understand and predict major social outcomes.

Both approaches are probably necessary for the development of our science, though psychology is not exempt from the impact of fad and fashion which may give a single approach current prestige and popularity. The merit of the first approach is its ability to produce firm knowledge and to be relatively free of the nonscientific pressures of the practical world. Its risk is that arbitrary and narrow limitations predispose toward trivial outcomes. The blinders the experimenter puts on may keep him in a scientific cul-de-sac. His limited theoretical approach may further success in his special narrow field, but prevent his rising beyond it. Psychophysics may develop a sound set of findings but it may never become a substitute for a valid psychology of perception.

The second approach is strong in its sensitivity to the complexity of social behavior, its possible discovery of significant variables, and its emphasis upon major rather than minor sources of variance. It runs the danger of overconcern with the phenotypical, of imprecision in re-

search, and lack of firmly based principles. In essence, the first approach is strong in method and weak in content, the second is weak in method but strong in content. Both approaches are needed, however; interaction between the two can be fruitful. Laboratory experimenters can be pushed to consider factors which seem to account for a greater share of the variance. The social scientist can be pushed toward greater precision in the formulation of his theory and research. Essentially we use the second approach, but in dealing with a wide range of social facts we are utilizing laboratory concepts and findings in an attempt at integration. Our concern is not with a model heavily restricted to one aspect of behavior but with the attempt to apply many concepts to the social field.

Because we emphasize the second approach, we cannot follow in detail the general plan of presentation suggested in the discussion outline for the present study. Many circumstances enforce a more freely discursive presentation, notably, the unsettled state of the attitude area, the incipient character of the present formulation, and (possibly) certain limiting conditions imposed by the character of the domain under study on the modes of systematization that may be achieved.

There is little agreement about the proper operational measures of the important variables which should be considered and little validation of the measures that have been used as indicators of attitudes and the related variables of motivational processes, value systems, and defense mechanisms. We shall attempt to define the structural characteristics of attitudes, to describe the motivational processes related to these characteristics, and to state our assumptions about attitude change, but we cannot systematically develop the conceptual properties of all our constructs, point to validated operational measures for them, or describe an appropriate mathematical model for handling the data in this field. We regard this paper rather as a preliminary airing of considerations which look toward theory than as a theoretical formulation. Since a position must be developed before detailed analysis can become fruitful, at this stage, the emphasis must necessarily be on presenting our ideas.

One difficulty to be anticipated in any attempt to achieve analytical characterization of the present approach is that the conventional independent-intervening-dependent variable framework may not prove entirely apt for theories which do justice to the interactional character of social life and experience. Nearly every phenomenon of an individual's social life which is influenced by another factor also has some influence upon that factor. Perceptions are influenced by motivation, and in turn, social motives are influenced by perception. Group membership can determine attitudes, and attitudes can determine group membership. At one stage or another in the development of an individual or of a social unit, a factor may exert more influence than it receives. This im-

balance may, however, be mere happenstance. A general theory should not be invalidated because at another period the influencing factor becomes the influenced factor. For example, during a period of social stability, group membership may be a dominant influence on the attitudes of members. During a period of social change, the attitudes of people may determine group membership. Theories based upon the proposition that attitudes are a function of group membership would be as limited as those which hold that group membership is a function of attitudes. To be truly general a valid psychological theory must, therefore, encompass both directions of influence.

This statement should not be construed to mean that some factors do not intrinsically exert more influence than others nor that all potential targets of influence are equally open. In a general theory, the degree of influence attributed to various factors should not be determined by events observed at a particular time. The degree of influence attributed to a factor should have such sound grounding in the theory that no shift in the interactional balance would be beyond the scope of the theory.

To postulate that certain independent variables govern certain dependent variables implies a noninteractional paradigm that does not do justice to the complexities of social life. The independent and dependent variables may reverse roles. For operational purposes in the conduct of research and experimentation, the independent-dependent formula is indispensable. It is not as useful for the development of theory. At the theoretical level, it may be more valid to conceive of an interactional system in a state of changing equilibrium but always moving toward balance. Factors outside the system may impinge at one or more points and the resulting change may reverberate throughout the system until that reaches some state of balance.

We postulate that the motivational component has greatest influence on the other parts of the system. That component is thus analogous to the independent variable, whereas attitudes are analogous to dependent variables. The attitude itself can be considered an independent variable affecting behavior, however, and it can also affect the independent variable of motivation. In one sense, the attitude can also be considered an intervening variable since it is a derivative of motivation which determines behavior. Yet it violates the cardinal principle of an intervening variable in that it has an effect upon its own independent variable. For example, an individual's desire for an object can be influenced by his attitude toward it. Or his attitude toward an object may determine whether or not he experiences it as need-satisfying.

There is no logical reason why the formulation of independent-dependent variables cannot be reversed within the conventional frame-

work. In practice, however, the traditional formulation tends to become a one-way street. It may be possible to fit the interactional characteristics of social data into the conventional framework derived from laboratory investigations, yet one may question the advisability of forcing such an outcome now. To be sure, such forcing may help develop subtheories of very limited scope, some of which can later be integrated into a more general theory. But since currently so many writers are interested in the development of limited subtheories, we have chosen to look at a wider range of phenomena.

Historical paradox. The term attitude, if not the concept, has been remarkably durable in the literature of social psychology. Thirty years ago, Read Bain [9] and Percival Symonds [66] read the term out of existence for sociologists and psychologists alike. Few have loved this orphan child, born in controversy and fostered in hostility, yet fewer have been able to abandon it. It has served rather contradictory functions for opposed theoretical approaches. The behavioristic system needed the concept of attitude both for flexibility and for the opportunity of getting inside the head of the robot. The field theory of Krech and Crutchfield needs the concept to give some stability and rigidity to their flexible system and also to give some elements out of the total field with which one can meaningfully work.

J. B. Watson defined the field of social psychology as the study of attitudes [69], and the social behaviorists led by F. H. Allport embraced the concept to give flexibility and adequacy to the mechanistic model of man [2]. The logic of the behaviorist system of stimulus and response, of the conditioned response and the resulting habit patterns, did not require an attitudinal concept. Yet dealing sensibly with human beings, with their cognitive representations of their experiences, their self-rehearsal of such representations, and the meaning they found in minimal cues, did call for the additional concept of attitude. Attitude was first introduced into the behavioristic system as a neuromuscular set or predisposition to respond to a certain stimulus or type of stimulus [2]. It did include verbal sets to respond, however, and soon these verbal sets were interpreted not in stimulus-response terms but as the subjective meaning the attitude had for the individual. Thus there was a shift from discussing the attitude to conform to social stimulation, which could be objectively defined, to considering radical and conservative attitudes, with radicalism-conservatism defined not objectively, as the ends of a continuum of response, but as supporting a program with definite social meaning. Attitude then became the back door through which the behaviorists could be as subjective as McDougall and the other mentalists they despised.

With the development of field theory in the hands of Krech and

Crutchfield, we have the counterpart of the behavioristic tour de force [40]. There we have a system which emphasizes the dynamics of the psychological field. The explanatory principles are the dynamics of the organization of that field. The psychological processes of perception, cognition, and motivation are interwoven into one system. In fact the old distinction between perception and cognition is abandoned. Since all processes are part of the same field of forces and since the forces are always more or less in flux, we have a very fluid system. Moreover, the determination of behavior is a matter of the organizational properties of the total field. Hence predictions postulating simple relations between stimuli and isolated or partial processes must be abandoned. The constancy hypothesis is rejected and the emphasis is upon the understanding of the total field of forces.

Again, this system, which emphasizes as explanatory principles the laws of total dynamic organization, does not logically require the concept of attitude. But the practical need for taking account of behavior does call for some stability and for some identifiable affective-cognitive elements which can be related to social behavior and to social situations. Hence the concept of attitude is introduced to allow for the fact that cognitive and affective organization can achieve stability and some degree of constancy. Before the authors have concluded, however, they are discussing elements of the cognitive structure without reference to the total field of forces when they talk about attitudes toward racial and ethnic groups.

One may interpret this historical paradox in two ways. First, one may hold that a concept which can be seized upon by opposed theoretical systems for opposed purposes is meaningless and should be abandoned. Or, one may contend that dealing with social realities brought the narrowness of the systems into bold relief and that, in practice, the concept of attitude offered their extreme positions a common meeting ground. It does not follow, but it is possible, that instead of dropping the concept of attitude, future theoretical systems must either include it or present similar concepts to account for essentially the same phenomena.

OUTLINE OF THE THEORY

The structure of attitudes. *Definition of attitude.* An attitude can be defined as an individual's tendency or predisposition to evaluate an object or the symbol of that object in a certain way. Evaluation is the attribution of qualities which can be placed along a dimension of desirability-undesirability, or "goodness"- "badness." Evaluation in this sense always includes cognitive and affective elements; hence presenta-

tion of the object or its symbol may elicit the attitude. Judgments which are purely cognitive would not fall into the category of attitudes. Evaluations are termed high when the attributed qualities are desirable and low when they are undesirable. A direct operational measure of an attitude would be the elicitation of verbal statements of goodness or badness about some object or symbol; for example, the subject may check agreement with the statement that Communists are untrustworthy. An indirect operational measure of an attitude would be the elicitation of affective responses such as "I dislike foreigners." The assumption in this indirect measure is that the person expressing the dislike will also have a definite evaluation of the object of his dislike.

Affective expressions do not always contain an evaluation, however. The work of R. S. Lazarus and R. A. McCleary indicates that people show emotional response to nonsense syllables which have previously been associated with shock when these syllables are not consciously recognized [43]. The concept of attitude does not include such affective response without cognitive evaluation.

Attitudes or evaluations thus have both an affective and a cognitive component. The amount of cognition may be minimal; it need merely specify the object sufficiently for its recognition and relate the object to some evaluative standard. In addition, some attitudes may have a more elaborated cognitive component, including beliefs about the object, its characteristics, and its relation to other objects, including the relation to the self. Attitudes may also include a behavioral component. The behavioral component refers to an action tendency toward the object of the attitude in addition to the expression of affect about it. For example, one may regard impressionistic art as desirable but not go to a museum of modern art, read about impressionism, or acquire prints of impressionistic paintings. An individual who has an attitude with a behavioral component, on the other hand, has some degree of impulsion to do something to or about the object. In our usage, the behavioral component corresponds closely to the term *orientation* of an attitude as employed by M. B. Smith, J. S. Bruner, and R. W. White to characterize the action tendencies aroused by the object of an attitude [62].

The Affective Component

The affective component is the central aspect of the attitude since it is the most closely related to the evaluation of the object. In evaluating the object some elements of cognition are necessary; the object must be recognized and must be related at least implicitly to other objects and beliefs. Nevertheless, it is the affective element which differentiates attitudinal evaluation and intellectual appraisal. A person may have beliefs and judgments about various objects and aspects of his world,

but these are not attitudes unless an attribution of good or bad qualities accompanies the specific belief. We believe this emphasis upon the affective component is consistent with previous theoretical and empirical work in this field. L. L. Thurstone relied heavily upon expressions of affect in his construction of attitude scales [67]. In applying psychophysical methods to the scaling of attitudes, Thurstone and his students first gathered statements about the issue or symbol in question which expressed feelings of liking and disliking, of affection and of hate, and then applied the specific scaling procedure to these statements. For Krech and Crutchfield, the essential difference between a belief and an attitude is that the attitude includes motivational and emotional processes which give to the attitude its *sign* nature, i.e., its "pro" or "anti" character [40].

The affective loading of an attitude may vary in degree, but there must be some minimal affect at the low end of the continuum. Attempts to measure attitudes have frequently been directed only at the measurement of the degree of affectivity of the attitude. This is the usual procedure when a rating scale is employed to measure how strongly a person feels about the issue or symbol under study. As just noted, Thurstone emphasized affectivity in his use of psychophysical methods in the construction of attitude scales. Other workers have distinguished between a positional dimension and an affectivity dimension. Thus, attitudinal statements can be ordered along a continuum representing either degrees of a logical position, e.g., conservatism-radicalism or steps toward the accomplishment of an objective, e.g., specific actions to achieve racial desegregation. E. A. Suchman, using a technique developed by L. Guttman, has distinguished between attitudinal position and degree of affectivity and has obtained separate judgments on the endorsement of statements of attitudinal position and the strength of feeling about these statements [65]. The latter judgments give him his U-shaped curves representing the intensity dimension on the positional scale. Nevertheless, he sees such a close relationship between the two dimensions that he has suggested using the lowest value of the intensity dimension to define the zero point of his positional dimension.

The Cognitive Component

Some attitudes may be quite low in their cognitive component in that there are few beliefs about the attitudinal object and its relations to other parts of the world or to the individual. An individual may have a high or low evaluation of the object but not know very much about it. People will not only reject Turks about whom they know little but will also respond negatively to a term like Wallonians because it must stand for some group of foreigners [25]. The cognitive aspect can vary then

from knowledge of some minimal cue necessary to define the object to a full and detailed description of the object and beliefs about it [39]. Usually these details are integrated into a logical organization of some degree of coherence; i.e., Negroes are a primitive, emotional, inferior people who contribute heavily to delinquency and disease and who must be excluded from white residential areas because property values deteriorate when Negroes move in, etc.

The cognitive component can thus be described according to three basic characteristics. First is the degree of differentiation, namely, the number of cognitive elements (i.e., the number of beliefs). Second is the degree of integration, the organization of these elements into a hierarchical pattern. A third characteristic of cognitive structure concerns the generality or specificity of the beliefs. An attitude with a high level of generality includes many particular objects under the same symbol and thus permits the same evaluation to be made in many specific situations. A specific attitude, on the other hand, is limited to a single object.

The Behavioral Component

Attitudes which have behavioral tendencies associated with them are of especial interest. The individual may take steps to protect or aid the object of his attitude; conversely, he may move to injure, punish, or destroy the object. If he attempts to aid, the attitude is called positive. If he tends to injure, it is called negative. This positive-negative dimension of behavior toward the object should not be confused with a phenotypic description of approach and avoidance. A person may approach a prowler to do him harm and avoid interfering with a child's interaction with his peers to aid the child's development. The effect on the object is the criterion of positiveness and negativity. In many instances, however, this dimension is correlated with approach and avoidance. In the nature of American society, acceptance is highly valued, whereas social rejection or avoidance usually does inflict harm on the rejected person. The Bogardus social-distance scale is based upon the assumption that this correlation obtains.

The cognitive and behavioral components may be closely related in that the impulsion to action can be symbolically represented and even rehearsed. The cognitive component also comprises knowledge of appropriate and inappropriate modes of action toward the object. And, of course, previous actions may be carried in memory. Yet these corresponding cognitive elements need not be present, for an attitude to have an action orientation; e.g., habitual motor outlets, which are the essence of the behavioral component, may lack cognitive representation. Generally, however, we assume presence of an action orientation if we know that the

individual has certain types of cognitive patterns. For example, if the person has a detailed knowledge of appropriate channels of social action, we would say that his attitude has a behavioral component. Accordingly, we would predict that the person favorably disposed toward a political party will be more likely to vote if he knows where the polling place is and when it is open, and if he believes his vote is important for the outcome, than the person who lacks such beliefs related to an action orientation. With respect to this problem, D. Cartwright has discussed the creation of a particular behavior structure in addition to cognitive and motivational structures [13]. He has asserted that "the more specifically defined the path of action to a goal the more likely it is that the structure will gain control of behavior."

Attitudes and Value Systems

As evaluations of objects or their symbols, attitudes have a single focalized referent. This is true even for general attitudes which may include a number of particular objects to which a single symbol applies. This is one reason why attitudes have often been regarded in social psychology as useful units for the analysis of social phenomena. Individual attitudes, however, are frequently organized into larger structures called value systems which are integrated about some abstractions concerning general classes of objects. A person can have a number of attitudes, some specific, others general, about the church of his denomination, its specific practices of worship, its religious symbols, its specific theological doctrines. When these attitudes are organized about some central conceptual themes, they comprise the individual's religious value system. The term *ideology* is often used to designate an integrated set of beliefs and values which justify the position of a group or institution. But whereas ideology is a relatively impersonal concept, the value system refers to the individual's own organization of his attitudes. Value systems resemble the concept of sentiment as used by Shand and McDougall, save that these authors were more concerned with the organization of different emotional predispositions about an object or class of objects than with an integration of different sets of evaluations [61, 47]. We use organization and integration to denote the relating of attitudes to one another in a hierarchical arrangement based upon abstraction and generalization. The individual may make logical slips in the process of abstracting and generalizing, but he does emerge with a hierarchical pattern. Later, when new experiences show logical discrepancies in the value system, difficulty may arise. Nevertheless, imperfect though the integration may be from a logical standpoint, its presence means that the individual has an organized system with some "logical" subordination and superordination of attitudes.

This system is also likely to contain additional beliefs and evaluations which justify and enrich the logical generalizations. For example, the person whose attitudes toward big business, labor unions, and social welfare legislation become organized into a value system of economic radicalism may then see the leaders of big business conspiring to thwart economic reform.

When beliefs are organized in hierarchical fashion without the inclusion of affective judgments, we speak of belief or cognitive systems rather than value systems. A person may have a belief system about the economic order which is merely an objective ordering of the facts, information, and ideas available to him.

Individual attitudes retain their identity even though they may be part of the larger structure of the value system. And it is possible for the same attitude to be part of more than one value system. An important characteristic of an attitude is the degree to which it is linked to a value system. At the one extreme are attitudes which are *isolated* and have no tie to a larger structure. At the other extreme are attitudes which are thoroughly embedded in a value system. A second, though not an independent, characteristic of attitudes is the number of value systems to which an attitude is linked. Tight linkage with one value system may prevent an attitude from developing connections with other value systems.

Such terms as *isolation* and *compartmentalization* are sometimes used to refer to the lack of attitudinal integration. We shall use the term *isolation* to refer to any kind of separation of the attitude from other attitudes and value systems. We shall use *compartmentalization* to refer to one type of isolation, namely, the separation of an attitude owing to the operation of some defense mechanism. Not all isolated attitudes are of this character. They merely may lack sufficient importance or relevance to a value system to attain linkage. The human mind does tend to organize its beliefs and evaluations, no doubt, but the intellectual is apt to overestimate the degree to which the majority of people integrate their attitudes.

Value systems are often involved in the individual's self-concept. He has an image of himself as having certain values; hence a threat to such a value system may arouse the same emotional response as does a more direct threat to the ego. The extent to which a value system and its related attitudes are tied to the self-percept is an important consideration in attempts to modify a specific attitude [15].

The nature and extent of the tie between attitudes and value systems has important implications not only for the amount of pressure which needs to be mobilized to change attitudes but also for the type of force which will produce change. A number of assumptions in this statement

need to be made explicit. First, we assume that the affective component of the attitude will be reinforced if it is locked into a value system. The value system will have affectivity in its own right over and above the affective components of the individual attitudes it includes. Second, information contradicting a specific attitude which is closely linked to a value system will result in the mobilization of some beliefs of the system. For example, if a person who has a low evaluation of Jews is confronted with evidence of their intelligence, his value system of ethnocentrism may give him defense in depth through beliefs about the consistent political superiority of Aryans over non-Aryans, the undesirability of miscegenation, etc. Thus, the amount of informational support of an attitude, to use a term from Smith, Bruner, and White, derives not only from its own cognitive component but from the value system of which it is a part [62]. Finally, the influence being brought to bear in an attempt to change an attitude must take account both of the content of the value system to which the attitude is tied and of the motivational force which the value system may reflect.

Attitudes and motivation. The role of attitudes in motivating behavior has provoked much dispute. One school holds that attitudes help account for the directionality of behavior but that attitudes in themselves are not motivational forces. For example, T. M. Newcomb regards an attitude as a readiness to be motivated in a certain way but sees the motivation as coming from other sources than the attitude itself [50]. The other approach views attitudes as having dynamic qualities. Thus, G. W. Allport sees attitudes as possessing both energizing and directional properties [5]. Our own insistence upon affect as a major component of an attitude places us in the latter camp, since we assume an affective process has energizing properties. Whether pleasant or unpleasant, affect is accompanied by neural excitation of greater intensity or extensity than that which accompanies a cognitive process. Hence, the person with an attitude which includes a behavioral component will need no other motivation than the presentation of the attitudinal object or its symbol to act positively or negatively toward the object. Yet elicitation of the attitude does not inevitably lead to behavior toward the appropriate object. The individual may be motivated at the moment by stronger drives in the direction of another goal; the affective energy of the attitude may even be discharged through the pathways serving the ongoing behavior. For attitudes which lack an action orientation or behavioral component, the presentation of the object will arouse the affective process but may not lead to overt behavior toward the object. It may lead to behavioral and verbal expressions of the emotion or to a diffuse, general feeling of pleasantness or unpleasantness. It may express itself by affecting other behavior occurring at the time.

To help discover the motivational support available to the attitude, it is necessary to inquire into the nature and genesis of the affect which is so central to it. Consequently, we now turn to a discussion of the relationship between basic motive patterns and attitudes. We shall distinguish among (1) affective associations which are by-products of the process of motive satisfaction, (2) functional attitudes in which activity directed at the attitudinal object is satisfying in itself, and (3) attitudes which are instrumental to the satisfaction of the other needs. In addition, attention will be given to ego-defensive needs which involve all of the three processes listed above. Finally, the trend toward consistency will be discussed as a motivational principle.

Affective associations. The attitudes which we term affective associations represent the spread of affect during the process of motive satisfaction to objects which happen to be present at the time. G. Razran, for example, has demonstrated that students showed a greater liking for pictures after these pictures had been presented during the eating of a meal [56]. The pleasant state aroused by the dinner had colored much of the situation. Similarly, we may acquire unfavorable attitudes toward many aspects of our environment because of their accidental association with unpleasant experiences. The child who has a painful experience in the dental chair may come away with an unfavorable attitude toward the smell of the strong soap emanating from the dentist's hands. In other words, the affect from motive satisfaction can spread to objects which are not necessarily instrumental to the satisfaction of a motive but which are associated with such satisfaction through contiguity in time and space. These affective associations become attitudes only if the individual thinks about them sufficiently to evaluate them. If an affective association of this sort is not salient enough in perception or memory, it will probably not lead to sufficient cognitive activity to become an attitude.

To be long-lasting, attitudes resulting from accidental associations require either repetition of the original experience or a sufficiently intense initial experience to have produced emotional arousal. The child in the dentist's chair may not only have felt the pain of having a tooth filled, but his sympathetic nervous system may also have been involved, and as a result, he may have experienced fear. Had he experienced only the pain from the drilling of the tooth, he would quickly have forgotten the incident, but since emotional arousal was also involved, the experience was harder to forget. An attitude based upon such an affective association would also be more permanent, for an appropriate cue would again evoke the emotion and thus reinforce the attitude without need for another visit to the dentist. Fears and phobias thus can arise from a single traumatic experience. The persistence of such attitudes has been most clearly demonstrated in the work on

avoidance learning, in which the association of some object with the negative affect of anxiety leads to a re-arousal of the anxiety each time the associated object is presented [48]. The persistence of attitudes based on association between an object and a pleasant or positive affect is not as well established as persistence based on associations with negative affect.

The functional basis of attitudes. Affective associations are not functional in the individual's adjustment. In a sense, they are excess baggage compared to attitudes which have a more instrumental function in the satisfaction of basic motive patterns. The concept of instrumental learning has been converted into the notion of perceived instrumentality by workers dealing with cognition, perception, and attitudes. Thus, D. Cartwright writes "To induce a given action by mass persuasion, this action must be seen by the person as a path to some goal that he has" [13]. And H. Peak has developed some of the implications of the notion of perceived instrumentality for attitude change [54]. We agree with the essential assumption that many attitudes have a functional significance for the individual since they play a part in the satisfaction of his needs. We do not assume, however, that the means-end character of the attitude as a path to a goal must be perceived by the individual in the process of attitude formation or change. Moreover, we regard this concept of attitudes as instruments for attaining goals as so general that specification needs to be introduced with regard both to types of motive patterns to which attitudes are related and to the role of the attitude in motive satisfaction. Accordingly we shall discuss three patterns which, broadly speaking, are instrumental or functional for the individual's needs: (1) proximal attitudes, (2) object-instrumental attitudes, (3) ego-instrumental attitudes. The major difference among these attitudes is the source of affect arousal. In the first case, the affect is directly associated with the object of the attitude; in the second case, it arises from or is evoked by objects other than the object of the attitude; in the third case, it arises from the functioning of the ego.

Proximal attitudes. Many attitudes of the individual are evaluations of objects which satisfy his needs and wants directly. In these instances, the attitudinal objects are instrumental in motive satisfaction with respect to the physiological source of the need but are consummatory with respect to psychological gratifications. For example, a person will place a high value on the foods he finds especially satisfying and upon the motor car which gives him a sense of power when he is at the wheel. These attitudinal objects have value for the person in and for themselves and are not easily substitutable in motive satisfaction. In contrast are the object-instrumental attitudes in which the object is valued as a means to some further goal. Here the object can be replaced by another object which has the same means-value without any felt loss by the individual.

When we leave the area of appetitive drives, it is difficult to infer whether the individual likes an object in and for itself or whether it is a means to some other goal, unless we have full reports from our subject or can study him over time. And in many instances an object can be both satisfying for the person and also a means to other satisfactions, as in the case of the Irishman who was paid to pull down a Protestant church.

Since the object of a proximal attitude gives satisfaction to the individual when he behaves positively toward it, the attitude will receive reinforcement with every repetition of such experience with the object. Hence, childhood preferences for food are often difficult to change. The mechanism by which the affect arises is the same as in the case of affective association, but since this latter type of association involves irrelevant objects, it is not necessarily reinforced through repeated elicitations of the same motive pattern.

Proximal attitudes are based on the principle that individuals put high value on objects which satisfy their needs and low value on objects which frustrate them. The need-satisfying or frustrating quality of an object we term its functional value, yet objects which are functional from a physiological standpoint may not be classed as "good" or "bad." We do not necessarily evaluate water or air as "good," although they are objects which satisfy basic needs. An individual is most likely to evaluate functional objects if he experiences some period of frustration or deprivation. During this period, he may try various means for satisfying the need before finding the appropriate object. Easy or automatic satisfaction of needs, as in the case of the need for air, will not lead the individual to make an evaluation of the functional object.²

It should be pointed out that attitudes based on such direct commerce with the object would tend to have behavioral components, since it is by acting on the object that the individual satisfies his motive. When attitudes based on such functional relationships to the object have behavioral components, they also tend to have a well-elaborated cognitive component. These latter aspects of the attitude often consist of the information about the object which is necessary for acting effectively toward it. All the features of the object which might influence the effectiveness of action toward it would be included in the cognitive component.

An important principle of motivation can be mentioned in con-

² It should be understood that the distinction between readily satisfied and frustrated needs is a matter of degree, so that attitudes can vary along a dimension of affectivity resulting from the degree of frustration. In addition, the affective-evaluative aspects of attitudes may not be developed in relation to experience with the object. Some may be taken over ready-made from social groups, in which case the degree of frustration is irrelevant.

nection with attitudes directed toward objects which have a functional value for the individual. The relatively constant value of such objects leads the individual to try to assure himself of their continuing availability. Thus his action orientation toward the object will include efforts to protect and preserve the sources of his satisfaction. The individual will also treat in terms of a temporal frame of reference objects with negative functional value, i.e., those which have frustrated or harmed him. He may seek means to destroy them or to protect himself against future harm. Consequently, the behavioral component of this type of attitude can include action tendencies which were not part of the original mode of dealing with the object.

Proximal attitudes and the need for understanding. We have discussed proximal attitudes with only casual reference to the kinds of motives which lead to their formation. Although the satisfaction of appetitive drives like hunger and sex furnishes the clearest examples of relevant motive patterns, other motives will lead to proximal attitudes. Among the most important needs to be mentioned in this connection is the need to understand. Here, the motive has been variously described as curiosity, exploration, the need to control one's world symbolically, the search for meaning, the tendency for more inclusive and stable organization and cognitive structure. Human beings are characteristically troubled if they cannot obtain enough information and ideas to resolve the confusions of a chaotic and disorganized picture of their immediate universe. Spranger includes this pattern as one of his basic value types [63]. Much of our everyday communication practice assumes that supplying information to people about their problems will form and modify their attitudes. Much of modern communication theory implicitly assumes this model, for the amount of relevant information on the input side is an important factor in the predicted outcome.

Thus, it would be expected that those objects in the environment which aid in understanding the world would be evaluated highly. Furthermore, if the object itself is clearly understood, it will be evaluated more highly than if it is understood but vaguely. The relationship between ease of understanding an object and evaluation of it has been demonstrated by A. R. Cohen, E. Stotland, and D. M. Wolfe, whose experiments showed that subjects liked clearly written stories better than ambiguously written ones [17].

Attitudes based on the need to understand will often have well-differentiated cognitive components, since this gives the individual a more adequate basis for understanding his world. On the other hand, it is less likely that such attitudes will have a behavioral component.

Object-instrumental attitudes. Indirection of motive satisfaction is characteristic of a complex society. To reach the objects we desire, we

must successfully negotiate lengthy and sometimes circuitous pathways. Many objects or paths which are not rewarding in themselves can be instruments for attaining rewards. Because of the associated affect from goal attainment and because he perceives the instrumental value of the intermediate object, the individual favorably evaluates the objects which are the means of reaching his goals. M. Rosenberg has demonstrated that where the individual saw the object as functional in achieving something he valued, it was regarded more favorably than objects not regarded as instrumental [58].

Instrumental attitudes of this sort differ from affective associations which merely receive the spread of affect from motive satisfaction. The object-instrumental attitude, however, bears a more constant relationship to the consummatory satisfaction. It has been the means for reaching this state and from the individual's own point of view an effective means. It is possible to substitute another means but generally the individual will not seek another path unless the old route was frustrating in some of its aspects. Many of our everyday social attitudes are of this object-instrumental character. People will have favorable attitudes toward the political party which promises them prosperity. Leaders will receive high evaluations if they can assure adequate returns to their followers. Workers will be highly regarded by their employers if they are high producers.

Since the indirection in reaching the goal in this pattern of motive satisfaction occasions some delay and sometimes some degree of frustration, these instrumental attitudes usually have a considerable cognitive component. Not only is there a recognition of the object itself but a perception of the object's function as a means of reaching the goal and frequently some notion about its appropriateness relative to alternative means for accomplishing the same purpose. There will tend to be a behavioral component because the attitude is important as a route to reaching some goal.

To confound our distinction between proximal and instrumental attitudes, people in real life situations over time may find the instrumental object or the path to the goal rewarding in some degree in itself. This resembles Woodworth's notion that mechanisms can become drives [72] and G. W. Allport's doctrine of functional autonomy [4]. For example, people may find that money is such a valuable means for satisfying so many needs that they take pleasure in possessing and handling it. Whether we are dealing with a proximal or instrumental attitude, the practical test of an object's reward character is not merely the individual's report of his source of satisfaction but his behavior when the instrumental act is no longer fundamental for achieving the original goal.

The motivation to behave positively toward instrumental objects may

follow the principle already described in discussing proximal attitudes, namely, the individual's need to assure himself of a continued source of satisfaction. He may seek not only to follow the instrumental path but to maintain it as a stable and dependable means of attaining his goals. Should he perceive no other feasible means available for gratifying his needs, he may evaluate the given path so highly that he tries to preserve it. The worker to whom seniority is the only feasible path to economic benefits will be committed to this instrumentality in a manner which his employer may not anticipate.

Ego-instrumental attitudes. The attitudes just described are based upon the instrumental value of the object of the attitude. Holding an attitude, however, can have another instrumental function, namely, maintaining the individual's conception of himself as a certain kind of person. And in expressing such an attitude, the individual indicates to his fellows the kind of person he is. In the proximal attitude, the affect derives from the object of the attitude, in the object-instrumental attitude, from the goal object to which the attitude is instrumental. With the third type of attitude we are describing, the affect arises from sources further removed from the attitude itself, from ego satisfactions. For example, a middle-class person may hold and express attitudes which are typical of the upper class because he sees himself as basically a member of the upper class.

The crucial point about attitudes based on this type of motive is their relative independence of actual interaction with the object of the attitude. With other sorts of instrumentality, the individual has dealt with the relevant objects and has found them satisfying either in themselves or because they are closely related to the end object. Thus, there is an experiential contact with the object of the attitude that is not essential to the ego-instrumental attitude. A person may know little about polo, he may never have seen it played, but he evaluates it highly because holding such an attitude bolsters his self-concept.

The basic behavioral component of such an attitude is a tendency for the person to express it to an appropriate audience as well as to himself. He must prove to himself that he is a certain sort of person and the expression of the proper attitudes aids him to achieve this purpose. The cognitive component becomes elaborated less upon the basis of the objective characteristics of the attitudinal object than upon the individual's need to maintain his own self-image.

The attitudes involved in conformity behavior may differ in nature though their expression may not yield palpable differences to the observer and in some cases may result in the same social consequences [8]. Some people conform to the norms of a group because they seek the specific rewards of group membership or want to avoid group censure.

Their expressed attitudes are instrumental to attaining specific objects. Others conform because they identify with the group and see themselves as group members; their attitudes would fall into our third category, the ego-instrumental. In both cases, the conforming evaluations and beliefs are not based upon experience with the object of the attitude. On the other hand, some evaluate the group norm in the positive fashion that is expected of group members because the group expectations happen to coincide with their own individual evaluations—in other words, proximal attitudes based upon their own experiences with the object. H. Kelman has suggested a similar description of conformity behavior though his interest is not in the analysis of attitudes [35]. He distinguishes among (1) the process of internalization in which the individual conforms because the ideas and actions expected of him are intrinsically rewarding, (2) the process of compliance in which he hopes to achieve a favorable effect upon another person or persons, and (3) identification in which the individual conforms because he wants to maintain a satisfying self-defining relationship to another person or group.

D. Riesman has described a generalized personality type which he suggests as increasingly common in our society—the other-directed man [57]. Such a person is sensitive to any cues which will tell him what the group expectations are on any issue or problem. But such a generalized trait could originate in the attempt to use the group to attain one's own specific goals or in effort to identify with the group to support one's self-concept. In the first case, we have the opportunist who can exploit the group for his own purposes. In the second case, we have the conformer who is used by the group for its purposes.

Ego-defensive attitudes. We have separated out the four patterns of affective associations, proximal attitudes, object-instrumental, and ego-instrumental attitudes to describe the processes through which attitudes develop from need gratification. Many attitudes fall clearly into but one among these four categories. One major type, however, combines proximal and ego-instrumental functions and is, therefore, of great strength. Ego-defensive attitudes protect the ego but their expression also gives the individual direct satisfaction. The person who projects his own hostilities onto other people and then attacks these hostile people satisfies two purposes. Projecting his own aggression protects his self-image from a recognition of undesirable qualities. Expressing the aggression gives cathartic release. Before discussing the components of ego-defensive attitudes, however, a more detailed account of the conception of ego defense may be useful.

The internal conflict between our ideal self-image and impulses unacceptable to it evokes many mechanisms of defense. These mechanisms seek to alleviate the conflict by keeping impulses from consciousness yet

permitting them partial expression. The classic pattern is one of repressing sexual and aggressive impulses and projecting these impulses onto others, where they can be properly attacked. The authors of *The Authoritarian Personality* have made a major contribution in showing how attitudes can function in the service of these defense mechanisms [1]. The projection of our own hostility can give us gratification while maintaining the fiction that these impulses originate in others.

There is an important distinction between motive patterns based upon ego defense and those not designed to protect the self-image. In the long view, defense mechanisms are not genuinely problem solving or adaptive; hence, they give only partial satisfaction. The inner conflict continues because, although defense mechanisms give some temporary relief, their net effect is to incapacitate the individual and impoverish his emotional gratifications. The individual devotes so many of his resources to devious means of obtaining slight satisfaction yet keeping the conflicting forces apart that he operates at a low level of psychological efficiency; in extreme instances, he will actually break down. Moreover, in his everyday adjustment he will maintain attitudes in defense of himself which can deprive him of rewards and incur punishment. The man who resents his boss because he is working out some of his relations with his father may deprive himself of advancement and the satisfaction of many other needs. Nor will he necessarily alter his behavior because he is punished. Since he is responding to his own internal conflict, if the degree of external reward and punishment affects him at all, it may be in the reverse direction.

Ego-defensive attitudes thus resemble two of the motive patterns already discussed. They are similar to ego-instrumental attitudes in that they give the ego security through the belief that the individual is superior to others and that these other people have certain unacceptable impulses. Ego-defensive attitudes are like proximal attitudes in that the release of the unconscious hostility against the attitudinal object is in itself satisfying. All aggressive behavior does not have this satisfying quality because the aggression can be an unpleasant means to accomplish a desirable purpose. Not all parents enjoy inflicting physical punishment on their children. In the case of the conflicted person, however, where hostility has been building up, its expression gives the individual positive satisfaction. The object of his negative attitudes may be evaluated in a contradictory manner. The scapegoat, for instance, will be evaluated poorly in many respects in order to justify the hostility. Thus the bigoted person may attribute subhuman qualities to the ethnic groups against which he directs his aggressions. Yet, he has a stake in assuring the continued availability of the scapegoat so that he may have a convenient object for the release of his hostility. Thus, the bigoted person will often

protect the object from harm by others and try to keep it available to him. A sadistic husband will not want his wife to leave him and may take positive actions to assure the permanence of the marriage.

The Principle of Consistency

The motive patterns so far discussed have had to do with the functioning of specific needs. At a more structural level, one can speak of the principle of consistency just as gestalt psychologists talk of principles of organization [71]. In fact, the trend toward consistency may be the most general principle of organization of the psychological field.

That the individual tends to make consistent with one another the various aspects of his psychological functioning is an old doctrine in psychology. Freudian theory starts with the apparent contradictions in human behavior and then proceeds to show how the logic of the unconscious reconciles discrepancies and inconsistencies. Gestalt psychology sees the human mind not as a collection of separate unrelated compartments but as a unified organized system. Krech and Crutchfield adopt this position when they maintain that any change in a cognitive structure will be absorbed in such a fashion as to produce minimal change [40]. S. Asch reinterpreted the old experiments on prestige suggestion which apparently showed inconsistency between the individual's rejecting a statement and then, later, accepting it when it was presented as the statement of a prestigious person [6]. Asch maintained that this represented no contradiction on the part of the subject who made a cognitive redefinition of the statement when it appeared in the context of a favorably perceived authority and saw a different meaning in the statement. More recently, attempts have been made to theorize about consistency at a more elementary level. Thus Heider [26], Newcomb [51], Cartwright and Harary [14], and Osgood and Tannenbaum [53] have assumed a tendency for the individual to achieve a state in which there is consistency between the sign quality (either positive or negative) of his relationship to another person and his acceptance or rejection of the other person's communication or his attitude toward some object.

We shall assume that there is a general but limited trend toward consistency in psychological functioning. We do not believe that the principle of consistency is simple and sovereign. The human mind is too complex, compartmentalization and rationalization are mechanisms too conveniently at the disposal of human beings, and wishful thinking is too deeply entrenched to make consistency a useful predictive tool without detailed specifications about its operations. The pressure upon the individual to achieve consistency arises from the need to avoid conflict. The conflict may be one in which opposing behavior tendencies are

activated at the same time, it may be one in which perceptions contradict previous belief, or it may be one in which feelings and belief are in opposition.

The trend toward consistency exists in its strongest form within the confines of a single attitude; there it seeks to make the components of the attitude congruent with one another. Inconsistencies can exist between attitudes more readily than between the components of a single attitude.

The reason for the strength of the operation of the consistency principle within the single attitude is that the affective, cognitive, and behavioral components are all directly tied to the same object. Thus they represent a molar unit of psychological functioning. It is easier to be inconsistent in dealing with things and people when the inconsistent reactions are separated in time and space. If the components of an attitude were inconsistent so that the person would want to destroy the thing he loved, he would be in a state of conflict. In general, then, the consistency principle will express itself here as a tendency to achieve a logical correspondence among the components of an attitude. The cognitive elements will be congruent with the behavioral tendency both in specifying the object and in describing the most effective and appropriate channels of action. The expression of the behavior component in overt behavior will in turn test out the attitudinal beliefs. The feedback from behavior will lead to a modification of the cognitive map to make it a better guide to behavior in the future. The affective components will also show a high degree of correspondence with the cognitive component. Where the object is cordially disliked, the person will also believe that its characteristics justify such dislike. His description of the same object will vary from the account given by another person who likes the object. Thus, C. Osgood and G. J. Suci found that a person's evaluation of an object accounted for most of the variance in the attribution of qualities to it [52].

Similarly, favorable affectivity will tend to be associated with positive behavioral tendencies toward the object. This can be understood readily by examining the conditions which led to the evaluation of the object in the first place. Affect is related to need satisfaction. Objects which satisfy a need or are associated with need satisfaction acquire pleasant affect and are evaluated favorably.³ The individual will also behave positively toward such objects, since by doing so he assumes the continuance of his present need satisfaction or of need satisfaction in the

³ In general, we can assume that positive evaluations tend to be correlated with need satisfaction. When the need that is satisfied is to express aggression for purposes of catharsis, however, the object of this aggression will be evaluated poorly.

future. Need satisfaction leads both to positive affect and high evaluation and to positive behavior toward the object. We assume that the individual learns that the association among the affective, evaluative, and behavioral aspects of his attitude is the consistent and natural one. In order to achieve consistency among the components of an attitude, he then tends to behave positively toward objects he evaluates highly and to evaluate highly objects toward which he behaves positively.

The concept of *public* and *private attitudes* has been introduced into social science to call attention to apparent inconsistencies in attitudes toward the same social objects. In Schanck's early study of this problem many people in a small community exhibited one set of attitudes toward card playing and drinking for public scrutiny and another set of attitudes within the privacy of their own homes [59]. It is our assumption that, although such discrepancies occur, there is pressure toward their reconciliation and that there will be not only rationalization but also changes in either the private or public attitudes to make them less discordant. In fact, Schanck's later observations of the public-private dichotomy in the same community showed a marked change toward an integration of attitudes. With the death of the community leader whose beneficence supported the local church and with a growing perception of the private views of others, the public attitudes changed in the direction of the private attitudes. The fact that private and public situations with differing sanctions may permit initial differences in private and public attitudes toward the same objects has important implications for social change. The propagandist constantly seeks to add the sanction of the universality of opinion for the publicly expressed attitude. The social engineer may be able to employ other sanctions as well. If people can be made to express certain attitudes in public, then, they will be under pressure to bring their private views into line.

The principle of consistency also manifests itself strongly within the confines of a value system. The attitudes contained within that system will reinforce one another with respect to their cognitive, affective, and behavioral elements. The person with a well-developed ethnocentric value system will tend to attribute many undesirable qualities to all out-groups, to follow discriminatory practices toward them, and to have many beliefs justifying his evaluation and behavior. And it is more likely that there will be discrepancies between the value systems of an individual than to find discrepancies within a single value system. A real estate operator can have a consistent set of buccaneering values with respect to business practices and another internally consistent set of humanitarian values with respect to the activities of his church.

Precise prediction of direction of change in reducing the inconsistency between components of an attitude requires knowledge of more

specific motivational forces and environmental pressures. In general, however, we assume a priority of the affective and behavioral components over the cognitive components. Need gratification is tied more specifically to the affective and behavioral components than to the cognitive content of the attitude. An individual may like beefsteak and regard it as having high nutritional qualities. If he develops a stomach disorder which makes it unpalatable, he will change his ideas about it because the affective component of the attitude changes. If, however, he is told by nutritional experts that steak is not especially nourishing compared to other foods, the cognitive component of the attitude can change but the favorable affect and the positive behavioral approach toward it will not necessarily be modified. Cognition permits flexibility of symbolic representation; different combinations and permutations of symbols are possible.

Attitudes with a behavioral component readily generate a cognitive component though the reverse does not happen as frequently. After people have bought a particular make of automobile, they find all sorts of arguments to justify their preference. The beliefs can justify the preference and behavior and these ideas can also guide the individual's symbolic behavior in regard to the object. Where the individual has substitute action tendencies he can rehearse his attitude more freely on the symbolic level if he has some cognitive content to work with. If he complains about something in the absence of the object, he has to verbalize his complaints. Even attitudes which start out with mostly affective elements tend to acquire more cognitive content. "I don't like that man and I am going to find a reason" is not an uncommon phenomenon. On the other hand, there is no similar pressure to give behavioral content to the cognitive component. For most people it seems easier to develop verbal elaborations and opinions than to act out their many beliefs. The social environment, moreover, limits opportunities for action and attaches penalties to many types of deviant action. The person cannot, however, be imprisoned for his ideas as easily as for actions.

The behavioral component of the attitude is in general consistent with the overt behavior which the individual directs toward the attitudinal object. This component derives from the original behavior in the process of obtaining need gratification from or through the object. On occasion, however, the individual will not respond toward an object as one might predict from a knowledge of his action orientation. Fear of punishment, rewards given for certain forms of behavior, and similar factors may lead the person to behave in ways inconsistent with his attitude. Yet over a period of time, the individual will tend to achieve consistency between his action orientation and his expressed behavior. He may change the behavioral component of his attitude if there is a

continued situational constraint against its expression; or he may strive to avoid situations in which such constraints are present. K. Clark has shown how prejudice will be reduced when social pressures force the individual to behave in a nonprejudicial way [16].

Our emphasis has been upon the self-contained consistency of a single attitude or a single value system. Compatibility and congruence are readily found at this level. In any individual, however, we expect to find many inconsistencies among his different attitudes and among his different belief systems. These discrepancies do not generally create conflict for him unless the situation forces him to make a choice. Then he will attempt some reconciliation. The logical model of man is too simple to do justice to the complexities of social behavior in spite of its persuasive resurrection by the phenomenologists.

We have omitted, however, one important source of consistency among attitudes or among value systems—the self-concept. The self-concept is the comprehensive value structure of the personality. It accounts for a major share of the congruence among value systems. The individual sees himself as a certain kind of person and also sees himself as holding attitudes appropriate to that kind of person. The attitudes fit his role and status in life or they fit some role or status to which he aspires, as in the case of *nouveau riche*. A stable attitude of the self gives the individual a feeling of continuity and integrity. This is probably basic to Festinger's concept of a drive to evaluate the self [22]. Nonetheless, for most people the self-concept does not embrace all attitudes and values. Moreover, some people have fluctuating concepts of themselves. Individuals with fluctuating concepts of their traits and abilities are apt to be more poorly adjusted and less effective group members, as Brown-fain has demonstrated [11].

The Concept of Appropriateness

In discussing attitudes and the principle of consistency, we have concentrated upon the structure of the attitude within the individual. We believe that an adequate social psychology should take account of the world outside the person, and not in the manner of the field theorists who are concerned with environmental factors only as they are represented in the individual's own psychological life space. Just as a knowledge of the stimulus is important to the laboratory psychologist working with sensory processes, so the social psychologist must consider the social environment independently of the way the subject happens to perceive it. Despite practical difficulties of time and expense, social research ideally should have independent descriptions from a number of trained observers of the characteristics of the social situation

in which behavior occurs and about which subjects report their perceptions.

To the extent that we have knowledge of the objective situation and of the subject's attitude, we can justifiably speak of the *appropriateness* of the attitude. By appropriate, we mean the correspondence between the beliefs of the person about his world and the characteristics of the world as agreed upon by independent objective observers.

If the cognitive component of an attitude does not accord with the actual characteristics of the object of the attitude, we shall describe the attitude as inappropriate. If beliefs about the object and its relation to other events and about possible causes of action are erroneous, the attitude is also inappropriate. The concept of appropriateness parallels that of veridical perception in perception theory. Equivalent notions are common in abnormal psychology, where we speak of hallucinations and delusional systems. But the same unconscious mechanisms operate in the normal person to produce distortions of and elaborate subjective additions to the objects of the attitude.

The principle of consistency can operate to produce appropriateness in the cognitive component of the attitude. When the cognitive component of an attitude and the perceived characteristic of the object appear inconsistent, the individual will tend either to change the cognitive component or perceptually to distort the object of his attitude. Many studies have demonstrated the latter process [44, 21, 70], but few have shown that attitudinal components change to be more realistic. The reason for this imbalance probably lies in the fact that the appropriateness of a cognitive component of an attitude is a relevant consideration only if the object of the attitude can be perceived clearly and unambiguously, as will be discussed later.

Stereotypes are examples of inappropriate attitudes. The person with a stereotyped belief does not utilize the information available in the situation but defines the situation in terms of his preconceived opinions and reacts accordingly. This lack of discrimination of the objective world may produce repetitive and apparently consistent behavior. It is consistent, however, only if we center on the individual himself and neglect the relationship between his behavior and the situation to which it is directed. He does not take account both of internal consistency and of the appropriateness of behavior to the environmental requirements. Some gestalt theorists have attempted to write the notion of stereotypes out of psychology because of the implications of blind or stupid behavior [6]. They believe that a stereotyped attitude is a value judgment of the observer and that all attitudes make sense to the subject in question. We do not believe that this phenomenological question is critical, but even from the point of view of the organization of the psychological field it

is necessary to distinguish between the highly differentiated and discriminating beliefs which permit true generalization and the simple rigid structures which have been called stereotypes.

One precaution must be mentioned, however, in determining the stereotype or inappropriateness of an attitude. Appropriateness is a concept useful for dealing with attitudes directed at relatively clearly defined objects. In many social situations, however, the objects of attitudes, such as other people or groups, often have characteristics or traits which are ill-defined and ambiguous. People may behave inconsistently; a person may behave intelligently on some occasions and stupidly on others. If the actual characteristics of the object of an attitude are difficult to ascertain, it is not particularly helpful to talk about the appropriateness of the attitude, since there is no base line from which to measure the deviation of the cognitive component of the attitude. Nevertheless, if the object of the attitude has ascertainable characteristics which are highly variable over time or space, any stereotype would be inappropriate. In that case, any appropriate attitude would have to take into consideration the high degree of variance.

One source of individual striving toward appropriate attitudes is their adaptive value. In some situations, however, the maintenance of inappropriate attitudes is adaptive for the individual. One such situation is found when the members of the individual's group share inappropriate attitudes. In order to communicate with them, to be accepted by them, and to relate to them, the individual may be obliged to partake of their inappropriate attitudes. Having this inappropriate attitude thus has object-instrumental value for him. In this case, the individual may feel little pressure to make his attitude appropriate.

A typology of attitudes. The following typology of attitudes is based upon our analysis of the components of attitude structure. It also takes into account the major distinction between ego-defensive and other types of needs. There will be some overlap, therefore, with the distinctions already made among motive patterns underlying the formation of attitudes. Attitudes can thus be grouped into five types: (1) affective associations, (2) intellectualized attitudes, (3) action-oriented attitudes, (4) balanced attitudes, and (5) ego-defensive attitudes. Intellectualized and balanced attitudes are consistent with the older model of a rational man who either seeks understanding of his world or follows patterns which maximize rewards and minimize punishments. Attitudes comprising the remaining categories deal with so-called irrational behavior which requires an understanding of the individual's own internal logic. These types of attitudes are similar to the empirical categories described by Bettelheim and Janowitz in their *Dynamics of Prejudice* when they distinguish among (1) the intensely anti-Semitic

veteran (action-oriented), (2) the outspokenly anti-Semitic veteran (the affectively oriented), and (3) the stereotyped anti-Semitic veteran (the cognitively oriented) [10]. We have elaborated our types more fully and with more attention to their theoretical nature and basis.

Affective associations. These attitudes have minimal cognitive content and little or no action orientation. Thus, it is not possible to predict from such attitudes to the individual's behavior toward the object itself. In addition, this sort of attitude is not related to the individual's cognitive structure. It stands alone and isolated from the person's belief systems. It is an evaluation based heavily upon affect arising from the object itself.

The major source of affective attitudes is past association of the object with need satisfaction. This association is not instrumental but simply contiguous in time or place. Since the object is not instrumental, its association with need satisfaction is fortuitous. Hence one cannot predict a person's affective associations from his present motivations for they stand apart from his present motivational structure.

As this type of attitude is essentially affective, it is logical to expect that another of its sources should lie in the value system of the individual. The object of the attitude might be only an instance of the class of objects of the value system. But since value systems are organized through the interrelationship of the cognitive components of attitudes, affective associations would tend to remain unintegrated into larger systems.

Affective associations lack a behavioral component because they are not intrinsic to the satisfaction of needs. To achieve such satisfaction the individual does not have to do anything to the object except express his affectivity.

Since affective associations have so few cognitive elements with which contact can be made, they are difficult to change through information or verbal communication. New affective associations are generally necessary if the old attitude is to be modified.

Intellectualized attitudes. Many attitudes have a heavy cognitive component, in addition to their evaluative core, though they lack behavioral structure. Consequently they cannot be used very reliably for predicting behavior toward the object of the attitude.

The major motivation for this type of attitude has already been discussed. Beliefs about the object can satisfy a strong need within the person, the need to understand the world about him realistically and coherently. The existence of such a need has been shown by a number of workers on curiosity needs, exploratory drives, cognitive stress, etc. The individual's beliefs about the object can serve to satisfy this need; hence, the individual develops a high evaluation of the object. Furthermore, as previously indicated, the individual will have low evaluations

of the object if his beliefs about it are confused and incoherent, or if he finds his beliefs inconsistent with his percepts of the object. When the evaluation of attitudes based on this need to know is high, one might expect the beliefs to be appropriate and highly differentiated.

Such attitudes tend also to be integrated with the cognitive systems of the individual. The need for a coherent view of the world evokes effort to encompass many objects and classes of objects in a coherent scheme, provided that facts offer some basis for such integration. The heavy cognitive content of these attitudes also makes possible many areas of connectedness so that they can be organized into a complex value system.

These attitudes are susceptible to change through new percepts, since they tend toward appropriateness. Since they tend toward integration, intellectualized attitudes are also susceptible to influence through changes in the cognitive structure. Should these two sources of change clash, as they may, the individual is faced with serious conflict.

Intellectualized attitudes may arise not only from the need to understand the world but from the need for self-consistency or some other ego-instrumental need. Attitudes derived in this fashion might tend to be inappropriate yet have some degree of differentiation.

Action-oriented attitudes. People can satisfy their needs and develop action tendencies toward valued objects with a minimum of cognitive representation. This occurs when the need can be satisfied simply and directly. Before the advent of Freudian psychology, it was assumed that needs associated with cleanliness and body functions led to attitudes without cognitive structure. In any event postponement, blocking, and indirection in the satisfaction of needs does lead to cognitive activity and cognitive structure and so reduces the number of action-oriented attitudes. On the other hand, the fact that social structure provides ready-made channels for the satisfaction of many needs for many people makes possible action-oriented attitudes. So long as there is little conflict, people may accept the established pathways to their goals with a minimum of intellectualization. Surveys which investigate national samples and explore the attitudes of people outside the academic world find a paucity of beliefs about many problems. Nevertheless, there often are evaluations and action tendencies toward social objects though the cognitive exploration yields poor returns.

Action-oriented attitudes, moreover, can have a substitute activity function rather than an orientation toward social action. They give tension release if they are not in the service of an appetitive drive. Frequently such attitudes are difficult to change because of the private nature of their rewards for the individual. Other action-oriented attitudes which are more socially directed can be changed through the

development of new sources of need satisfaction, through changes in the need-satisfying qualities of the object, and through finding new paths toward the same goals.

Balanced attitudes. Many attitudes in the service of biological and ego needs are fully developed, with elaborated cognitive content and action orientation to supplement the affective core. These are the attitudes which have often been assumed to be the dominant orientations of people in the economic and political realms. And in fact the platforms of political parties are often directed at such dispositions which are supposed to characterize various interest groups or blocks of voters.

These attitudes have their source for the most part in trial-and-error learning in achieving motive satisfactions. The process of learning is complex enough to involve ideational processes, and the resulting beliefs help identify pathways to the goal. Beliefs also develop to justify the course of action. The behavioral component is necessarily built in since the rewards are directly related to the activities pursued, so that the arousal of the need reinstates both memories of the goal and an impulsion toward specific action.

The belief component can be further enriched through cognitive structures related to any aspect of this particular pattern of motive satisfaction, including the attitudinal object. The process may be more complex in that the evaluation of the object can be related to a specific value system which, in turn, is connected with some cognitive structure.

Attitudes of this type tend to be differentiated in cognitive structure, especially if the means-goal relationship exhibits complexity. Moreover, they are generally not compartmentalized because the individual is trying to maximize their satisfaction and not to protect himself against the operation of the motive.

Attitudes in the category under discussion permit predictions of collective behavior. People are not only consistent in their patterns of need satisfaction but they are sufficiently alike in a cultural setting so that, given knowledge of rewards and punishments in a situation, fairly good predictions can be made. Balanced attitudes can be changed through manipulating the external punishments and rewards, the pathways for avoiding and approaching these goals, and the perceptions of such pathways.

Ego-defensive attitudes. These attitudes also have all three components of affectivity, cognition, and action tendency in fair measure, but they differ from balanced attitudes in the nature of the motivational processes to which they are related. Balanced attitudes function in the interests of more consciously recognized and acceptable needs, such as physiological drives or the ego needs for affiliation, achievement, expression, or self-determination. Ego-defensive needs arise from internal

conflict and the resulting behavior is directed at an object which cannot be an instrumental means for resolving the conflict.

There is a very tight relationship between the content of the attitude and its motivational source in the ego-defensive attitude. The person who expresses hostility toward those he regards as the aggressive, inferior members of a minority group, and thus obtains some temporary relief from his own inner conflicts, cannot readily meet his problem by attitudes of acceptance and cooperation. There is little flexibility in the types of attitudes he can hold in the service of this need. We cannot necessarily say in advance whether this motivational pattern will be directed more intensely against one minority group than another, but we know that the probabilities are high that, outside the majority group, it will find a scapegoat which permits the safe expression of hostility.

The relationship between personality type and social attitudes is an old problem in social psychology. Some of the earliest work on attitude research started with the premise that social attitudes were closely tied to personality needs and conflicts [3]. Early attempts to show the defensive nature of radicalism were not productive because they did not choose an area in which there was any necessary connection between a deep-lying need of the individual and attitudinal expression.

Workers in the field of authoritarianism, on the other hand, selected a motivational pattern and its resultant attitudes where the nature of the motive permits little flexibility in expression [1]. Though they have overgeneralized their findings and are dealing with only one type of attitude, their contribution is still of major proportions. Yet though we can predict attitudes of prejudice toward minority groups from the personality syndrome of repression and projectivity, we cannot predict from the attitude to its motivational source with the same degree of success. Expressions of hostility toward minority groups may have other causes than repression and projectivity. Sufficient allowance for the one-way directionality of prediction between motive and attitude is necessary in personality theories of attitude determination.

The cognitive content of ego-defensive attitudes can be partly changed by external pressures; i.e., the individual may be taught not to express his hostility against one type of scapegoat. But since the basic motivation remains, he may seek a new type of scapegoat. Fundamental change in ego-defensive attitudes calls for some degree of personality change through the development of self-insight by the individual.

Behavior and the Expression of Attitudes

Researchers in the area of attitudes have often been disturbed and discouraged by their inability to predict the behavior of an individual from a knowledge of his verbalized expression. The public opinion

pollsters with many years of experience predicting election results still cannot predict which of their respondents will actually vote on election day. It is necessary to identify several sources of difficulty in dealing with the relationships between attitudes and behavior. First, insufficient attention has been given to the differences among types of attitude. Attitudes which have little or no action orientation are not necessarily good predictors of behavior. Often the investigator assumes that the individual who expresses an evaluation of an object is also committing himself to a corresponding form of behavior toward it.

Secondly, failure to inquire carefully into action orientations in the measurement of attitudes can lead to incorrect inferences about the predisposition to behave in certain ways. The subject may have an action orientation but it may not be the one the investigator assumes, since the investigator is basing his judgment on the subject's expressed liking or disliking of some object.

Examples of this mistake in the identification of the presence or absence of action structure in an attitude, and the precise nature of such a structure where it does exist, can be found in many practical situations. Social and industrial organizations expend considerable effort to create favorable evaluations of the group and its goals but give little attention to the action orientation of the attitude. Katz and Kahn have shown the absence of high positive relationship between morale and productivity in industrial organizations and have suggested two reasons for this finding [33]. (*a*) Some workers who like the company and their jobs may have no action orientation accompanying this favorable evaluation. (*b*) Other workers have an action orientation but it is directed solely at staying within the system. Accordingly, they may work hard enough to avoid being fired but not as hard as their abilities would permit. Even where favorable attitudes toward the system exist, membership in a group or social system does not give sufficient evidence to predict behavior beyond minimal role requirements. Moreover, there may be more than one set of group standards within a social system [60].

Thirdly, predicting behavior from a knowledge of single attitudes is difficult because the same object may be tied to more than one attitude. For example, a real estate agent may have an unfavorable attitude, with an action orientation of avoidance, toward members of minority groups. He also has a positive attitude toward clients who are in the market for new homes. He may, then, encounter a minority group member who wants to buy a new house. The choice of the attitude which is expressed is a function of (1) the strength of the two attitudes, (2) the strength of the present motivational forces, and (3) the context in which the object of the attitude is perceived. The strength of the attitude refers to its degree of affectivity as this relates to its action orientation. The

strength of present motivational forces refers to the needs which happen to be uppermost in the person at the given time. The real estate salesman who has not sold a house in weeks is in a motivational state in which the attitude toward a client is more easily elicited. The context for the perception of an attitude would include the environmental situation as well as the existing cognitive frame of reference. The minority group member may appear in the real estate office with influential friends from the majority group and so may be more easily perceived as a client. Immediately preceding experience may have set up an expectation for certain types of objects and thereby may determine how a given object will be perceived.

Fourthly, difficulties in predicting to behavior from attitudes arise from the distinction between the object and its symbol. The object and its symbol may be related in any one of several ways. An individual may not distinguish between the object and its symbol; in this instance, the prediction to behavior is made easier. The individual reacts in the same manner to both object and symbol, as in the case of the person who states on the Bogardus social-distance scale that he will not admit Negroes to his club as members and acts accordingly. Or the man grumbling to himself or his family may be rehearsing the sort of activity that may later be directed toward the object itself. On the other hand, the individual may distinguish sharply between the object and its symbol, and the attitudes toward the two may not be identical. In many cases, moreover, social objects are not readily available as attitudinal targets, or if they are available, behavior directed toward them requires efforts in a social world which already overtaxes the energy of the individual. Furthermore, in some instances, expressed behavior toward the social object may lead to some risk of punishment. It is also true that people of a quietistic frame of mind may consistently avoid involvement in the arena of practical problems, whereas the fringeline type of person is consistently action-oriented.

The expression of an attitude may be a substitute activity which gives some release and may actually mean less likelihood of subsequent action against the object itself. Or, an individual may react to the symbol in a manner consistent with his ego ideal in order to protect himself against the devastating effects of his full awareness of his behavior toward the actual object. The situations in which he reacts to symbols and to the object may be so different as to elicit entirely different motivational systems, and therefore, different attitudes.

The individual may, however, express his attitude toward the symbol of an object in order indirectly to affect the object. The individual may communicate his feelings to other people so that they may be more favorably or unfavorably disposed to the object. These people as a

group may be powerful enough to behave to the object in ways that the individual feels helpless to undertake.

Assumptions about Attitude Change

We have examined the nature of attitudes from three points of view: (1) their structural components and characteristics, (2) their relationship to the more comprehensive structures of value and belief systems, and (3) their functional relationship to motive patterns. In this process a number of assumptions about attitude change were suggested. We should now like to make these assumptions explicit and to add further assumptions that follow logically from the characteristics and functions of attitudes which we have described.

1. The most basic assumption is that the key factors in attitude change are not the situational forces or the amount and types of information to which the individual is exposed but the relation of these factors to the individual's motive patterns. The following assumptions are either supplementary to this first assumption or detailed elaborations of it.

2. Major motive patterns may be ranked in terms of their relative urgency: (*a*) biological or appetitive drives, (*b*) ego motives (social drives) and ego defense, (*c*) curiosity, the need to understand. As drives at one level in this order are satiated, drives at the next level become all-important. In prisoner of war camps where hunger begins to operate as a drive, the more complex motives lose importance. The culture of the camp becomes a food culture, and self-preservation in its most elementary forms becomes manifest. The need for understanding and knowledge comes into full play only when more basic motives have been satisfied. A well-constructed ideology can crumble overnight if more basic patterns which it assumes are frustrated.

3. The principle of consistency is almost always operative, but the direction it takes and the devices used to achieve consistency are subject to definite limitations. Thus, individuals can reduce inconsistency by giving up one of two opposed sources of need gratification. The common solution, however, will be to keep both incompatible desires and to resort to compartmentalization and rationalization of single attitudes to avoid the inconsistency.

People will attempt to maintain both discriminatory practices and democratic attitudes by rationalizing the discrimination. In one study of this problem in college fraternities, students' reasons for barring certain people from their fraternities were examined [32]. Of the five reasons advanced, two were frank statements of personal objections to the excluded groups, two concerned fear of public opinion, and one included both personal objection and fear of public opinion. The majority of the students, however, did not espouse the first two reasons

and professed no personal objection in spite of the fact that the questionnaire was anonymous. If these results are taken at face value, we find that the majority fear public opinion although they themselves are the major portion of the public. Possibly, a condition of pluralistic ignorance did prevail but it is also possible that individuals rationalized their own objections as the common prejudice of others.

4. Where the inconsistency does not lie deep enough to require repression and compartmentalization, the individual will try to resolve it by compromise rather than by rejecting one of the logically opposed alternatives.

In the interesting experiment of Janis, Lumsdaine, and Gladstone, the effects of a preparatory communication upon reactions to a subsequent event were tested [31]. The communication was a statement about the poor prospects Russia had of developing atomic weapons; this information was later contradicted by the news that Russia had exploded an atomic bomb. The experimenters wanted to find out which of two theories would better account for the reaction of subjects to the information that Russia had the atomic bomb. One hypothesis predicted overcompensation, or a boomerang effect of the contradictory news, so that people would reject completely the information from the early communication. The other hypothesis predicted a positive effect: the early communication would prevent complete acceptance of the meaning of the actual event. The results confirmed the second hypothesis. After President Truman's announcement that Russia had exploded an atom bomb, the experimental subjects who had received the earlier optimistic communication showed greater resistance to the impact of this pessimistic event than did the control subjects who had not received the earlier information. Apparently, people did not accept the literal logical meaning of the two opposed sets of information but found a compromise position between them.

The boomerang hypothesis does work on occasion, and we do reject one source of information completely, but the academician tends to give the boomerang hypothesis greater weight than it possesses. The advertiser and the propagandist would not be flourishing today if the boomerang effects of false claims were the rule rather than the exception.

Another experiment which shows the tendency toward cognitive compromise is Stouffer's analysis of conflicting social norms [64]. Students were presented with a hypothetical situation in which a proctor found his roommate cheating on an examination. They were asked to indicate what course of conduct by the proctor would be approved by the other students. They also were asked to indicate the actions the authorities would approve in the same situation. The over-all percentages showed a conflicting emphasis of the expectations of authorities and of

students, but the largest single group of students was able to find some compromise by indicating at least one action in this situation which would be approved by both authorities and students. Stouffer concludes that a social norm is not a point but a band of permissible behavior which allows for some slippage from the logically prescribed role.

5. Attitudes which we have termed affective, action-oriented, intellectualized, and balanced can all be changed through gaining control of the individual's behavior toward the attitudinal object. Organized groups characteristically emphasize the control of behavior and make certain actions an essential requirement for all group members. The priority of cognitive and perceptual factors in modern psychological theory has obscured the importance of required behavior as a determinant of the individual's beliefs. Role playing, though weaker as an influence than the assumption of a role in social life, is effective partly because it calls for behavioral change. Culbertson found that more favorable attitudes toward Negroes resulted from the assignment of roles in a role-playing situation in which subjects worked out problems of Negro housing [20]. Similarly Janis and King found that the task of making speeches led to attitude changes which conformed to the position outlined in the speech [30].

In a more natural setting, Harding and Hogrefe showed that white department-store clerks who had to work with Negroes tended to be more favorable to this association after the fact [23]. One of the most frequent and revealing reasons given by the clerks for their changed attitudes was simply that they *had* to work with Negroes. Thus they made their attitudes consistent with their behavior.

Attitudinal change resulting from role change was dramatically demonstrated in the Lieberman study of workers promoted to positions of foremen and elected to the office of union steward [45]. Both foremen and stewards had been included in a survey of all rank-and-file workers before their promotions to these roles. In this first measurement the foremen-to-be and the stewards-to-be were more critical of the company than the other workers, but the former two groups resembled each other in attitudes and personal qualities. After a year in their new roles, however, they differed strikingly in their attitudes. The new foremen had taken over management ideology; the new stewards had taken over union ideology.

6. Attitudes of the intellectualized and balanced type can be changed through a change of the value system in which they are integrated. The object of the attitude is but one instance of the general class of objects to which the belief and value system refers and thus the attitude tends to be made consistent with the larger structure. If this larger structure or value system can be changed, the individual will tend to shift his

attitude accordingly. Changes in value systems may occur through changes in the component attitudes over time or through a modification of the generalized values brought about by some radical experience in a drastically changed environment. A dramatic illustration of this second type of change is illustrated in the experiment of Ashley, Harper, and Runyon in a replication of the Bruner-Goodman study on the relation between need and the estimation of the size of coins [7]. The experimenters hypnotized their subjects after first obtaining their judgments of the size of coins. While in the trance state, some subjects were given the suggestion that they were very poor and other subjects that they were very rich. The estimates that were then made of the coins shifted upward for the subjects who had been told they were poor and downward for those who had been told they were rich. Thus the change in the value system involving the self-concept (whether he saw himself as wealthy or poor) led to a change in the cognitive components of his attitude toward monetary objects.

7. Intellectualized and balanced attitudes can be changed through modifying the cognitive component of the attitude. Though motives and behavior are assumed to be more significant determinants of psychological functioning than beliefs, the cognitive part of the attitude has some importance in its own right. It defines the object of the attitude; beliefs about the object though they may be rationalizations are nonetheless helpful in guiding behavior. Individuals characteristically seek rationalizations and often must find them before they act or before they feel comfortable about their actions. Timing is important since the ingenuity of the individual is limited and finding the proper rationalization takes time. With a long time span, as in an election campaign, the individual can find the rationalizations he needs to justify his voting behavior. This is one reason why, in spite of fluctuations during the campaign, the overwhelming majority of people show the same political preferences in November that they showed in June.

The role of beliefs in behavior has been clearly demonstrated by Raven's work on pressures on deviates to conform to a group norm [55]. Groups of subjects read a juvenile delinquency case and gave their opinion about the extent to which the delinquent was personally responsible for his crimes. Most subjects took a favorable position toward the delinquent. A false consensus of group norm was then reported to subjects. They were asked to restate their evaluations and then to write a description of the delinquent. At a later point they were asked to give their evaluations of the case once more. The subjects who shifted toward the unfavorable group norm were those who gave more unfavorable descriptions of the delinquent. In other words, the subjects who felt pressure to conform to the group norm did not do so until they had devel-

oped beliefs about the case which supported their shift toward the norm's attitude. A related finding occurred in Crockett's study of the use of group norms in producing change [19]. When subjects were given new group norms with reasons for the group position, they changed significantly more than subjects who were presented with only the group norms.

8. Attitudes can be changed through a modification of their affectivity as a result of emotional conditioning. The discussion of affective associations referred to the fact that to be heavily laden with feeling an object did not need to have an instrumental value for the person. The Razran study has been cited as an example of how such associations can occur between satisfying a hunger need and positive evaluations of associated pictures or musical selections [56]. Another instance is Murray's demonstration that frightening children leads them to make low evaluations of faces presented to them at the time of their fear [49].

A more complex case of the influence of associating fear with objects is shown by the work of Janis and Feshbach on the influence of fear-arousing communications upon the degree of conformity to the objectives of the communication [29]. Three groups of subjects were informed about the consequences of failure to practice proper dental hygiene. The first group was shown pictorial material of a frightening character; the second group saw less dramatic material; the third group received similar information but of a nonemotional character. The three groups were given the same instructions about care of the teeth. The follow-up study indicated that the group subjected to the minimal fear appeal had the most members observing the instructions. The group exposed to the maximum fear appeal had fewest members following the suggestions about good dental care. Apparently the emotional arousal was so strong that it colored the situation and led to an avoidance of the problem. Instead of the negative feelings becoming attached to improper practices with respect to tooth decay, the unfavorable affect spread to the proper practices. Thus, in order to predict the effectiveness of an emotional appeal, it is necessary to know whether the affect can be narrowed to the appropriate object. When punished severely, children may react negatively to the punishing parent rather than to the forbidden activity. Factors which determine the outcome are the clearness of the pattern of the desired activity, the degree of emotional arousal, and the other associations with the emotional source.

9. Intellectualized and balanced attitudes can be changed if the instrumentality of their objects for achieving the individual's goal can be changed. The low evaluation of an object can be shifted upward if the object can now be seen as a means for attaining some desirable end. This is not a matter of developing new needs or values but rather of estab-

lishing new connections between behavior and existing needs. The baseball magnate who previously has had discriminatory attitudes toward Negroes can be made to see them as desirable people since they can help him win pennants and draw crowds to the ball park. Carlson has been successful in changing attitudes toward a minority group by making his subjects aware of motivational consequences which they had not previously related to favorable behavior toward this group [12].

Experimental attempts to compare emotional with logical appeals have not taken sufficient account of the instrumentality character of the appeals employed. Hartmann reported that leaflets with an emotional appeal were more effective than leaflets with a logical appeal in a political campaign [24]. In his emotional appeal, however, the instrumental value of voting in the advocated way was clearly stated. In the logical appeal, this instrumental value was not so obviously expounded. Thus, part of the greater efficacy of the emotional appeal may have been the result of the exposition of the instrumental value of the attitude rather than of its "emotionality." The failure of Knowler and others to find any differences between an emotional and nonemotional appeal may be owing to a lack of difference in the degree of instrumentality of the appeals which were used [37, 38].

Making an object instrumental for some goal or value of the individual generally requires some consideration of the frame of reference in which the object is perceived. The frame of reference consists of the standards of judgment at any point in time and may reflect past learning or events just prior to the present, as well as the immediate situational forces. Apparent inconsistency in behavior may result from the same object being judged at various times in different frames of reference and therefore having different attitudes directed toward it. R. T. LaPiere found that restaurant and hotel owners accommodated a Chinese couple when they were approached by the couple in person [42]. Nevertheless, they responded to a questionnaire by the flat statement that they would not accept Chinese. The same discrepancy is reported by Kutner, Wilkins, and Yarrow who found restaurant owners admitting Negroes but refusing to make reservations for them [41]. The written commitment may be of some importance here but it also is probable that a customer seeking food or shelter, even though of a different shade of skin color, is perceived in a different frame than a written request from a Negro.

To change the instrumentality of the object it is necessary to elicit a frame of reference involving a positive and fairly powerful value system. When this value system is salient in the individual's thinking, the old attitudinal object can be introduced and its usefulness for the individual's goal considered. Effort to change a person with a negative evaluation of an object is peculiarly difficult, since mere mention of the object calls

forth the negative attitude and interferes with further consideration of the matter. A frame of reference not involving the object directly, however, can be employed. Within this changed focus, the touchy object can be introduced and connected with some important value of the person.

10. The lasting effects of attempts to change intellectualized and balanced attitudes are related to the figural or background character of the various factors in the situation productive of change. Memory operates differentially with respect to information received, with greater retention and effect of figural than of background items. *The need to know* works through the imperfect mechanism of retentive assimilation. Central elements from a communication may be recalled and may affect attitudes much more than peripheral items concerning the source of the information, the time and the place, the people present, etc. We remember events but not their exact dates; we can quote lines but we cannot cite their chapter and verse; we remember ideas but not their source. Hence information which is received and discounted at the time, because of the attendant source and circumstances, will later have its effect upon the individual. This fact has been exploited by propagandists with the technique of the big lie, the repeated lie, or the whispering campaign.

A number of experimental findings can be explained on this basis. Hovland and Weiss presented the same communications to matched groups of subjects [28]. In the one instance, the communication came from a highly credible source; in the other case, from an untrustworthy source. The immediate effects upon attitudes were in the expected direction, but four weeks later the results from both sources were about equal. The sources of the communication had been forgotten, as far as any appreciable effect upon the message was concerned. Kelman in another experiment reinstated the sources in an otherwise replicated procedure and with the reinstatement came the original effect [36].

The "sleeper effect" in which the influence of the communication is greater over time than shortly after the reception was reported by Hovland, Lumsdaine, and Sheffield [27]. Short-time and long-time effects of a film "The Battle of Britain" designed to strengthen confidence in America's ally, Britain, were measured in two matched groups of American soldiers. The group tested immediately after the film showed greater memory for factual content of the film, but the group tested after nine weeks showed a greater change in favorable attitudes toward Britain's role in the war. Thus when the situational pressures against accepting the film were removed or forgotten, the changed beliefs about the object produced changed evaluations of it.

One of the hypotheses advanced by the experimenters is the same as the assumption we have made, "forgetting is the rule but the *source* of

an item of information is more quickly forgotten than the material presented" [27, p. 197]. Collier has reported interesting results from students who were asked to read Nazi propaganda [18]. In spite of instructions, they came through the task with more favorable attitudes toward the Nazis than were held by other students. Again, the effect of contextual factors did not prevent the information absorbed from the documents from having an influence.

11. Ego-defensive attitudes will be relatively susceptible to change through procedures designed to give self-insight and will be resistant to change through procedures employing information and action. Our major thesis has been that since attitudes serve different needs and functions, they can be changed only through relating the change-procedure to the appropriate motive pattern. In general this calls for separating subjects on the basis of their needs and values to begin with and making differential predictions for various change methods. Thus far the greater bulk of the research on attitude change has started with the attitude itself and has assumed a common motive pattern for all people. Our own method, as shown by the following experiments, is to begin with measures of ego-defensiveness as one of the major sources of attitudes toward minority groups and to gear influences directed at change to the anticipated motive patterns.

Subjects high in ego-defensiveness as measured by projective tests were not significantly influenced by information and attempts at cognitive restructuring in the experiment of Katz, Sarnoff, and McClintock [34]. Unfavorable attitudes toward Negroes both with respect to cognitive and behavioral components were not appreciably affected by new information. These findings were replicated in a follow-up study employing a different population. Self-insight procedures were also employed in these studies on the assumption that the people in the middle ranges of ego-defensiveness would be most responsive to the influence. High ego defenders, it was assumed, could be affected only by fairly intensive therapy. McClintock has shown that the insight procedure, employing a case study illustrative of the dynamics of repression and projectivity, did in fact produce the anticipated changes [46]. The same type of defensive people who had resisted the information approach now became more favorable toward Negroes. McClintock further demonstrated that conformity pressures had little influence upon ego defenders but did successfully change people who possessed a high degree of the need to conform.

The success of Culbertson's role-playing experiment in changing attitudes toward Negroes has been mentioned but it is significant that her important changes occurred among her subjects scoring low in the F scale [20]. The high ego defenders showed little change as a result of role

playing. Finally, it should be added that Wagman found that people low in ego-defensiveness resisted authoritarian suggestion directed at changing their attitudes but responded positively to information [68]. The people high in ego-defensiveness resisted the information but were influenced by authoritarian suggestion toward more favorable or unfavorable attitudes toward Negroes.

SUMMARY

The concept of attitude is useful in social psychology if it is not stretched to cover all aspects of psychological functioning and if it is also given specifications within the area to which it refers. On the first count, we have limited attitudes to evaluations of objects and have ruled out beliefs which are not colored by affect and affective processes which are not tied to cognitive elements. On the second count, we have described characteristics of attitudes and their relation to motivational processes and have suggested a typology of attitudes which takes into account both structural and functional aspects. This analysis may be helpful in dealing with problems of attitude formation and change. Not all types of attitudes follow the same pattern of dynamics either in genesis or in subsequent modification. Hence a procedure which will be successful in changing one type of attitude may be completely ineffective in attempting to change another attitude.

The interactional nature of the factors in social experience makes it possible for change to be introduced with varying effectiveness into any part of the psychological system of which the attitude is a part. If there is a change in the person's needs, in his beliefs, in his values, in his perceptions, or in his behavior, there can be modifications of his attitudes.

At one time or another, social scientists have emphasized a single factor as the major determinant of attitudes. Some emphasized the social environment of the individual and made ecology central in their explanation. A related theory made behavior the important variable. People become aware of their roles after they have played them. The modern emphasis is upon the individual's own perception, upon his "definition of the situation." We believe that emphasizing one set of factors produces a model inadequate for dealing with social attitudes. The phenomenological approach does not enable us to predict a fairly common occurrence in social life, namely, that attitudes develop to justify behavior which is imposed upon the individual. Slaves do not generally hug their chains, but within a wide range of limiting conditions, people accept their prescribed roles and evaluate them favorably. The technique of the *fait accompli* is certainly as old and as common as the technique of trial balloons.

In addition to describing characteristics of attitudes, we believe it is important to include concepts which deal with the relation of attitude to personality and the relation of attitude to the objective social world. Hence, we have spoken of the isolation and connectedness of attitudes to value systems and of the instrumental function of attitudes for the protection of the self-image and for ego defense. We have also suggested the notion of the appropriateness of the attitude to the object to which it refers. Appropriateness resembles the concept of veridical perception.

Though it is possible to enter the psychological system at the point of need, belief, or behavior, not all attitudes are equally susceptible to influences directed at beliefs or behavior. Ego-defensive attitudes are particularly resistant to environmental forces which exert direct pressure to create change. In this instance, we are dealing with a need within the individual which is sufficiently complex in origin to make personality change the necessary condition for modification of the attitude.

The implications of this analysis for research are twofold. The assessment of attitudes should include more than the measurement of affectivity and evaluation. It should also include measures of the belief component, the behavioral component, and the linkage of the attitude to its value system. Moreover, research on attitudes should assess the motivational basis of the attitude. Secondly, in research on attitude change, the procedures to produce change should be designed to affect a specified factor or factors which previous assessment has suggested as particularly significant. The change procedure used should represent the manipulation of a known factor of some degree of generality so that general statements are possible about the effect of a given variable upon certain types of psychological functioning. Finally, research on attitudes should emphasize experiments on attitude change, since change is critical for understanding any phenomenon.

THE RELATION OF THE PRESENT FORMULATION TO THE ORGANIZATIONAL PLAN OF THIS VOLUME

The foregoing attempt at an initial statement of a theory of attitude structure and change has not followed the rubrics suggested for theory presentation. We think that in this area we are not yet far enough along in the measurement of variables or in systematic conceptualization to meet the formal demands of many of these categories. To have so stated our definitions, distinctions, and assumptions might have led to inference that we are attempting a more fully developed theoretical model than is the case. On the other hand, it may be helpful to point out the instances in which the conventional rubrics could be profitably used in the understanding and possible further elaboration of our material.

Background Factors and Orienting Attitudes

We would make two observations concerning background factors which have influenced our thinking. One is the remarkable durability of the concept of attitudes in social psychology. Most theories concerned with the social nature of man employ the concept of attitude and often use the term itself, even though there is little necessary relation between the formal requirements of the theory and the construct of attitudes. This suggests that an adequate social psychology must include the concept of attitude or some very similar construct and that an examination of the problems in this area is of crucial importance to progress in our field. We feel that attempts at predicting and understanding behavior in the social world offer confirmation of this historical conclusion. Efforts to deal with the real world show our need for a concept more flexible and more covert than habit, more specifically oriented to social objects than personality traits, less global than value systems, more directive than beliefs, and more ideational than motive pattern. Recently there has been progress along the major fronts of cognition and motivation. We believe that the study of attitudes is the means most likely to link these two lines of development.

A second background factor is our impression of the slow progress of cumulative knowledge in social psychology. Terminology and limited theories have multiplied, to be sure, and the literature has grown in mushroom fashion. Yet the advance of knowledge has been discouragingly slow. We believe, therefore, that something may be gained by pursuing an old problem and examining it thoroughly before pushing into areas which seem new but which may be novel only in fresh terms and labels. The strategy of skimming off the cream and moving on needs to be supplemented by thorough attention to long-standing problems.

Our major orienting attitudes can be summarized as follows:

1. The prediction of behavior is the major goal of social psychology. We see very limited value in highly general theories which can explain after the fact but contain no specifications for the prediction of social events. We do not mean that the scientist should attempt to predict the phenotypical event in its uniqueness. But reaction against the phenotypic can become an excuse to avoid critical predictions.

2. Psychological prediction must concern itself more with major variance in social behavior. We must walk before we can run, to be sure, but there are disadvantages in the complete absorption in any difference which is statistically significant even though it may account for a very small fraction of the total variance. One consequence is a failure to confirm findings when studies are replicated. Another is the development

of a social psychology limited to certain restricted aspects of classroom situations.

3. Models should be helpful in the development of any science, but formal mathematical models have thus far made very limited contributions to the progress of social psychology. One reason may be that model building becomes an end in itself. The tendency is to forget what is already known and to start with such oversimplified schemes that they generate no meaningful predictions for social events. Another reason may lie in a misconception of the role of models. They can help in the systematization and logical ordering of knowledge and in derivations which might otherwise be neglected. But they do not in themselves give us major theoretical insights or lead to significant discoveries. Models should serve a substantive theory rather than replace theorizing about the nature of psychological processes. The models frequently used are borrowed from other fields and so do not take the place of concepts derived from a direct study of the phenomena of our own field. Part of the vitality of Freudian concepts stems from the fact that they grew out of observation and speculation about human behavior and were not attempts to borrow from physics or physiology.

The limited type of model now popular in psychological theorizing is adapted to laboratory experimentation where a problem is narrowed down to a few variables which permit fairly precise measurement. But social psychology requires a model more appropriate to field studies. Most of the other sciences have grown because of their ability to produce within the laboratory powerful manipulations which are called for by their theories. Social psychology is handicapped in this respect because, for the most part, it can create only relatively weak variables within the confines of the laboratory. The laboratory approach should be utilized as fully as possible, but we will make merely limited progress until we can study the powerful forces which affect people in the real social world. The systematic study of social change calls for models appropriate to this level of investigation and such appropriate models are not now available. F. H. Allport's event system theory of behavior is a promising development in this direction.

4. Besides rigorous hypothetico-deductive axiomatization, currently, social psychology needs some intermediate level of systematic concepts which have particular relevance for the prediction of social behavior. We need hunches about the significant variables and combination of variables which produce movement or change in the social world. Weak as were the formulations of Emile Durkheim, Max Weber, Karl Marx, and Sigmund Freud from the standpoint of formal scientific theory, we still lack a systematic set of concepts as content-oriented and as useful as

those provided by these early writers. Small wonder that when we have to deal with social reality, we revive concepts of *anomie* from Durkheim, legitimacy, charisma, and bureaucratic structure from Weber, and production relations, power, and group conflict from Marx.

This is not to take the position of the man in the street, who wants science to give him immediate answers to all practical problems. We do hold, however, that a major referent of our science is the ongoing stream of social events and that our major need is some system of constructs which would enable us to move in and measure these ongoing forces. Social psychology would be better off with a fairly loose system of concepts, such as those provided by Freud for a study of personality, than with a rigorous formal system which will not permit coming to grips with social realities nor give any basis for predicting social occurrences.

Structure of the system: independent, intervening, and dependent variables. The variables we have discussed form an interdependent system which can be entered at any point. The causal sequence can flow in either direction between two variables and lead to circular reinforcement. A form of behavior imposed by environmental forces can lead to a set of beliefs and these beliefs can, in turn, result in the behavior in question. We do assume, however, some priority of variables in the determination of behavior. Though beliefs can modify needs and can affect their means of satisfaction, we would in general regard motives and environmental forces as independent variables, attitudes as intervening variables, and their expression in behavior as the dependent variable. We shall attempt to apply this ordering to the following motive patterns basic to attitude formation:

Ego-defensive attitudes toward out-groups. In line with the theorizing of Frenkel-Brunswick and her associates, we regard the defense mechanisms of repression and projectivity, in combination, as the independent variables leading to the intervening variable of an ego-defensive attitude, with the dependent variable being prejudiced behavior which could assume the form of (1) discriminating and aggressive acts toward out-groups, (2) negative stereotypes of out-groups, and (3) expressions of negative affect toward out-groups. All three forms of behavior would be predicted for this type of ego-defensive attitude. The independent variable could be measured by TAT protocols or the Michigan Sentence Completion Test, the MMPI, the Blacky test, and certain portions of the F scale. The dependent variable could be measured by observation of behavior in natural settings or by stereotype and attitude scores on questionnaires or in interviews. But not all prejudiced behavior would derive from ego-defensive attitudes. Moreover, the ego-defensive attitude would always be accompanied by fairly intense feelings of hostility either in the expression of negative stereotypes or of discriminating acts. In

other words, the behavior could properly be considered aggressive. Discriminatory behavior and the expression of negative stereotypes which do not involve high affect would have other origins and functions than ego-defensiveness. They could be object-instrumental or ego-instrumental (see discussion below).

The ego-defensive attitude of conformity. Another pattern of independent variables would combine the defense mechanism of conformity and the existence of clear social norms of the groups in which the individual moves. Again, a type of ego-defensive attitude is postulated as the intervening variable, and the dependent variable becomes the expression of this attitude in the approval of the practices and beliefs sanctioned by group norms. In this case the pattern of independent variables would be measured as follows: the defense mechanism of conformity would be assumed if there is weak ego strength plus a high score on other-directedness. The Morris Paths of Life is a possible measure of other-directedness and the Thomas-Zander-Stotland scale is a possible measure of ego-strength. Not all conformity, even as a generalized trait, is ego defensive. This is the reason for including a measure of ego strength with a measure of other-directedness. The social norms would have to be ascertained through a statistical survey of a representative sample of the relevant group population.

Proximal attitude. The independent variable here would be an object in the environment which gives the individual consummatory satisfaction for some one of his motives. The other variable in this pattern would be a need state which arouses the motive in question. The intervening variable would be the proximal type of attitude, and the dependent variable would be (1) the expression of favorable beliefs about the object, (2) the expression of favorable affect toward it, and (3) patterns of overt behavior to acquire the object. Favorable beliefs and favorable affect could be expressed without the arousal of the motive but acquisitive behavior would require motive arousal. The measure of the independent variable would depend upon the motive in question. For a relatively uncomplicated appetitive drive like hunger, the individual could be asked such questions as "How hungry are you?" "When did you eat last?" "What foods do you like?" Other more complex motives would have to be measured either through longitudinal observation of the person or through projective tests similar to the measures for need achievement, need affiliation, and need for power.

Object-instrumental attitudes. The independent variables here would be objects which are instrumental to motive satisfaction but nonconsummatory and be the arousal of the relevant motive. The intervening variable would be an object-instrumental attitude; the dependent variable would be the expression of favorable affect toward the object, the expression of

favorable beliefs toward it, and other positive behavior toward it. Again, the affect and the expressions of belief could be elicited without motive arousal, but the full pattern of overt behavior of an instrumental sort would depend on motive arousal. The measures here would follow the same pattern as suggested for proximal attitudes.

Ego-instrumental attitudes. The independent variable here would be an object which is instrumental to the satisfaction of ego motives such as ego enhancement and self-determination. The intervening variable would be the ego-instrumental attitude. The dependent variable would consist of a favorable evaluation of the object. The measure of the independent variable would be based upon tests of objects which enhance the ego as indicated either by the self-report of the subject or by the reports of outside observers.

Affective associations. The independent variable would be an object which has been involved in the satisfactions of a motive but which bears no necessary relationship to such satisfaction. The intervening variable would be affective associations, and the dependent variable would be affect expressed toward the object, behavioral avoidance of the object if the association had been unpleasant but no positive overt pattern of behavior if the association had been pleasant. The measure of the independent variable could be based upon longitudinal observation of the individual or could be derived from statistical norms of the relevance of objects for motive satisfaction for many subjects who came from the same background.

The independent variables listed above all relate to the processes of need satisfaction in the person. Another type of dynamic stems from the tendency of the components of the attitude (affective, cognitive, and behavioral) to be consistent with each other. Thus, a change in one component can lead to a change in another. The first component is, therefore, an independent variable, whereas the other is dependent. We assume that the affective component has the most potency in changing other components and the cognitive has the least. The affective component can, of course, be influenced by changes in the person's pattern of need satisfaction, as indicated above. Such changes in need satisfaction act as independent variables to produce changes in all three components through first influencing the affective component. A person may sometimes behave toward the object of his attitude in quite a different fashion than would be predicted from a knowledge of the attitude. If this behavior is the result of some sustained environmental force from which the individual cannot escape, the behavioral component of the old attitude will become more consistent with the expressed behavior. As a result, the other components of the attitude will change to become more consistent with the modified behavioral component. The repeated oaths

of loyalty to Hitler by German officers helped to modify attitudes toward the *Führer*. In this type of attitude change, the independent variable is the behavior toward the object and the components of the attitude are the dependent variable.

Finally, we would admit, but give lowest priority, to changes which come about through changes in the cognitive component. A person may be given new information which changes his beliefs about an object, and in turn his feelings about it change as does his action orientation.

Barriers Blocking General Theoretical Advance in Psychology

We have already indicated our belief concerning the need for an intermediate level of concepts which would have some content orientation. We hold that one of the real barriers to general theoretical advance in social psychology is the distance between genotypic constructs and our phenotypic measures. In physics, the concept of atmospheric pressure is fairly close to its operational measurement. In physiological psychology, many concepts are similarly tied to their operational measurement. In personality theory and in social psychology, however, concepts like ego strength, defense mechanisms, role systems, and role conflict are so remote from their measurement that we have no single, clearly required set of operational measures.

We believe this is a basic difference between the social and the natural sciences. In general, this separation of concepts and their phenotypic indicators has produced two consequences. Factually minded investigators have pursued phenotypic observations and measures and have given us rank empiricism. In the natural sciences, such rank empiricism would have been much more useful because generalization would have emerged readily from the collection of facts. Theoretically minded investigators, on the other hand, have been satisfied with any measure which could be remotely justified as an indicator of the concept with which they were concerned. Thus their research has not led to cumulative knowledge. It is not because behavioral scientists are essentially different from natural scientists that there has been less progress in the behavioral sciences. It is because the closer relationship between the concepts and phenotypic measures in the *natural* sciences imposes objective restraints upon the investigator. We will not make substantial progress in the behavioral sciences unless we recognize the barrier produced by the nature of our subject matter and attack it along two major fronts: the development of an intermediate level of concepts between our genotypic constructs and their phenotypic indicators, that is, concepts of some level of generality which still point toward a class of objects; and more systematic testing of all the assumptions of a theoretical scheme, including the exploration of its relationship to a variety of empirical settings.

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LATENT STRUCTURE ANALYSIS

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INTRODUCTION

All the social sciences deal with concepts which seem somewhat vague. Who can, in practice, recognize an extrovert personality? Who has not read many discussions as to the real meaning of public opinion? Who can say precisely what a folk society is? There are various reasons why the social scientists' language has so many of these terms, which at first sight seem to be ill defined and even at their best are "fuzzy at the fringe." In some cases we can, by the nature of the concept, only observe symptoms, behind which we assume a more permanent reality. This would be true, for example, in the case of personality notions. In other matters the object of investigation is so vast that we can analyze only certain aspects of it: notions like patterns of culture or *Zeitgeist* belong here. For still other purposes the problem itself seems to require a looser kind of formulation: wherever we study adjustments—e.g., in marriage, in job performance, or in standard of living—we find that large numbers of actual solutions may serve the same functional purpose.

This peculiarity of the social scientist's intellectual tools has been deplored by some, considered as unavoidable by others. Most of all, however, it has been covered with nomenclature. Syndromes, genotypes, underlying concepts, hypothetical constructs, and many other terms have been used. It is hard to say to what extent we have today a clear formulation of the problem behind all these terms, let alone clear directions on how to deal with them in the pursuit of empirical research. And yet it is in the course of actual investigations that some clarification is most needed. For if we have to decide whether there is increased bureaucratization in government, or whether city life makes people progressively neurotic, we must get some measures of these tendencies. And whatever index we use, we make implicit assumptions about the meaning of the kind of terms which we have just exemplified.

Thus, problems of measurement, of meaning, and of concept formation fuse necessarily into each other. No empirical procedure of classifying social objects can be understood without reference to general logical discussions. And philosophical disquisitions about the nature of the social sciences are not likely to be fruitful without an incisive analysis as to how empirical social research does actually proceed.

The present paper will analyze one special procedure by which it is possible to make what one might call inferential classifications.

Any number of well-known topics are covered by this provisional name tag: a person's attitude as inferred from his behavior, the intention of a document as inferred from certain linguistic characteristics, the morale of a group as inferred from its various performances, and many others. No exhaustive listing or explicit definition will be given of the applications which we intend to cover. If it were possible to state clearly at the beginning the purpose of the procedures to be described, the whole paper—even the procedure itself—might be superfluous. But the basic thesis to be developed is exactly this: measurement, classification, and concept formation in the social sciences exhibit special difficulties; they can be met by a variety of procedures, and only a careful analysis of the procedure and its relation to alternative solutions can clarify the problem which the procedure attempts to solve.

The editor of this series of monographs developed a careful discussion of the nature of a "systematic formulation" for the contributors. He had in mind theoretical developments which attempted *prediction* of substantive observation. As will be seen, the following pages deal with the *organization* of such data. Consequently his outline could not be followed in detail. The presentation which our topic required turns out in retrospect, however, not to fit badly the general program of the whole project.¹

The first section, indeed, deals with "background factors and orienting attitudes" {1}. It takes some problems well known to psychologists and briefly sketches how they became clearer and more articulate between, say, 1900 and 1930. It shows how logicians have looked at the same matter. Finally, it suggests a formulation which leads to the threshold of the solution offered by latent structure analysis.

The second section turns to the "structure of the system" {2} and to the "initial grounds for its assumptions" {3}. It shows that a central axiom of local independence is an idealization of certain empirical procedures habitual in test construction and in survey analysis. The distinction between manifest data and latent parameters takes the place of the three types of variables suggested by the general outline. The accounting equations presented toward the end of this section summarize the "formal organization of the system" {6}.

The third section carries one example through the nine basic steps of a latent structure analysis. It gives, thus, an idea of the "measuremental and quantificational procedures" {5}. At this point the limitation of space is most obvious. The main contribution of the whole approach is its mathematical flexibility. The "construction of func-

¹ In the following paragraphs we refer to the rubrics of the editor by citations followed by the appropriate number.

tion forms" {4} depends upon the data under investigation, and their adequacy can be tested empirically. But each type of accounting equation must be studied in its own right. One example, therefore, can illustrate only the main principles involved and cannot provide procedures applicable to all models.

The fourth section does not continue the mathematical discussion but gives, instead, a variety of findings to indicate the "range of applications" {7}. The "mediating function" {8} of latent structure analysis is approached by discussing its relation to factor analysis and to formal test theory. It is not easy to decide whether one can talk of "evidence for the system" {9} in what is essentially a theory of concept formation. But even in terms of usefulness, it is too early to form a judgment. At the same time, it would be easy to show that "outside the present context" {10}, that is, beyond traditional psychology, the procedure has numerous applications to sociological and anthropological problems. No attempt was made to digress along these lines. The section, however, gives an idea of the high "degree of programmability" {11} the system has and how the "intermediate and long-range strategy" {12} points to numerous specific problems, which are not yet solved and require the collaboration of mathematicians, experimenters, and behavioral theorists.

A word needs to be added as to the administrative history of latent structure analysis. Much of the early work was generously supported by the Rand Corporation. An over-all monograph has been in draft for several years but was always delayed by new developments. In the meantime, some of the publications listed among the references have appeared. Other findings listed there are available as dissertations at Columbia University. A number of results, however, are still incorporated only in memoranda to the Rand Corporation. To facilitate orientation, the text and the references mention only two such memoranda. The first is a summary of the more recent mathematical developments; the second a collection of specific studies, in which various models were applied to empirical data. Finally, the author wishes to thank Mr. Arnold Simmel for much editorial and computational help and Miss June Alter for resourceful secretarial work in the preparation of the present summary.

I. SOME PROBLEMS OF CLASSIFICATION IN THE SOCIAL SCIENCES

The trait and other intervening variables. Traits became the topic of more systematic reflection in connection with moral problems—at least as far as American psychological literature goes. One starts naturally with William James's *Principles of Psychology*; there one does

not find the term "trait" in the index. In the chapter on habits, however (chap. 10), there is an extensive discussion of how people can acquire desirable habits like industriousness, or lose undesirable ones like drunkenness. This intertwining of ethical problems with the question of traits, their acquisition and change, is still equally strong in John Dewey; it is the main theme of his *Human Nature and Conduct*. Dewey also uses "habit" as his central term; he considers it interchangeable, however, with terms like trait, characteristic, attitude, and tendency. Dewey's concern with the changing of habits, the examples he chooses, and the advice he gives are often very similar to James's treatment.

The moralist observes differences in his and other people's conduct, tags them as good or bad, and reflects on how valuable traits can be strengthened. The methodologist starts from the same observations but is more interested in defining, classifying, and measuring these traits. The pragmatists were a combination of moralist and logician, and they found a way to fuse their double motivation into a view which combined their activist philosophy and their operational idea of scientific work. James showed this clearly [11, our italics].

Suppose, e.g., that we say a man is "*prudent*." Concretely, *that means that he takes out insurance, hedges in betting, looks before he leaps. . . .* As a constant habit in him, a permanent tone of character, it is convenient to call him prudent in abstraction from any one of his acts. . . . There are peculiarities in his psychophysical system that make him act prudently. . . .

We are not surprised to see that James is very explicit on the relation between an inferential concept and the indicators connected with it. He is, after all, the one who coined the phrase "concepts signify consequences." He felt that abstract descriptions are often useful enough, yet they are "sucked up and absorbed without residuum into the concrete ones, and contain nothing of any essentially other or higher nature which the concrete descriptions can be justly accused of leaving behind." Here is picturesque language, the precise meaning of which is not easily checked. But it is safe to assume that James inclined toward identifying the concept and its indicators. Now this leads to obvious difficulties. Do all prudent people always look before they leap? Where do we take account of the amount of insurance taken out? How about people who show some but not all the symptoms mentioned by James? Inversely, should not other indicators have been included?

Dewey was obviously aware of these difficulties. As a moral philosopher he was not less convinced than James of the mutual interaction between "disposition" and "doing"; today's tradition of progressive

education is testimony to his point of view. But as a logician, he saw a looser connection between concepts and indicators. He warned that one should *not* "assume that there is or ever can be an exact equation of disposition and outcome."

It was characteristic of habits [6] ". . . that their outworking in any particular case is subject to contingencies, to circumstances which are unforeseeable and which carry an act one side of its usual effect."

Here a new idea comes to the fore, although rather shadowy at first—the notion of probability. It is easy to see how the progress from James to Dewey can be reformulated. The prudent man is *likely* to look before he leaps because any specific behavior item is only a probable but not a necessary condition of a related trait. This has remained ever since as one well-recognized element in all presentations. In an early systematic discussion Allport tried "with the aid of eight criteria to define trait and to state the logic and some of the evidence for the admission of this concept to good standing in psychology." One of his criteria is important here. [2]

Acts and even habits that are inconsistent with a trait are not proof of the non-existence of the trait . . . there are in every personality instances of acts that are *unrelated to traits, the product of the stimulus and of the attitude of the moment*. Even the characteristically neat person may become careless in his haste to catch a train.

A second element has not found equally clear recognition. How does the notion of a trait develop, either in daily life or in scientific work? We experience, say, anxiety, and its role in our own course of action (R). We observe how other people act in situations (S) which would, we know, bring on our anxieties; we notice that their reaction R is similar to ours. As a result, we file away in our minds that as a rule such stimuli S are likely to be followed by responses R. We "explain" such S-R sequences with the help of an intervening variable: anxiety. The value of this construct becomes particularly apparent if many S-R situations are observed where the S and the R vary, but where the same intervening variable (anxiety) seems appropriate. We can then organize our observations in a somewhat more economical way: we remember the series of x situations which create anxiety and the series of y responses by which anxiety is expressed. Instead of registering x times y relationships of the S-R type, we need only remember $x + y$ findings: the x prompters to and the y indicators of anxiety.

A schematic presentation, on page 482, has been proposed [18].

If we want to "create anxiety" we would choose one or more situations on the left side of the frame, and if we want to "measure anxiety" a selection and combination of responses from the right side would be

necessary; they would be used as indicators. The important point is that usually several indicators would be derived by an investigator, i.e., facial expressions, interpretations of ink blots, etc. But because indicators have only a probability relation to traits, the crucial problem arises as to how they can be combined if they do not all go in the same direction, for example, if in a specific case R_1 makes high anxiety and R_2 low anxiety probable. It is too early even to hint at an answer here; but it is surprising how grievously the issue can be missed by

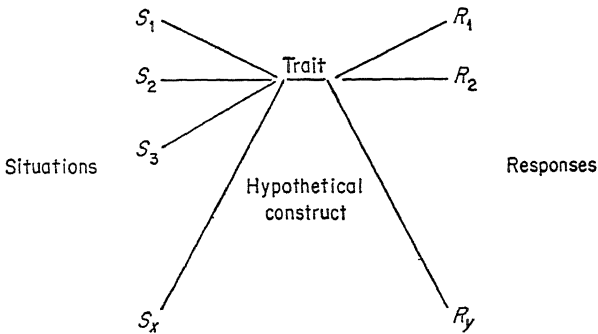


FIG. 1.

scholars when they leave the field of their special research experience.

When Edward Tolman developed the notion of intervening variables, he was concerned about how the intervening variables are operationally defined, measured, or whatever other terms he used to apply in pointing to the problem. The answer Tolman gave fifteen years ago [29], when he was concerned with the behavior of a rat at a choice point, is the same he gave recently when his whole system was "elaborated for the special case of a hungry actor going to a particular restaurant and ordering and eating a particular food." For the rat it read as follows [28, p. 333]:

By an operational definition of an intervening variable I shall mean, first, a statement about a standard defining experiment in which a certain measurable variation in some feature of the observed behavior will, by definition, be assumed to be a direct measure of corresponding variation in the magnitudes of a given intervening variable. Second, such a definition will involve an assumption about the linear or nonlinear nature of this mathematical function connecting the measured feature of the dependent behavior to the intervening variable. And, third, the specific constants in this form of mathematical function must also be known, or assumed, before such definitions will be final.

The idea is that we can find one specific indicator for each intervening variable. Everything else being constant, the variations in the indicators correspond to the variations in the intervening variable.

We have grave doubts whether such a procedure is feasible even with animal experiments. And we are confident that it is the wrong idea as far as the study of human behavior is concerned. Tolman's own description of how he would proceed in a concrete research situation shows this. At one point he exemplifies an "actor's belief-value matrix" by the opinion he has about various restaurants, his food preferences, etc. How will Tolman find out about this intervening variable? By [28, p. 295]

. . . mere questionnaires or interviews. Thus, for example, one could ask the subjects: (1) "What are you ready to do when you haven't eaten for a considerable length of time?" (2) "What kinds of foods do you like? Name six varieties of food in order of preference. What do you like about each of these six?" (3) "For each of these six foods what types of restaurant would you go to and in what order? List all the considerations you would take into account in choosing the one kind of restaurant or the other."

The repeated references to questionnaires in this monograph make it easy to predict what problems Tolman would face if he were really to develop measurements along his line of argument: the student experienced in social research knows that answers to questionnaires vary considerably if wordings are slightly changed, if the interview is done under slightly varying conditions, etc. There is just no way to develop a "standard experimental setup" or "standard defining experiment." We will have to face the fact *that to an intervening variable there will correspond a variety of indicators and that they will have to be reconciled in some way.*

The discussion on intervening variables covers a wide range, and we cannot enter it in detail. Tolman was chosen as an example because he is, to our knowledge, the only one who applied the equipment of the learning theorist to an everyday life situation. There exists, however, a careful analysis of Hull's writing which brings out one point of special importance in the present context. Koch has pointed out that the relation between an intended measure and the multiple ways it can actually be approached does not pertain only to intervening variables in the narrow sense [13]. In discussing the notion of "independent variables," he makes a distinction between systematic and experimental independence. The "experimental independent variables *may* be specific singular realizations of a systematic independent variable; they are not however to be identified with it," and well might not be singular [13; p. 28]. Koch, too, is critical of the "remote leaps from the data of single defining experiments to general theoretical statements." He states as a general principle that "all alternate experimental variables to which a given independent vari-

able is reducible . . . must be brought to converge by appropriate scaling techniques" [13; p. 65]. In a way, Koch's position is very similar to the one we take here: every "systematic variable" is intervening in the sense that additional assumptions are needed to link it with an array of actual observations.

We find then that a tradition has grown in psychology whereby the intended classification required by concepts like trait, attitude, intervening variable, etc., is performed by using indicators directly accessible to the investigator. These indicators are presumed to have a probability relation to the "underlying" (intended) variable; and because of it, if we use—as we invariably do—a number of indicators simultaneously, we will always get into "contradictions" which have to be reconciled. Before discussing the matter further we shall show that the psychologists' problem is only a special case of the general logical issue regarding disposition concepts.

The disposition concept. In recent writings of logicians, one can find frequent discussions of "disposition terms" which refer not to a directly observable characteristic, but rather to a disposition on the part of some physical objects to display specific reactions under specifiable circumstances. The definition of such terms seems to create considerable difficulties. A famous paper by Rudolph Carnap on "Testability and Meaning" [5] has convinced most of his fellow philosophers that for the introduction of such a term a somewhat different kind of logical operation is needed, which he calls partial definition or *reduction*. Following Hempel's simpler presentation the correct way to "define" the disposition term "magnetic" would be as follows [10; p. 26]:

(6.4) If a small iron object is close to x at time t then x is magnetic at t , if and only if that object moves toward x at t .

This definition is partial for one obvious reason. If there is no way to approach x with small iron objects, e.g., x is at the bottom of a lake, we could not determine whether it is magnetic or not. Hempel further states [10, p. 27]:

The indeterminacy in the meaning of a term introduced by a reduction sentence may be decreased by laying down additional reduction sentences for it which refer to different test conditions. Thus, e.g., if the concept of electric current had been introduced previously, (6.4) might be supplemented by the additional reduction sentence:

(6.5) If x moves through a closed wire loop at t , then x is magnetic at t if and only if an electric current flows in the loop at t .

Continuing this trend of thought, we find to our pleasant surprise that the modern logician is disclosing a practice of the natural sciences,

which was considered to be embarrassing by many social scientists. That is, they define important concepts as "intervening variables" or underlying constructs by reference to a series of test situations, which all have to be used together.

A historian of science might one day try to prove that this emphasis on the special nature of disposition concepts results from the growing importance of the behavioral sciences. It is not without interest that the psychological term "disposition" is here introduced into the epistemology of the natural sciences. The connection with the problem of introspection is explicitly referred to in Carnap's original paper. During an autobiographical remark on how he developed his notion of reduction, he says [5]:

The members of our [Viennese] Circle did not wish in former times to include into our scientific language a sentence corresponding to the English sentence S: "This stone is not thinking about Vienna." But at present I should prefer to construct the scientific language in such a way that it contains a sentence corresponding to S.

The formal analysis of the procedure is, of course, independent of its history and its terminology. The question is whether it really covers the research procedure with which we are concerned here. To decide this, we must add two more elements in Hempel's exposition. First is his distinction between the empirical and the theoretical import of concept formation [10, p. 46, italics ours]:

In the theoretically advanced stages of science these two aspects of concept formation are inseparably connected; for, as we saw, the interpretation of a system of constructs presupposes a network of theoretical statements in which those constructs occur. *In the initial stages of research, however, which are characterized by a largely observational vocabulary and by a low level of generalization, it is possible to separate the questions of empirical and of systematic import; and to do so explicitly may be helpful for a clarification of some rather important methodological issues.*

This has immediate bearing on the enterprise in which we are engaged here. Indeed we shall concentrate on certain measurement procedures to clarify how we create "underlying" concepts like traits, attitudes, group characteristics, etc.: their role is to summarize a variety of empirical observations and to store them, one might say, for systematic use in a "theory" which we hope will one day develop.

No one can seriously deny that most of the social sciences are in what Hempel refers to here as the "pre-theoretical stage of research." On this point, then, the Carnap explication of disposition concepts is fully transferable to our problem area.

On another point, however, we must look for an additional development. Hempel points out what is implied when we use a variety of

reduction sentences. Let us go back to the example of magnetism, where attracting metal and inducing currents are used as two test situations [10, *italics ours*]:

But, since the two conditions are not exhaustive of all logical possibilities, the meaning of the word is still unspecified for many conceivable cases. On the other hand, the test conditions clearly are not logically exclusive; both may be satisfied by one and the same object; and for objects of this kind the two sentences imply a specific assertion, namely: Any physical object which is near some small iron body and moves through a closed wire loop will generate a current in the loop if and only if it attracts the iron body. But this statement surely is not just a stipulation concerning the use of a new term—in fact, it does not contain the new term “magnetic,” at all; rather, it expresses an empirical law. *Hence, while a single reduction sentence may be viewed simply as laying down a notational convention for the use of the term it introduces, this is no longer possible for a set of two or more reduction sentences concerning the same term, because such a set implies, as a rule, certain statements which have the character of empirical laws.*

The reader who has followed our examples in the previous sections will have noticed that there the reduction sentences are different in one respect. A “magnetic personality” is one which is likely to attract other people, which is likely to induce in them currents of enthusiasm. As we have pointed out, the items of observation are linked to the concepts to be defined by probability relations. One other logician has seen this point very clearly.

In a short paper on “Definition and Specification of Meaning,” A. Kaplan moves on from Carnap’s partial definition. He recapitulates the position in the following words [12]:

Whenever a term is introduced into a context of inquiry . . . situations . . . are described in which the term may be applied. Any such description may be called an indicator for the term. But . . . indicators assign to the application of the term under the described conditions, not a logical certainty but only a specified weight. Thus failure to interbreed is an indicator for distinctness of species; but that two animals do in fact interbreed does not logically entail that they belong to the same species but only adds some weight to the assumption.

Kaplan draws his examples from biology and occasionally from one of the social sciences. The importance of his analysis is his clear recognition that the relation between the indicators and the concept to be specified does not need to have the rigid relationship implied in the original Carnap formulation. In short, says Kaplan, “What is suggested here is that indicators be formulated in terms of some type of probable implication.” He is also aware of an important consequence of this more general approach to our problem: if we have two test

situations it is not necessary that their outcome be related by a rigid law. To turn once more to the example of magnetism, it is now sufficient to say that attracting iron objects and inducing electric current are correlated, that they frequently occur together but not necessarily always. To this point we shall return once more.

Whereas Hempel stressed that such concept formations are characteristic of any early stage of science, Kaplan stresses the fact that they facilitate flexibility of thinking and therefore leave the road open for new developments. In Kaplan's formulation [12]:

We begin with indicators in terms of which the initial application of context can be confirmed. As the context of application grows, the specified meaning grows—and changes—with it. The stipulation of new indicators affects the weight of the old ones, while they in turn limit the range of choice in the stipulation. The adequacy of a particular indicator is not judged by its accordance with a predetermined concept; the new and old indicators are appraised conjointly.

Thus something which seemed to be an embarrassing shortcoming of social science concepts, such as IQ, introversion, or cohesion, becomes the common property of a large group of concept formations in all sciences.² In all such cases we must decide what items should be included in the base of observations from which intervening variables of any kind are inferred. The explication of disposition concepts thus certainly covers all the elements we are concerned with: the use of indicators to place people correctly into an "underlying" order required by a more abstract conceptualization, the somewhat fluid choice of these indicators, their probability relation to the intended "ordering," the consequent fact that they will not *all* point in the same direction and that, therefore, they have to be combined into a kind of "index" or "measurement" which represents the best inference which can be made from the manifold of our empirical observations. But the formulation of the logicians is so general that it does not lead directly to concrete research operations. If they are our goal, one more translation has to be attempted. The notion of "property space" seems to serve this purpose best.

The property space. The term "space" has had an interesting biography. Originally it was used to connote the direct experience people had when they located things in their surroundings. Then it was seen that the points in a space could be described in algebraic terms. Now everyone is acquainted with the notion of "coordinates." Start-

² Carnap also stresses that often "We wish to determine the meaning of a term at the present time for some cases only, leaving its further determination for other cases to decisions which we intend to make step by step, on the basis of empirical knowledge which we expect to obtain in the future" [5].

ing, say, with the corner of a room, any other location in this room can be indicated by saying how high up it is from the floor and how far it is from the two walls which meet at the original corner. To each point corresponds a triplet of distances. This leads to the extension of the notion of dimension. Although the points in the room require three data for their location, on a blackboard we can work with only two coordinates—which is identical with saying that the face of the blackboard, or any other plane, has two dimensions. Correspondingly four-dimensional sets become easy to grasp. The best known is the space-time continuum: a bug in a room can be characterized by the point at which it rests and the amount of time it has been there.

There developed finally an inversion of terminology. Whenever a set of objects is characterized by a multiple of data one would talk of them in terms of points in a space. This space would have as many dimensions as there are data needed to characterize each of the objects under consideration. The advantage of this terminology is that it brings out formal similarities between materials which would be overlooked because we habitually give them different representation. Take as an example two students who were given three tests, language (L), social science (S), and natural science (N). Assume their test profiles look as follows:

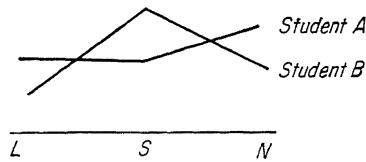


FIG. 2. A test profile of two students.

Now the test scores are triplets of data and therefore can be considered coordinates in a three-dimensional space. To each test corresponds an axis and the two students thus become two points.

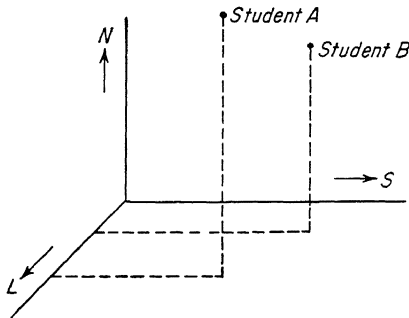


FIG. 3. The same tests as in Fig. 2 in terms of a test space.

This translation into space terminology has the advantage that a number of seemingly disparate notions turn out to be clearly related. The similarity of profiles, for example, can be expressed as the distance between points; the famous "ideal type" becomes a special region in the property space, e.g., the region around the origin [4].

So far our examples have all assumed that the basic data which characterize our objects are in some way quantified. But this is not necessary, and with this last step the most general notion of property space is reached. The dimensions may, for example, be rank numbers of positions in a preestablished list. All people with Christian, middle, and family names can be put into a three-dimensional "initial space" in which each dimension has 26 "classes," the letters of the alphabet. Therefore, a man with the parameters (4,1,3) would have the initials D. A. C., and David Arthur Chester and Donald Avery Casey would belong in the same "point" in this space. In other cases the properties might well be dichotomies, i.e., attributes which take on two values only. Suppose, for example, people are classified according to whether they are male or female, native or foreign born, above or below thirty-five years of age, residing in a city above or below 100,000 population. This would provide a space of four dimensions, but on each of them, objects could only have two distinguishable positions; or to put it still another way, each of the four coordinates could take on two values only. The whole "space" would therefore consist of

$$2 \times 2 \times 2 \times 2 = 16$$

"points." This space will be of basic importance for our subsequent discussion.

The relation of manifest to latent property space. We reach the end of this introductory section by showing how the explication of disposition concepts and the notion of property space merge into a rather precise formulation of our main problem. To begin with, we must see the close connection between definition and classification. One should not be deceived by differences in wording. Many of the authors we reviewed seemed to ask: what is intelligence, prudence, or friendship? Actually these writers visualize themselves as being confronted with concrete cases of "intelligence" or "prudence" and want to know how to recognize them, how to relate them to each other, and so on. If we could ask these writers some further questions, they would say something like this: they want to differentiate "types" of "friendship" and "love" or distinguish between "prudence" and "distrust." From a research point of view, these are all problems of classification, although of a special kind as will be seen in the course of our discussion.

“Measurement” is also a special case of classification; it is irrelevant at this point to distinguish “measurement” from “ordering” and other classificatory devices.

The reader should have no difficulty in referring to the many examples given in the preceding pages, and in verifying that the terms mentioned, such as “traits,” “intervening variables,” “disposition concepts,” etc., are really special cases of classificatory characteristics. They have one thing in common: they are *intended* characteristics; that is, they are ways in which we *want* to organize a set of objects under investigation. This locating of “objects” (individuals, groups, social relationships) cannot be done directly in the cases we have discussed. We are dealing with *latent characteristics*, in the sense that their parameters must somehow be *derived* from *manifest observations*. The terms manifest and latent have no connotation here beyond the distinction between data directly accessible to the investigator (manifest) and parameters which in some way must be inferred from the manifest data (latent).

The matter can be reformulated in the following way. Empirical observations locate our objects in a manifest property space. But this is not what we are really interested in. We want to know their location in a latent property space. *Our problem is to infer this latent space from the manifest data.* This reformulation of the relation between concept formation and classification by indicators has a number of advantages. One of them deserves special attention.

In any empirical classification guided by conceptual considerations we try to overcome the accidental elements inherent in the use of indicators. Suppose we want to order people according to how they feel about the role of government in economic affairs. We might ask them a series of questions as to public ownership of railroads, mines, banks, etc. It is reasonable to assume that the more someone favors *laissez faire* the fewer of these items he will answer *pro* public ownership. Still we know that many individual idiosyncrasies will creep into the answers. A strong *laissez faire* person has just read about a mine accident and under this impact he gives a *pro* public ownership response to the mine item; a strong interventionist happens to know a very fine bank president and therefore excludes the bank item from his list of *pro* responses. In the manifest property space we are at the mercy of these vagaries. But in the latent space, as we shall see, we can take them into account and thus achieve a more “purified” classification.

We are now ready to turn to the one question which has still been left unanswered: how is the probability relation between the observed indicators and the intended classification established? How do we move from the manifest to the latent property space?

**II. THE LOGICAL FOUNDATION OF LATENT STRUCTURE ANALYSIS:
A SYNOPSIS OF THE MAIN ISSUES**

Inferential classifications with the help of a set of indicators are nothing new in the world of science. A doctor who uses a series of tests to see whether a patient has tuberculosis, a psychoanalyst who uses free associations to retrace a childhood experience, a chemist who observes various reactions to identify the nature of some substance—all use what might be called diagnostic procedures. They know, or believe they know, laws and regularities which link their manifest indicators with their latent space. Their diagnosis applies previous knowledge to a specific new case.

Some initial clarifications. But there exists a second type of procedure where, so to say, the acquisition of general knowledge and its application to a specific case are performed simultaneously. This happens if the starting point of an investigation is a statistical one and if our attention is mainly focused on the covariation of indicators in a large number of cases. The present section is devoted to a clarification of this idea. It will help if we sketch the course of the following discussion by raising a number of questions and offering some preliminary answers.

In the previous section we tried to describe the intellectual climate which led to the general idea of latent structure analysis. Now it is necessary to describe its elements more precisely. We shall first list them and then discuss their ramifications in some detail.

1. With what kinds of manifest material shall we deal? They are qualitative, but to further simplify matters, they will be dichotomies through most of this report. Thus our examples will be “yes” or “no” answers to an observation. Does a man agree or disagree with a statement? Is he native or foreign born? Is a city above or below the national suicide rate?

We shall call any piece of such information an “item.” In each case we shall have an *item list* in which items are numbered in an arbitrary but fixed way. The number of items in this list coincides with the dimensionality of the manifest property space.

One alternative of each dichotomy will arbitrarily be called positive (+), the other negative (−). Often a judicious use of these designations will help in intuitively grasping the material as a whole. Each object in our study will be characterized by a *response pattern* of the following kind:

	<i>Response to item no.</i>				
	1	2	3	. . .	<i>m</i>
	+	−	+	. . .	+

The term response pattern is taken from questionnaire practice but is used here in a metaphorical sense. The items might all be derived from observation, e.g., the behavior of a person in various situations, and the objects might be collectivities and not individuals.

2. A whole group of "respondents" will be characterized by their *response frequencies*: they are the proportion of the group who answer each item i affirmatively (p_i), two items (p_{ij}), three items (p_{ijk}), etc. A barred index will be used to indicate a negative response. Thus $p_{1\bar{2}3}$ is the proportion of people who give an affirmative answer to item 1 and 3 and a negative answer to item 2. The whole set of these response frequencies is called a "dichotomous system" and its nature is very important for a more detailed study of latent structure analysis. For our present purpose, acquaintance with the symbolism will suffice.

3. How are we to represent a latent space? In the traditional way by a system of coordinate axes, i.e., a so-called cartesian frame of reference. An example of a three-dimensional frame of reference was given in Fig. 3. Actually in this section we shall restrict ourselves to one-dimensional latent spaces to facilitate exposition. But in a later section we shall see that assessing the number of dimensions in the latent space is possible. A one-dimensional space is, of course, a straight line. We shall often call it a *latent continuum*.

4. What is the relation between the manifest items and the latent continuum? It involves probabilities. Thus we shall assume that there exists a curve—preferably a mathematically simple one—which relates to each point of the latent continuum a specific probability that a given item has a positive response. Suppose that our latent continuum deals with socioeconomic status, and three of our items are

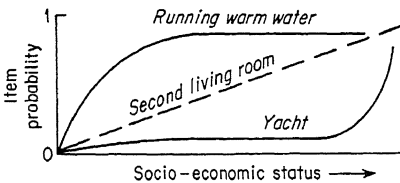


FIG. 4. Probability relations between the intended classification by socioeconomic status and the observed frequency of three indicators.

ownership of a yacht, presence of running warm water, and presence of two living rooms in the house of the respondent. Common sense would let us suspect that the corresponding probability relation will be somewhat like Fig. 4. The graph intimates that very quickly as we go up the social scale almost every family will have running warm water; only the upper

crust is likely to own yachts, while the probability of more space in addition to bedrooms increases fairly proportionally with socioeconomic status. The truth of these surmises is not relevant here. What matters is the way they are expressed through Fig. 4. Of course we have not yet stated precisely what we mean by probability, and we

are still in the dark as to how we would define and know a respondent's socioeconomic status. These two points soon will be taken up in considerable detail.

The curves of probabilities in Fig. 4 we shall call *trace lines*; they trace the probability for an item as a "respondent" moves along the latent continuum. If the latter is a two-dimensional one then the probabilities form a trace surface. In full generality we shall talk of *latent traces*. Notice that a trace line is defined for each item separately. Later a crucial problem will be what to think about the probability of joint responses to several items occurring simultaneously at each point of the latent space.

5. How are we to understand the term "probability" used all through the preceding pages? A traditional example of how to look at probability is as follows: we take a sample of people aged fifty and find out how many die within the next year; we compute the proportion of people who died between the ages of fifty and fifty-one. Then we generalize this ratio and say that it is the probability of dying at the age of fifty within one year. This operation can be refined as far as we want to go. We might say, for instance, that the probability of business executives' dying within a year at the age of fifty is greater than the corresponding probability for office clerks. The class for which such probabilities are computed and then generalized is usually called the reference class of the probability [21].

This same procedure, however, can be used in still another way. Suppose we ask an individual, Mr. Brown, repeatedly whether he is in favor of the United Nations; suppose further that after each question we "wash his brains" and ask him the same question again. Because Mr. Brown is not certain as to how he feels about the United Nations, he will sometimes give a favorable and sometimes an unfavorable answer. Having gone through this procedure many times, we then compute the proportion of times Mr. Brown was in favor of the United Nations. This we could also call the probability of Mr. Brown's being in favor of the United Nations. But now the reference class is not many Mr. Browns having been asked this question once, but one Mr. Brown having been asked the question many times.

There is one interesting consequence of this version of the probability notion. A specific Mr. Brown, for instance, might feel that it is his duty to be in favor of the United Nations. Therefore, if he is asked a question when he is sober, his probability—or, if you please, his propensity—to be in favor of the United Nations might be rather high. Under the influence of alcohol, however, his hostility to the international organization might come out. Therefore, his probability under the influence of alcohol could be different than his probability if he

were sober. This is an idea which is often used in the parlance of daily life. A man can drive "60 miles an hour" and at the next moment if a policeman is around, slow down to "40 miles an hour." What we call miles per hour is not what a man actually drives within an hour, but what he would drive if certain specified conditions were to prevail. Thus, we have a concept of probability which can apply to a single individual; furthermore, this probability or propensity itself can be different under various conditions.

6. How can we know trace lines? This is, of course, the central problem of latent structure analysis, and the third section of this report is given to developing an appropriate answer. The remainder of the present section will provide some preparation, considerations, and examples.

The whole configuration of trace lines for all items and the location of each object in the latent space is called the latent structure. It is a typical example of what is often called a "mathematical model," a construct which is derived from actual data together with certain general reflections on the purpose these data serve. In our case the situation is as follows: from our manifest data we actually know the frequencies in which the various response patterns occur in a given population; what we want to know are the *latent parameters* of the model, the coefficients which characterize the latent traces, and the distribution of the population within the latent space. We therefore need equations which link the manifest frequencies to the latent parameters. From these so-called *accounting equations* we then can compute all the elements in the model. The name given to these equations is meant to indicate that with the knowledge of the full latent structure, we can account for everything known about the manifest data.

In order to clarify this basic idea, it is best to discuss in some detail two empirical operations with which most research students are well acquainted. By a slight extrapolation they become basic elements of latent structure analysis. The situation is somewhat similar to what was just mentioned about the concept of probability. Probabilities are formal extrapolations from the empirical notion of relative frequencies. In our case we are referring to item analysis which forms the basis of the notion of trace lines, and to the "explanation" of statistical relations which becomes the basis for the accounting equations.

We turn first to item analysis and trace lines.

Item analysis and item curves. Every graduate student who takes a course in applied psychology knows about item analysis. If he wants to develop an attitude or a performance test he knows that he should proceed in the following way. He is permitted to start with many questionnaire items which he hopes will be indicative of what in

the end he wants to "measure." But then he is supposed to distinguish between good items and bad items. This he is taught to do in the following way: he forms a "raw" score by adding up for each respondent the number of items which are answered in the "correct" way. Then he plots each single item against this raw score. (This we shall call the *item curves*.) The items which have a high association with the raw score are acceptable. The items which have a low association are considered inappropriate and should be eliminated. We will now give an example of such an item analysis, but we will refine it in two ways:

1. We shall plot *two* items against the raw score.

2. We shall not only plot each item separately, but we shall investigate *how the association between the two items is related to the raw score*.

Item analysis applied simultaneously to more than one item.

Our material comes from a public relations study where 560 respondents were asked questions regarding their attitudes to the oil industry. Do oil companies treat their workers fairly; do they make too much profit; are they wasteful of our natural resources, etc.? To each question the respondent could give one of five answers, which ranged from firmly favorable to firmly unfavorable (from the oil industry's point of view). There were ten questions in all and eight of them were combined into an arbitrary score in the following way: a firmly favorable answer was given a weight of 4 and so on down to the firmly unfavorable, which got a weight of 0. Then all the weights were added so that a respondent's general attitude score could range from 0 to 32. This score was used as the "outside continuum" or base variable. Against it the probabilities (proportion) of answers to the remaining two questions were plotted. These two were:

Item 1. Do the big oil companies control too much of the oil business?

Item 2. Is the oil industry wasteful of our natural resources?

TABLE 1. THE INTERRELATION OF TWO TEST ITEMS FOR FIVE SUBCLASSES OF RESPONDENTS CLASSIFIED ACCORDING TO A GENERAL ATTITUDE SCORE DERIVED FROM EIGHT OTHER QUESTIONS*

		Item 2			Item 2			Item 2			Item 2			Item 2		
		+	-		+	-		+	-		+	-		+	-	
Item 1	+	3	14	17	15	27	42	35	26	61	40	18	58	67	17	84
	-	30	59	89	32	61	93	34	29	63	13	21	34	12	7	19
Total		33	73	106	47	88	135	69	55	124	53	39	92	79	24	103
General attitude score		0 to 16			17 to 20			21 to 23			24 to 26			27 to 32		

* A response favorable to the oil industry is indicated by a + sign.

A positive reply (from the industry point of view) was one in which a respondent expressed at least some disagreement (score class 3 and 4). The joint positive response required such disagreement with both items. In Table 1 we now have the data for the item curves for each item alone and for the joint responses. The vertical marginals of the five partial fourfold tables indicate the item curve of item 1. In the lowest general attitude group, 17 out of 106, or 16 per cent, give an affirmative response. In the group farthest to the right, 84 out of 103, or 84 per cent, do so. The horizontal marginals indicate the item curve for the second item; the corresponding figures are 31 per cent and 79 per cent. Thus item 1 (concern with economic control) is more expres-

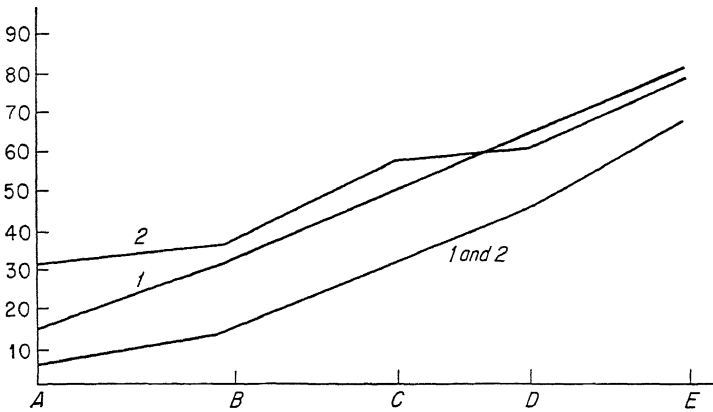


FIG. 5. Item curves corresponding to Table 1.

sive of the "underlying" classification than item 2 (concern with natural resources). Figure 5 shows the item curves for items 1 and 2 and adds a third: the proportion of people who give a positive answer to both items. These proportions are based on the left upper corner figures of each of the five fourfold tables in Table 1. Note that the item curve for both items is more concave (seen from the top) than either of the curves for items 1 and 2 separately.

The items in our attitude tests have obviously been selected by the investigator as indicators of an underlying continuum according to the reasoning discussed in our historical section. The item curves are a crude representation of the relation between these indicators and the intended classification. But what about the relation among the indicators? The reader will remember that we came to the general expectation that indicators will be statistically related to each other because they have their links with the underlying continuum in common. In terms of probability notions we can now put it this way: the probability of joint occurrence p_{12} will not be $p_1 p_2$ —the chance result of two

independent probabilities—but, rather, p_{12} will be greater than p_1p_2 . In the empirical data we shall expect a positive association in the fourfold table, which cross-tabulates the reply to two items. This turns out to be the case in our public relations example, as can be seen from Table 2.

Table 2 is obtained by adding the five partial tables in Table 1.

TABLE 2. THE INTERRELATION OF ITEMS 1 AND 2 OF TABLE 1,
FOR ALL 560 RESPONDENTS

		Item 2		
		+	-	
	+	160	102	262
Item 1	-	121	177	298
		281	279	560

Here $p_{12} = 160/560 = .29$, whereas $p_1 \cdot p_2$ is only $(.47)(.50) = .24$. Still, using rather informal language, we can say that the responses to two items of a test show positive relations because they were chosen as indicators of an underlying property. But this argument can be turned around. *If a class of people are alike in an underlying property, then the indicators of this property should not be statistically related in this class.* In our example we can submit this idea to a crude test. Our general score was supposed to be a crude measure of the general attitude of the respondent to the oil industry. By dividing the respondents into five classes, as in Table 1, we get groups of people who among themselves have a rather similar attitude.

In general our expectation is borne out. We now have five partial fourfold tables: the association is negative in one, practically zero in two, and positive in two. The five tables can be looked upon as chance variations from an association which is actually zero. Extrapolating the results of Table 1, we can say that if by an appropriate score, the underlying property of a population is kept constant, then the indicators of the property are statistically unrelated.³

³ Whether the association or correlation between items 1 and 2 in such sub-classifications can be considered a result of chance can be tested by χ^2 procedures. It should be desirable to obtain more such examples, because the present one points to an interesting possibility. The five associations go uniformly from negative to positive as we move from low to high general scores. If this turns out to be the case in other tests, we would be confronted with a result in test psychology which deserves further investigation and interpretation. For our present purpose this matter is irrelevant, because we use our concrete data only to lead up to an axiomatic idealization.

The essence of our example is this. On the basis of their raw scores, we divided our respondents into five classes. Within these classes raw scores are relatively similar, and this similarity is understood to correspond to the similarity of individuals within a class in their general attitude toward the oil industry. Just as individuals within a class have a similar attitude, individuals in different classes have dissimilar attitudes. This dissimilarity manifests itself in the differences between probabilities of affirmative responses in the different classes. *Within* each class the probabilities for all people are the same for any one item; of course, different items will generally have different probabilities of affirmative response within a given class. Considering the responses to a single item by individuals in one class, we still find a mixture of positive and negative responses. After all, the class does not determine the response; it only determines the probability of each response. The variability of response is supposed to stem from accidental elements. Quite irrespective of their attitude toward a specific industry, some people happen to be more concerned with the preservation of natural resources, others are more worried about the growth of economic monopoly. Biographical and other reasons might account for such a difference; in any case, these idiosyncratic elements are assumed to be unrelated to each other. Within a class which is homogeneous in regard to its basic attitude, the answers to specific items are assumed to be unrelated. This was not quite the case in our concrete example but was enough so that an extrapolation seems indicated. *We shall define a homogeneous class as one in which this statistical independence of indicators prevails.*

This leads us to investigate the characteristics of a group of respondents which can be considered a mixture of subgroups where, within the subgroups, a set of indicators are statistically independent of each other.

The “mixture” phenomenon and its role in the explanation of statistical relations. We start with a simple case. Suppose that in each of three groups two items are statistically independent. Table 3 should be looked upon as an idealization (and simplification) of Table 1. We “mix” these three groups and form a new one by adding box by box the corresponding numbers on the left side of Table 3. On the right side we now find an association between the two items, which

TABLE 3

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did not exist in the three partial tables to the left. Where does this association come from? We understand this best if we look at the margins of the three left-side parts of Table 3. In Class I the probability of a positive response is much lower on *both* attributes than in Class III. If in the combined population we select successive respondents, they will sometimes come from Class I and sometimes from Class III. In the former case, they will be more likely to give negative responses on both attributes; in the latter case, both responses are more likely to be positive. *The statistical association between the two items in the total population is thus accounted for by the fact that each attribute by itself is positively related to the general attitude of the respondents which distinguishes the three subclasses.* This was, of course, also the case in our previous example of the public relations study of the oil industry. Table 1 shows that, for all the items, the probability of a positive response increases with the general attitude score. The positive association between the two items in Table 2 is therefore accounted for by the fact that they are both indicators of an underlying attitude or, more precisely, the probability of positive responses is positively related to the general attitude score. The resulting association in Table 3 is noticeable but not very strong. Therefore we increase the marginal differences between the three homogeneous classes and mix again. Now the resulting

TABLE 4

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association is much more marked. How is this finding to be explained in the light of the previous discussion? It will be remembered that the marginals in the partial subtables correspond to the "item curves" of the two items. In Table 4 they are clearly much steeper than in Table 3. This means that now the two items have a much stronger relation to the underlying continuum than before. As a result the interrelation between the two indicators on the right side of Table 4 is much stronger than in Table 3.⁴

So far our emphasis has been on the mixing of homogeneous subgroups and the resulting associations between indicators. But Tables 1,

⁴ The reader should satisfy himself that many other combinations could occur. Suppose, e.g., that we made Class III in Tables 3 and 4 much larger than Class I. Then, in the resulting fourfold table, both items and their joint occurrence would show higher frequencies.

3, and 4 can also be read in the opposite direction, from right to left. We then start with an existing association between indicators; we “unmix” the population under study and end by showing the homogeneous subgroups in which the associations disappear. Actually, this is always done if, in empirical research, a statistical finding is to be explained. We want to remind the reader of the three major types of such explanations.

Type 1. A good example is available from political research. In a presidential election educated people vote more frequently than the uneducated. We can classify people into three groups, however, according to their interest in elections. Then on each interest level we can set up a fourfold table between voting and education (graduation from high school forming the point where higher education begins). We then find that with each increase in interest the proportion of voters increases, as well as the proportion of people having higher education. *Within* interest groups, however, there is practically no relation between education and voting. Interest, therefore, accounts for the original relation in terms of what is usually called an “intervening” variable. The whole structure can be represented by the following scheme where arrows stand for a vague idea of causation.

Education → Interest → Voting

The original two variables are underscored. Their association is *interpreted* through the role of “interest.” The interpretation is tested by showing that the original association disappears within subgroups which are homogeneous in regard to interest.⁵

Type 2. The second major type of accounting is usually known as the controlling of *spurious factors*.

Examples are almost proverbial: fires where many fire engines come out cause more damage; does this mean that fire engines are dangerous? Obviously not. Large fires bring out many engines and cause much damage. The arrow scheme corresponding to this case would be as follows:



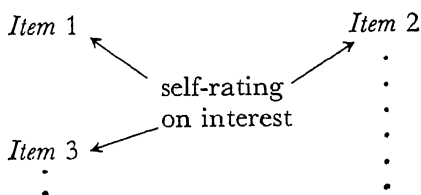
⁵ Cf. [15]. There a characteristic counterexample is included. Men vote more frequently than women. This cannot be accounted for by interest. Even *within* the same interest group men vote more than women.

If the size of the fire is "kept constant" there would be no positive relation between equipment and damage.

In both types of accounting the statistical test is the same, and it is the one which we have carefully analyzed above. The ultimate relation between two attributes is owing to the fact that they are both related to a third property; once this property is kept constant the original relation disappears. The difference between type 1 and type 2 lies in the sequence of variables involved. In both cases we start with an association between two factors: education and voting in the first example, equipment and damage in the second. But in type 1 the explanatory factor intervenes between the two original variables; whereas in type 2 it antecedes the damage as well as the number of engines the fire brings out.

Type 3. The third type of accounting is usually less discussed because its outcome seems so obvious from a substantive point of view. Still it is the most important one for the present purpose, and all our initial examples belong here. When we deal with indicators of a supposed underlying property, there exists no necessary time relation between the intended classification and its overt manifestations. Rather the relation here is one of generality and specificity. Still, the test of whether we are really dealing with appropriate indicators is the same as before; we want to know *whether the underlying property does account for the interrelation between the manifest indicators*. We would look for a way to classify people according to the underlying characteristic and assume that if this is held constant no further statistical relation should exist between the various indicators.

The most obvious way to make this test is to see whether people who are alike on the majority of the indicators show any appreciable relation between the remaining ones. This we did with our oil study example. Another approach would be to use a rating scale. For instance, people could be asked to rate their political interest on a scale from 1 to 10. Then they could be divided into fairly homogeneous classes according to this self-rating. If we then have an itemized interest test, we could raise this question: does the self-rating account for the interrelation between the test items? The answer would be in the positive if, on each level of self-rating, the items were not statistically associated. The arrow scheme corresponding to that of the previous example would be:



It is hoped that we have given enough examples so that the reader has a clear picture of the following two related facts:

1. There exists a uniform operation of accounting for an empirical relation between two properties. It consists of studying this relation for subclasses of the original population, these subclasses being formed by the introduction of additional properties. The substantive nature of these "accounting properties" and their relation with the original data make for the main types of accounting procedures as they occur in the practice of research.

2. These various accounting procedures are all in fact inversions of the "mixture phenomenon" described above. In mixtures of homogeneous groups, indicators show statistical associations: they are due to the covariation of the indicators *between* these subclasses. Inversely, associations between indicators in empirical populations can be accounted for by dividing them into homogeneous subgroups; the variables along which this "unmixing" can be done "explain" the statistical associations originally found.

We can now relate all these considerations to latent structure analysis.

The accounting equations and the principle of local independence. We are prepared to answer the question which we first raised at the end of the historical survey in the first section. There we came to the conclusion that the problem of disposition concepts boiled down to the task of *relating a manifest to a latent property space*. The manifest space was given by the observed properties of our objects, which for our present purpose, we have reduced to dichotomies. We came to use the word "item" for an indicator and the term "response" for its observed presence or absence. A "response pattern" was a point in such a dichotomous property space.

The latent space corresponds to our intended classification, which, as we saw, was variously called in the literature "an underlying characteristic," "a trait," "a disposition," etc. It was not necessary to assume anything about this latent space; the number of its dimensions was not specified, nor did they need to be of any particular mathematical form, dichotomous, continuous, or whatever. But when we came to this point of our discussion we left it undecided how we would achieve this latent classification, in view of the fact that we have only manifest observations available. What solution does latent structure analysis propose for this problem?

It defines the latent space as that classification which accounts for the statistical interrelations between the manifest observed indicators. It is the classification which "unmixes" a given population into homogeneous subgroups. The many consequences of this definition must now be spelled out.

The latent space is not known in advance but *is defined by its accounting role*. When we discussed examples of item curves, we first divided people by some index (e.g., a rating on the number of positive responses) into fairly homogeneous classes and then studied empirically how the indicators were related in the various subclasses; Table 1 was a typical example. But *in latent structure analysis we do not have an empirically provided general classification against which the occurrence frequencies or probabilities of the different items can be plotted. The underlying classification is derived from the statistical behavior of the indicators themselves*. The sequence of affirmative response proportions for a given item over all homogeneous "latent" classes becomes its item curve; it is now called a trace line to stress that it is not directly given but derived from empirical data.

Accounting equations. Let us assume that we have c homogeneous classes and n items. Let us assume further that these homogeneous classes are ordered in some way. (In Table 3, for instance $c = 3$, and the three classes are ordered from left to right.) Let us now focus on two specific items, say, the two items in the scheme just mentioned. The response frequencies of the two items in the composite population (exemplified by the right side of Table 3) can be derived from the following equations:

$$p_1 = \sum_{x=1}^c v^x p_1^x$$

$$p_2 = \sum_{x=1}^c v^x p_2^x$$

$$p_{12} = \sum_{x=1}^c v^x p_1^x p_2^x$$

This is in algebraic form the box-by-box summation we have carried out in all our mixing examples. The superscript in p_1^x shows from what class x the "latent probabilities" have been taken; v^x is the proportion of the whole population in class x . Suppose now the joint frequencies on the left were given and the task consisted in computing the response frequencies in the homogeneous subclasses. We could not solve it because there are fewer equations than unknowns. Obviously, however, we could add more equations by adding more items. Not only would that give us more equations of the type just mentioned, but it would also add an additional type: we could now set up equations for higher-order frequencies, for instance,

$$p_{123} = \sum_{x=1}^c v^x p_1^x p_2^x p_3^x$$

As we will see later, in general there always occurs a point where we have enough equations to solve the whole problem. Equations of this type are called *accounting equations* because they permit us to derive parameters of the latent structure from the manifest data. These accounting equations are, in a way, the mathematical summary of everything we have said so far. Let us review, therefore, how they are related to the different elements of our discussion.

First, they formalize algebraically the diagnostic procedure by which we make the inference from the manifest data to the latent position of a respondent. An indicator or test item is introduced because we have a more or less vague idea how it is likely to be related to what we want to find out about each of our respondents. We assume that if we could by some manipulation put people into various positions of this intended classification, their response probability would vary according to this general image. The latent probabilities give precise expression to the relation between the latent and the manifest space. They are tantamount to a diagnosis for any manifest response pattern which might be empirically observed.

But, of course, we cannot move respondents into various positions; we observe each respondent only once. Instead, we have a variety of respondents whom we assume—in the spirit of the whole model—to be actually at different places in the latent structure. As a matter of fact, we make an even stronger assumption: for the purpose of our model, we assume that all our respondents are alike but for one fact, that they are different in regard to the latent property. This is the second element in the whole analysis.

A third element is the following trend of reasoning. Even if we knew where a respondent belongs in the latent space, we would have to make him respond repeatedly to each item so that we could ascertain empirically his response probability. But again we must remember that each respondent is observed only once on each of the items. This difficulty is surmounted by the idea that in an empirical population we are most likely to have many people who are at the same point in the latent space. Now we consider such respondents to be, for our purpose, identical; therefore, the proportion of affirmative answers in such a homogeneous group can be taken to be the same as we would have obtained if we had observed one member of each of these groups repeatedly.

Let us get this series of constructions clearly in our minds by visualizing the process in reverse order. We could get all our trace lines by the following procedure:

1. We imbue one respondent, by some kind of manipulation, with various amounts of the latent property.

2. At each point, we make him respond to each item repeatedly with "brainwashing" inserted between any two trials. This would give us, at each point of the latent space, the probability of an affirmative response for our "typical" subjects.

3. The totality of these probabilities, attached to each point of the latent space, would be the trace of an item—in the one-dimensional case, the trace line.

Now the steps (1) and (2) are replaced by the fact that at each point of the latent space we have many respondents; we substitute their response frequencies for the probabilities we are looking for. But remember that even this is a fiction. Although we are convinced that our whole population can be subclassified into such homogeneous groups, when we deal with concrete respondents we do not know at what point of the latent space they are. Here we take advantage of the accounting equations just explained and developed. What we actually know are the response frequencies of a mixed population to a number of items in all their combinations. Therefrom we can compute the response probabilities in the postulated homogeneous subclasses. As a matter of fact, we learn, as a by-product of this computation, what proportion of our respondents is in each of these latent classes.

But notice that even now there is one topic which we have not discussed at all, namely, the single respondent. We do not know at which point of the latent space he is located. This is a matter which we will take up only in the next section. What we now know is the latent structure, the proportion of people in each class and the conditional probabilities of giving an affirmative response to each item in these classes.

This whole web of assumptions and deductions can be fruitfully divided into three sections. One has to do with rather conventional ideas which are accepted wherever probability notions are introduced; the idea, for instance, that for the purpose of a specific investigation different people can be considered as alike, and that the proportion having a property is an estimate of the probability that a single one of them will exhibit it. Although the logical foundations of this idea are by no means simple, we need not justify them here because of their general acceptance in all model building.

A second group of our ideas has to do with the problem of unmixing: deriving the probabilities in homogeneous subclasses from the response frequencies of a mixed population. This is straightforward algebra and does not require any further logical foundation. Actually, it is the most characteristic and novel aspect of latent structure analysis, and much of the rest of our monograph will elaborate on it. Here the accounting equations come in.

Finally, we have the principle of *local independence*. (The term has been suggested by Frederick Mosteller.) It covers the phase of our discussion in which an intended classification (an underlying, intervening variable) is *defined* as the one which divides a given population into homogeneous subgroups. The principle of local independence identifies the "measurement" problem with the mixing phenomenon or, rather, its inversion—unmixing. For this, no further foundation can be introduced. The principle is proposed as a mathematical axiom which formalizes the basic assumption of what we have called index formation in the social sciences. If an investigator chooses a number of indicators for the purpose of diagnosis, for the purpose of putting people or social objects into an intended classification, he does assume—knowingly or not—that the statistical relations between these indicators are essentially owing to the fact that they are all related to the intended latent property. For a group of people, therefore, who are alike in regard to this latent property, all the indicators will be statistically unrelated. This principle partakes of the common characteristics of all axioms which are introduced into a theory—and no theory exists without at least one axiom: if, after all proper consideration, some specific empirical data seem to contradict the axiom, then the investigator will decide that "there is something wrong about the data" and will maintain the axiom. This formulation, put by purpose in an almost paradoxical form, will be carefully amplified in subsequent sections.⁶

The idea of making the principle of local independence the nub of index construction, even of concept formation in the social sciences, is the central logical feature of latent structure analysis. Together with conventional probability notions and some newly developed but quite orthodox algebra, all procedures and all empirical findings derive from it.

We shall now present the main steps in an actual, relatively simple application.

III. THE NINE STEPS OF LATENT STRUCTURE ANALYSIS

A latent structure analysis of necessity involves a certain sequence of operations which can be cast into a schedule of nine steps.

Summary of the nine steps. First we must think about the form of models which might reasonably be appropriate. This means that we want to consider systems of manifest and latent variables such that their interrelations mirror the interrelations between indicators in the

⁶ Frederic Lord in discussing the principle of local independence has aptly stated that it is "almost indispensable for any theory of measurement." Cf. [17].

data and the concept which is the real object of concern. Having considered these questions in a general way, we must state our assumptions in explicit mathematical form (step 1). Then we can write the accounting equations, which give the relations between manifest and latent parameters, in the particular form which the chosen model imposes on them (step 2). Next we must ask what conditions or restrictions are put on the interrelations within the data by the assumptions of the model (step 3). These "conditions of reducibility" are useful in a number of ways: first, they are explicit statements of relations which must hold among the manifest parameters, so that by means of simple operations on the data, and without solving the accounting equations, we can determine whether the assumed model is appropriate for the given data. Second, the conditions of reducibility are useful in evaluating how closely the data are in accord with the requirements of the model. Third, the conditions of reducibility contribute to an understanding of the model and to the question of the solvability of the accounting equations.

This question of the solvability of the accounting equations may be asked more specifically in the form: given the manifest parameters, are there a sufficient number of conditions imposed by the model to make it possible to identify the latent parameters? (step 4) Having answered this question, we proceed to its logical corollary: if the equations are solvable, how does one actually solve them? (step 5)

Up to this point everything is algebra. Now the data must be introduced, and we are forced to do some arithmetic. A "fitting procedure" (step 6) in latent structure analysis is usually a shuttling back and forth between data and latent parameters—using data of lower order to identify certain latent parameters, from these computing what the data would have been if they had fitted the (partially identified) model perfectly, then combining these "fitted data" with higher-order manifest data to compute further latent parameters. A fitting procedure has two goals: (a) a set of latent parameters and (b) a set of "fitted manifest parameters" which are perfectly in agreement with the demands of the model and at the same time are as close as possible to the actual data. How close a fit was achieved requires some evaluation (step 7). Two questions must be answered here: Are the differences between the actual and the fitted parameters small enough? Do the differences appear to be randomly distributed, or do they fall into some pattern which suggests that a somewhat different latent structure model would be more appropriate? If this is the case, we must again start from scratch, except for what we have learned by the experience. But if we are satisfied with the fit, there is still some work to be done.

We want to know how the respondents who give a particular response pattern are distributed over the latent space (step 8). And it is of interest to ask about the most likely or the most typical location of individuals who gave a particular response pattern, and in some way to assign a *score* to each respondent, or to each response pattern. Similarly we may ask how much each item contributes to the diagnostic process, and perhaps we may wish to give each item a score indicating its ability to discriminate between individuals at different points of the latent space (step 9).

For illustrative purposes we shall, throughout our description of the nine steps, make use of one simple latent structure model, the so-called linear traceline model.

In order to illustrate what we have to say with concrete numerical data, we have taken six items which were included in a questionnaire because it was thought that they would serve as indicators for the concept of "job satisfaction." These questions were answered by 876 employees of a large industrial concern. Note that with each question is given a definition of what is considered a positive response, and the proportion p_i of all respondents giving this response.

Question	Positive response	p_i
1. "Are there any things about your job that you particularly like?"	"A lot of things"	.34
2. "Are there any things about your job that you particularly dislike?"	"None" and "not many"	.57
3. "How often do you look forward with some pleasure to your day on the job?"	"Every day" and "almost every day"	.62
4. "If someone asked you about getting a job like yours, which of the following would you be inclined to do? Encourage her? Discourage her? Neither?"	"Encourage her"	.48
5. "Do you ever feel you would like to quit and get a job with some other company?"	"Never"	.38
6. "Do you feel that you would like to get a transfer from your present job to some other kind of work in your department?"	"Seldom" and "never"	.58

We are restricting our discussion to these six items for the sake of simplicity—by means of them we can illustrate quite well what we want to present. Indeed, when four items suffice to illustrate a point, we shall use only four. In the actual questionnaire, however, there were more items tapping the notion of "job satisfaction."

Step 1: Choice and specification of the model. The first problem to be faced is the choice of a specific model. In our example we are

assuming that job satisfaction is a single dimension. The six questions just quoted are the indicators for the latent continuum. In this special case the probability of an affirmative response to each item is proportional to the degree of "underlying satisfaction." This means mathematically that all the trace lines are linear functions

$$f_i(x) = a_i^0 + a_i^1x \quad (1)$$

The two parameters of such a trace line correspond to different elements in the content of a questionnaire item. a_i^1 , which is the slope of the trace line, indicates something like the discriminating power of the item. If the slope of the trace line is steep, then a small increase in job satisfaction will lead to a considerable increase in the probability of an affirmative answer. If the trace line is flat, then the answer to an item is not very indicative of the latent variable. We will see, for instance, that this is the case for question 4. Maybe people do not ask for transfers even if they are not very satisfied with their jobs because they feel that there is not much difference among the various jobs they have a chance to get.

The coefficient a_i^0 corresponds to the probability that a question is answered affirmatively irrespective of a respondent's job satisfaction.

What about the distribution of the population over the latent space? For the model at hand, very little need be said about it. Indeed, we shall put no restrictions on this distribution except one which is implied by the form of our trace lines and the fact that they represent probabilities. Probabilities cannot assume values greater than 1 or less than 0. But any straight line will eventually escape from these bounds, unless it is horizontal. Consequently, we must rely on the distribution function to make sure that whenever a trace line is larger than 1 or less than 0, there is "nobody at home," the density function $\phi(x)$ is 0.

The choice of the model then expresses in mathematical form certain substantive notions which the investigator had in mind when he collected his data. Whether his expectations are justified will only be known in step 7.

Step 2: Accounting equations specialized for the model. We have so far talked about the equations relating manifest and latent variables only in general form. When we now bring the specification made in step 1 to the accounting equations, the first-order accounting equations for the linear traceline model come to be

$$\begin{aligned} p_i &= \int_{-\infty}^{\infty} f(x)\phi(x) dx = \int_{-\infty}^{\infty} (a_i^0 + a_i^1x)\phi(x) dx \\ &= a_i^0 \int_{-\infty}^{\infty} \phi(x) dx + a_i^1 \int_{-\infty}^{\infty} x\phi(x) dx \\ &= a_i^0 + a_i^1M_1 \end{aligned} \quad (2)$$

$\int_{-\infty}^{\infty} \phi(x) dx = 1$ by definition, since $\phi(x)$ is a probability density function. M_k , the k th moment of a distribution, is defined as

$$M_k = \int_{-\infty}^{\infty} x^k \phi(x) dx$$

where the first moment is the ordinary mean of the distribution.

For the second-order accounting equations we have

$$\begin{aligned} p_{ij} &= \int_{-\infty}^{\infty} (a_i^0 + a_i^1 x)(a_j^0 + a_j^1 x)\phi(x) dx \\ &= a_i^0 a_j^0 + (a_i^0 a_j^1 + a_i^1 a_j^0)M_1 + a_i^1 a_j^1 M_2 \\ &= a_{ij}^0 + a_{ij}^1 M_1 + a_{ij}^2 M_2 \end{aligned} \quad (3)$$

The last line introduces a convenient shorthand notation. The elements a_{ij}^k arise in the multiplication of polynomials as the coefficients of the k th power of x . The symbolism is easy to understand: the subscripts indicate which items are involved; the superscript is the power of x , or the order of the moment, to which the coefficient is attached. These coefficients, also called "convolutions," make possible even greater economies in notation for the accounting equations for higher-order frequencies. These accounting equations are similar to Eqs. (2) and (3), the integrands being, however, products of larger numbers of linear expressions of the form (1). Thus the accounting equations for third-order frequencies are obtained by the integration of a product of three linear expressions, giving rise to so-called third-order convolutions as the coefficients of the moments:

$$p_{ijk} = a_{ijk}^0 + a_{ijk}^1 M_1 + a_{ijk}^2 M_2 + a_{ijk}^3 M_3 \quad (4)$$

The reader will have no difficulty in verifying that, for instance,

$$a_{ijk}^2 = a_i^1 a_j^1 a_k^0 + a_i^1 a_j^0 a_k^1 + a_i^0 a_j^1 a_k^1 \quad (4a)$$

The right side of (4a) is the sum of all possible products involving one coefficient from each of the three items i , j , and k , the sum of superscripts in each term being equal to the superscript of the term on the left. This may be considered a defining property of the convolutions; but we cannot here enter a detailed discussion of this intrinsically interesting topic [7]. Note that the third moment M_3 occurs in the accounting equation for third-order frequencies. For the linear trace-line model this is a general situation: the accounting equations for a manifest frequency of order n involve all moments up to the n th.

If we deal with m items, we have to write 2^m accounting equations. A very important condensation can be achieved if matrix algebra is used. Let us take as an example the second-order frequencies. The

proportion of joint affirmative answers to items i and j can be written in the form [1]

$$p_{ij} = (a_i^0 a_i^1) \begin{pmatrix} 1 & M_1 \\ M_1 & M_2 \end{pmatrix} \begin{pmatrix} a_j^0 \\ a_j^1 \end{pmatrix} \quad (5)$$

All the manifest second-order frequencies can be put in the form of a matrix P as follows:

$$P = \begin{pmatrix} - & p_{12} & p_{13} & \dots & p_{1m} \\ p_{21} & - & p_{23} & \dots & p_{2m} \\ \dots & \dots & \dots & \dots & \dots \\ p_{m1} & p_{m2} & p_{m3} & \dots & - \end{pmatrix} \quad (6)$$

The accounting equations for all the manifest frequencies in the matrix P can be combined in one matrix equation. We introduce two more matrices. The one consists of moments of the latent distribution function, and the other consists of the coefficients of all the trace lines. The first—the so-called moment matrix M —has already been exhibited on the right side of Eq. (5). The matrix of the latent traceline coefficients is defined as follows:

$$\Lambda = \begin{pmatrix} a_1^0 & a_2^0 & a_3^0 & \dots & a_m^0 \\ a_1^1 & a_2^1 & a_3^1 & \dots & a_m^1 \end{pmatrix} \quad (7)$$

All the accounting equations of the second order then can be written as

$$P = \Lambda' M \Lambda \quad (8)$$

where Λ' is the transpose of Λ .

Similar equations can be developed for higher-order frequencies. As an example we present the accounting equations for the third-order frequencies in matrix form. The accounting equation for the third-order frequency p_{ijk} can be put in a form similar to Eq. (5), namely,

$$p_{ijk} = (a_i^0 a_i^1) \begin{pmatrix} 1 & M_1 & M_2 \\ M_1 & M_2 & M_3 \end{pmatrix} \begin{pmatrix} a_k^0 & 0 \\ a_k^1 & a_k^0 \\ 0 & a_k^1 \end{pmatrix} \begin{pmatrix} a_j^0 \\ a_j^1 \end{pmatrix} \quad (9)$$

How can third-order frequencies be combined into matrices? There are a variety of answers to this question. For our present purpose the best way is to consider a selection of third-order data in a stratified matrix (10). It comes about by attaching to the entries of P —see Eq. (6)—one additional index, the so-called stratifier. This gives us:

$$P_k = \begin{pmatrix} - & p_{12k} & p_{13k} & \dots & p_{1mk} \\ p_{21k} & - & p_{23k} & \dots & p_{2mk} \\ \dots & \dots & \dots & \dots & \dots \\ p_{m1k} & \dots & \dots & \dots & - \end{pmatrix} \quad (10)$$

Now we want an equation like Eq. (8) which shows how the manifest matrix P_k looks in terms of latent parameters. This time our departure is Eq. (9). It is easy to verify that

$$P_k = \Lambda' \begin{pmatrix} 1 & M_1 & M_2 \\ M_1 & M_2 & M_3 \end{pmatrix} \begin{pmatrix} a_k^0 & 0 \\ a_k^1 & a_k^0 \\ 0 & a_k^1 \end{pmatrix} \Lambda \quad (11)$$

The reader who is not well acquainted with the shorthand value of matrix equations can get the gist of the story merely by inspecting Eqs. (5) and (9). Equation (8) is really nothing more than Eq. (5) for all combinations of two items, and Eq. (11) is a composite of many equations like Eq. (9). We dispense for the moment with carrying the story on to higher-order frequencies and turn to the main problem, the solution of Eqs. (8) and (11). This, and the corresponding problem of other models, has so far been the central concern of latent structure investigations. The next four steps are devoted to it.

Step 3: The conditions of reducibility. From the previous step the following observation can be made. For m items, there are $3m$ latent parameters, two coefficients for each of the m trace lines and one moment for each of the m frequency levels which can be formed. But we have 2^m manifest data. Thus as the number of items increases we will have many more equations than unknowns. This means that the model imposes restrictions on the manifest frequencies. One could say that each model determines the morphology of the dichotomous system which it generates.

The third step in the latent structure analysis consists in studying the dependencies which exist between the manifest frequencies according to the special nature of the model.

These restrictions are called conditions of reducibility, for the accounting equations can be solved only if these conditions are met; or to put it differently, a dichotomous system to be reduced to a system of homogeneous classes in accordance with a special latent structure model has to satisfy these conditions. The difficulty at this step derives from the fact that no general rules for the finding of these conditions can be established. Each model has to be investigated separately, and one usually ends up with a large number of conditions. It is much more difficult to say what minimum set of conditions is sufficient to determine all others. As a matter of fact, we will not tackle this problem at all here, though in the subsequent comments the answer will be provided for the particular model under discussion.

One general lead can be given as to the nature of these conditions of reducibility. They usually consist of quite complex combinations of manifest data which on the latent side, however, are much

simpler than the original accounting equations. Let us take as a first example such a combination which can be formed within the matrix P of Eq. (6).

We select out of this matrix a special matrix by picking any two rows and any two columns, but the selection is made so that the column numbers are all different from the row numbers. To be specific, we might pick the first two rows and the third and fourth columns. We then form a matrix of order 3×3 by adding a first row and a first column which consist of marginals corresponding to the rows and columns we have just selected. If we put 1 in the upper left corner, we get the following form:

$$B = \begin{pmatrix} 1 & p_3 & p_4 \\ p_1 & p_{13} & p_{14} \\ p_2 & p_{23} & p_{24} \end{pmatrix} \quad (12)$$

We will say that the *bordered matrix* B has a *vertical signature* 1,2 and a *horizontal signature* 3,4. It can be seen from the formation of such matrices that we never have to worry about the missing diagonal entries of the original matrix P . We can form as many such specific matrices as there are combinations of four items in our reservoir of items; in addition, from each such matrix as the one shown in Eq. (12), we can always derive one essentially different matrix by exchanging the identifying indices of one row and one column.

How does such a combination of manifest data look on the latent side? The relation is very similar to Eq. (8); only we have now to deal with two different matrices Λ , one corresponding to each of the two signatures. They are defined in Eq. (13).

$$\Lambda_V = \begin{pmatrix} 1 & a_1^0 & a_2^0 \\ 0 & a_1^1 & a_2^1 \end{pmatrix} \quad \Lambda_H = \begin{pmatrix} 1 & a_3^0 & a_4^0 \\ 0 & a_3^1 & a_4^1 \end{pmatrix} \quad (13)$$

The accounting equation for the matrix B reads as follows:

$$B = \Lambda_V' M \Lambda_H = \begin{pmatrix} 1 & 0 \\ a_1^0 & a_1^1 \\ a_2^0 & a_2^1 \end{pmatrix} \begin{pmatrix} 1 & M^1 \\ M_1 & M_2 \end{pmatrix} \begin{pmatrix} 1 & a_3^0 & a_4^0 \\ 0 & a_3^1 & a_4^1 \end{pmatrix} \quad (14)$$

From the expression on the right we know immediately that the determinant of B must be zero. This follows from some theorems of elementary matrix theory which may, for our particular case, be stated as follows: If a matrix W is equal to the matrix product UV , then the largest square submatrix of W with non-zero determinant cannot be larger (i.e., cannot have more rows and columns) than the largest square submatrix with non-zero determinant contained in either of

the matrices U or V . Since none of the three matrices on the right of Eq. (14) is large enough to contain a square submatrix with more than two rows and columns, the matrix B on the left cannot possibly contain a non-zero determinant of order greater than two. Thus one condition of reducibility is clearly that any bordered 3×3 determinant which can be formed from the full matrix of the second-order joint frequencies vanishes. It can be shown in a similar way that for the stratified matrix P_k the same conditions hold.

In addition to these conditions, which hold for each level of stratification, one can also deduce another set of conditions which prevail between different levels of data. This can be shown by forming determinants of order 2×2 taken from bordered matrices P and P_k as defined in the previous section, Eqs. (6) and (10). We form submatrices by picking just one row and one column bordered by marginals as before. Choosing for example row 1 and column 2, we get now from P the form:

$$\begin{pmatrix} 1 & p_2 \\ p_1 & p_{12} \end{pmatrix} = \begin{pmatrix} 1 & 0 \\ a_1^0 & a_1^1 \end{pmatrix} \begin{pmatrix} 1 & M_1 \\ M_1 & M_2 \end{pmatrix} \begin{pmatrix} 1 & a_2^0 \\ 0 & a_2^1 \end{pmatrix} \quad (15)$$

Similarly we get from P_k

$$\begin{pmatrix} p_k & p_{2k} \\ p_{1k} & p_{12k} \end{pmatrix} = \begin{pmatrix} 1 & 0 \\ a_1^0 & a_1^1 \end{pmatrix} \begin{pmatrix} 1 & M_1 & M_2 \\ M_1 & M_2 & M_3 \end{pmatrix} \begin{pmatrix} a_k^0 & 0 \\ a_k^1 & a_k^0 \\ 0 & a_k^1 \end{pmatrix} \begin{pmatrix} 1 & a_2^0 \\ 0 & a_2^1 \end{pmatrix} \quad (16)$$

Before going on we shall anticipate one point which will be discussed in more detail in the next step. It is not possible to develop a complete metric in the latent space. The zero point of the latent continuum and its unit of measurement remain unidentifiable. We deal with measurements similar to the temperature scale, where only ratios of "distances" have an intrinsic meaning. This means that without loss of generality, we can fix the values of two moments; we might as well simplify our computations by making $M_1 = 0$ and $M_2 = 1$. The average position of the population is then at the origin of the coordinate system and the standard deviation of the population distribution becomes the unit of measurement. As a result the determinantal equations corresponding to Eqs. (15) and (16) acquire an especially simple form. Using an obvious symbolism for their left sides, the right sides become:

$$[12] = a_1^1 a_2^1 \quad (15a)$$

$$[12;k] = a_1^1 a_2^1 [(a_k^0)^2 - (a_k^1)^2 + a_k^0 a_k^1 M_3] \quad (16a)$$

Forming the ratio of these two equations we find

$$\frac{[12;k]}{[12]} = (a_k^0)^2 - (a_k^1)^2 + a_k^0 a_k^1 M_3 \quad (17)$$

The essential feature of Eq. (17) lies in the fact that the latent form on the right side contains only moments of the distribution function and the traseline coefficients of the stratifying item. If on the left side we had chosen any combination of items other than 1 and 2, the ratio would still remain the same as long as item k remains the stratifier. Once the frequencies on one level are fixed, there is little freedom left in this model for frequencies on another level because there exists a kind of proportionality between levels. It is easily shown that conditions similar to Eq. (17) exist between any two different frequency levels.

Step 4: Identifiability. Our next concern is whether the data of our dichotomous system are sufficient to fix the values of the latent parameters. From the previous step we know that it is obviously not enough to count whether we have more equations than unknowns. The conditions of reducibility have shown that in this (as in any other) model, many of the manifest data are derived from others and therefore only a portion of the accounting equations can be independent.

The accounting equations, like (2) and (3), contain definite integrals. These do not change in value under a large class of transformations of the x axis. However, if we want to maintain the linearity of the trace lines, then only *linear* transformations affect neither the model nor the data generated by it. But the fact that linear transformations of the latent continuum do not affect the observable consequences of the model implies that we will be able to identify the whole latent structure only up to a linear transformation of the latent continuum. For instance, if we fix the coefficients of one trace line arbitrarily, the rest of the structure would be fixed.

Alternatively we can choose an arbitrary zero point and an arbitrary unit for the latent variable x . For a variety of reasons, it is in general preferable to make the second choice. We set $M_1 = 0$, $M_2 = 1$. The implications of this in terms of traditional measurement theory were explained in the previous step.

We have, so far, only shown that two latent parameters must be arbitrary. We have not proved that all the others are identifiable. This will be obvious as a result of the next step.

One should not confuse the problem of identifiability with the question of whether enough items are available in a specific research problem. The origin and the unit of measurement for the latent continuum cannot be found in this model irrespective of how many items

are added. On the other hand, if we are interested in getting an additional number of higher moments we can always find them by adding additional items to our reservoir of manifest data. In some latent structure models it is not at all easy to find out how many items are needed to identify a latent structure, or to identify it up to a given point. In the present model it is very simple. As we shall see from step 5, the coefficients of the trace lines can be found by using only data of the first and second order. As we want to find more moments, we have to move to ever higher frequency levels.

Step 5: Identification. The problem of solving the accounting equations is somewhat similar to the task of finding the conditions of reducibility. It again requires that we think creatively of combinations of manifest data which, however complex on the manifest side, become more simple in their latent form.

The traceline coefficients are very easily found by going back to Eqs. (2) and (15). Let us remember that we have given the first two moments arbitrary values: $M_1 = 0$, $M_2 = 1$. This puts Eq. (2) into very simple form:

$$p_i = a_i^0 \quad (18)$$

In other words, the position of the trace line, its intercept with the y axis, is immediately given by the marginal of the corresponding item. To get the slope of the trace line we turn to Eq. (15). Forming determinants, we get

$$\begin{vmatrix} 1 & p_j \\ p_i & p_{ij} \end{vmatrix} = p_{ij} - p_i p_j = [ij] = \begin{vmatrix} 1 & 0 \\ a_i^0 & a_i^1 \end{vmatrix} \begin{vmatrix} 1 & 0 \\ 0 & 1 \end{vmatrix} \begin{vmatrix} 1 & a_j^0 \\ 0 & a_j^1 \end{vmatrix} = a_i^1 a_j^1 \quad (19)$$

Whereas in Eq. (15) we used two specific items, 1 and 2, we now write the result in general form for any two items, i and j . The symbol introduced in the middle of Eq. (19) is a convenient representation for the cross product, which occurred already in Eq. (15a).

Thus it turns out that the manifest cross product between two items is the product of the slope coefficients of the two corresponding trace lines.

How would we get this coefficient for the single item, say i ? We need two auxiliary items, b and c . Then we have the answer in the equation:

$$a_i^1 = \sqrt{\frac{[ib][ic]}{[bc]}} \quad (20)$$

It should be noted that Eq. (19) implies that it makes no difference which two auxiliary items b and c we use. (Indeed, this is a condition of reducibility.) We shall see in step 6 that when we deal with actual data the situation is somewhat different.

At this point we have in principle identified the two traseline coefficients for all items. How about the moments? The third moment can obviously be obtained by making the same substitutions in Eq. (16) as we made in Eq. (15). Without going into details, we write the result in Eq. (21).

$$M_3 = \frac{[ij;k]}{[ij]p_k a_k^1} - \frac{p_k}{a_k^1} + \frac{a_k^1}{p_k} \tag{21}$$

Recall that a_k^1 is already known from Eq. (20).

When it comes to the fourth moment, a new idea is introduced. It can be shown that none of the manifest forms we have mentioned so far would ever help us to compute a fourth moment. We have to discover a new combination of manifest data for this purpose. It turns out that so-called ascending matrices are the appropriate device. In these the signatures contain elements of different orders, e.g.,

$$A = \begin{pmatrix} 1 & p_3 & p_{34} \\ p_1 & p_{13} & p_{134} \\ p_{12} & p_{123} & p_{1234} \end{pmatrix} \tag{22}$$

When more than four items are under consideration, stratified ascending matrices also play a role. We can write a matrix equation which contains the accounting equations for all the elements in the ascending matrix A in the form

$$A = (\Lambda_v^*)' M_{3 \times 3} \Lambda_H^* \tag{23}$$

where

$$M_{3 \times 3} = \begin{pmatrix} M_0 & M_1 & M_2 \\ M_1 & M_2 & M_3 \\ M_2 & M_3 & M_4 \end{pmatrix} \tag{24}$$

and

$$\Lambda_v^* = \begin{pmatrix} 1 & a_1^0 & a_{12}^0 \\ 0 & a_1^1 & a_{12}^1 \\ 0 & 0 & a_{12}^2 \end{pmatrix} \quad \text{and similarly for } \Lambda_H^* \tag{25}$$

The elements a_i^k are the traseline coefficients of the k th power of x for the i th item. The elements a_{ij}^k are the convolutions which we have encountered already in Eq. (3). We shall not go through the details of demonstrating Eq. (23); the reader will have no difficulty convincing himself of the truth of it by carrying out the necessary matrix multiplication.

Again by taking determinants, this time on both sides of Eq. (23), and then simplifying, we obtain for the fourth moment the formula

$$M_4 = 1 + M_3^2 + \frac{A}{[12][13][34]} \tag{26}$$

The fifth moment can be obtained by stratifying the ascending matrix we have just introduced. The sixth moment requires an ascending matrix with one more row and one more column, and so on.

As we mentioned before, the higher moments we want, the more items we need. In our example we will be satisfied with the fifth moment.

So far we have dealt with purely algebraic problems. We assumed that the manifest data were generated by the model under investigation. We asked ourselves then how we could, so to speak, rediscover the parameters of the model if we were only presented with "perfect" manifest data. In actual research practice, of course, these data are at least subject to sampling variations. Besides, most models under investigation cannot be expected to be more than a rough approximation of whatever the "true" latent structure might be. The next step requires dealing with empirical data.

Step 6: Computation: The fitting procedure. In the last section we discussed the identification problem—the problem of solving the accounting equations when the datum fits the model exactly, when it is of the form which would be generated by the assumed model when the sample size approaches infinity. With empirical data the situation is never so clear. In empirical work it is from data beclouded by sampling variability that we have to find the latent parameters. The computation for the first traseline parameter a_i^0 is no more complicated than the formula (18) obtained in the identification process indicates. They are simply equal to the manifest marginals, $p_i = a_i^0$:

$$a_1^0 = .34 \quad a_2^0 = .57 \quad a_3^0 = .62 \quad a_4^0 = .48 \quad a_5^0 = .38 \quad a_6^0 = .58$$

The second traseline parameters a_i^1 are a little more troublesome. From Eq. (19)

$$[ij] = a_i^1 a_j^1$$

we know that

$$(a_i^1)^2 = \frac{[ib][ic]}{[bc]}$$

Thus a_i^1 may be computed from the cross products. However, there are $\binom{m}{2}$ cross products but only m latent parameters a_i^1 . (For the case of our six-item example there are fifteen cross products.) The result of this is that there are a number of different combinations of cross products which should give the same latent parameter a_i^1 . These different combinations will not have the same value if empirical cross products are used in our computations. At present there are no stand-

ards available on the basis of which one might judge when two of these estimates of latent parameters are effectively equal.

In some way we must average the different estimates for the parameter a_i^1 that can be computed from our data. The easiest way to include all the cross products in the computation of the a_i^1 is to consider the equations of the form

$$(a_i^1)^2[bc] = [ib][ic] \tag{27}$$

If we add the equations for all possible combinations b,c , holding i fixed, we may then factor out the $(a_i^1)^2$ on the left and get a_i^1 as the square root of the ratio of the two sums:

$$a_i^1 = \sqrt{\frac{\sum_{b,c} [ib][ic]}{\sum_{b,c} [bc]}} \tag{28}$$

Although this appears quite complicated, there happens to be a very convenient computing device in terms of more symmetric operations than appear in Eq. (28). We first write out the cross product matrix as in Table 5.

TABLE 5. CROSS PRODUCT MATRIX

	1	2	3	4	5	6
1	—	.041	.062	.069	.057	.029
2	.041	—	.080	.088	.077	.050
3	.062	.080	—	.107	.088	.054
4	.069	.088	.107	—	.103	.061
5	.057	.077	.088	.103	—	.058
6	.029	.050	.054	.061	.058	—

Then simply by summing the columns and squaring and similar operations and substituting in the computation formula⁷

$$a_i^1 = \sqrt{\frac{\left\{ \sum_b [ib] \right\}^2 - \sum_b [ib]^2}{\sum_b \sum_c [bc] - 2 \sum_b [ib]}} \tag{29}$$

we get the values written in the second line of Table 6. In its first line we have repeated the values we found for a_i^0 .

⁷ This formula, which is algebraically equivalent to Eq. (28), is Spearman's famous single-factor formula.

TABLE 6. THE TRACELINE COEFFICIENTS

Item number i	1	2	3	4	5	6
a_i^0	.34	.57	.62	.48	.38	.58
a_i^1	.185	.254	.309	.348	.300	.181

We now have all the necessary data for drawing graphs of the trace lines, as we have done in Fig. 6. We are not surprised that the graphs of items 1 and 6 are almost parallel, since their slopes, a_i^1 and a_6^1 , are very nearly the same; and similarly, the trace lines of items 3 and 5 are just about parallel. None of the trace lines in the set can

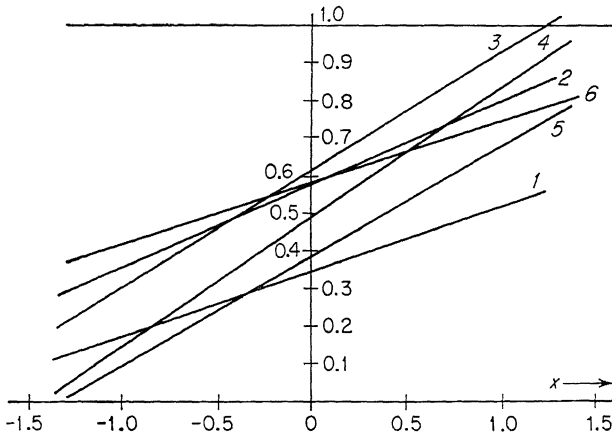


FIG. 6. The trace lines.

meaningfully extend beyond the point where any one of them becomes greater than 1 or falls below zero—certainly the distribution of respondents must be zero beyond those points. Linear trace lines give us some insight into the relationship between the different items and between each item and the latent continuum. (This will be discussed in the next section.) The data of Table 2 permit us to construct composite trace lines, those for response patterns consisting of more than a single item. The principle of local independence ensures that the trace line of a response pattern is given by the product of the trace functions for the individual responses which make up the response pattern. For example, the trace function for the four-item response pattern, consisting of positive responses to all the items 3, 4, 5, and 6, is

$$\begin{aligned}
 f_{3456} &= f_3(x)f_4(x)f_5(x)f_6(x) \\
 &= (.626 + .3009x)(.481 + .3454x)(.406 + .3002x)(.577 + .1833x) \\
 &= .006x^4 + .046x^3 + .131x^2 + .158x + .070
 \end{aligned}$$

When a response pattern includes negative responses, we have to replace the trace functions corresponding to the items answered negatively by their difference from unity, that is, we replace $f(x)$ by $1 - f(x)$. For example,

$$f_{1\bar{2}} = f_1(x)[1 - f_2(x)]$$

In Fig. 7 there are drawn the composite trace lines for the four response patterns 46, $4\bar{6}$, $\bar{4}6$, and $\bar{4}\bar{6}$. Individuals at the high job-satisfaction end of the latent continuum are most likely to respond positively to both items 4 and 6, whereas at the other end of the latent

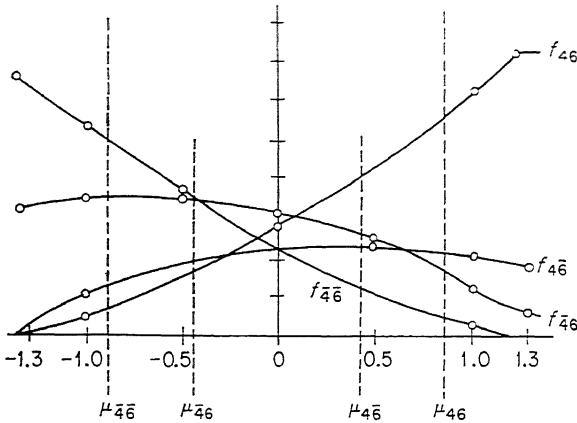


FIG. 7. Trace lines for four response patterns.

continuum, a negative response to both items is most likely to occur. This is what one would expect in advance. The really new insights come if we compare the trace lines $f_{4\bar{6}}$ and $f_{\bar{4}6}$. Their form could not be guessed by looking at the content of the items. It turns out that $f_{4\bar{6}}$ is more like f_{46} and $f_{\bar{4}6}$ more like $f_{\bar{4}\bar{6}}$. A more detailed analysis would show the reason: item 4 (as can be seen from the slopes in Fig. 5) has a sharper relationship to the latent continuum, is more indicative of it; as a result it, rather than item 6, determines the place of $+-$ and $-+$ between the consistent response patterns $++$ and $--$. The matter of ordering will be taken up in step 9, and then we shall also explain the other features of Fig. 6 not yet discussed here.

The basic and the composite trace lines represent conditional probabilities. They indicate how likely a person is to exhibit a given response pattern if he is at a point x of the latent continuum. This still leaves the question open, how many respondents are at each point x . To answer this we need the density function $\varphi(x)$ for the whole population of respondents. What we can identify are the moments of this distribution.

For the computation we can use fitted cross products $a_i^{-1}a_j^{-1}$ or fitted second-order positive frequencies $a_i^0a_j^0 + a_i^{-1}a_j^{-1}$ rather than the values obtained directly from the data. We thus eliminate some of the random variability. We can, for example, substitute these fitted values into the expression for M_3 [Eq. (21)]. Only three items are involved in any one computation of the third moment, so that $\binom{6}{3} = 20$ different computations are possible. In practical work we average either all of these computations or some sample of them. In our example the average of the estimates of M_3 turns out to be $-.011$.

From the accounting equations for third-order parameters we can now compute third-order fitted frequencies. Then in the computation of the fourth moment we can again use fitted parameters, instead of raw data, at all levels up to the third. On the other hand, there are $\binom{6}{4} = 15$ new pieces of empirical data, the 15 fourth-order joint positive frequencies, each of which provides us with one estimate of the fourth moment. Again averaging all these computations, we get a value of 1.57 for M_4 , and finally a value of 5.47 for M_5 .

From four moments one can get a rough idea how a distribution looks as compared with the well-known normal distribution. M_3 is slightly less than zero, which means that the distribution is somewhat skewed to the right. M_4 is considerably less than 3, which means that the curve is much flatter than the normal distribution. With higher moments we can compute equivalent discrete classes. This is a procedure which is of importance for many latent structure models and therefore deserves further special mention.

Suppose we want to approximate the distribution of people by assuming that they are concentrated at three points x_i with a relative frequency of v_i so that $\sum_i v_i = 1$. We can then define two moment matrices

$$M = \begin{vmatrix} M_0 & M_1 & M_2 \\ M_1 & M_2 & M_3 \\ M_2 & M_3 & M_4 \end{vmatrix} \quad M^* = \begin{vmatrix} M_1 & M_2 & M_3 \\ M_2 & M_3 & M_4 \\ M_3 & M_4 & M_5 \end{vmatrix}$$

It can be seen easily that

$$M = WNW' \quad M^* = WNXW'$$

$$\text{where } N = \begin{vmatrix} v_1 & 0 & 0 \\ 0 & v_2 & v \\ 0 & 0 & v_3 \end{vmatrix} \quad \text{and} \quad X = \begin{vmatrix} x_1 & 0 & 0 \\ 0 & x_2 & 0 \\ 0 & 0 & x_3 \end{vmatrix}$$

and

$$W = \begin{bmatrix} 1 & 1 & 1 \\ x_1 & x_2 & x_3 \\ x_1^2 & x_2^2 & x_3^2 \end{bmatrix}$$

If we form the determinantal equation

$$|M^* - xM| = 0$$

its roots can be shown to be the location points x_i . Computation shows that practically all the cases cluster in two classes located at $x_1 = -1.03$ and $x_2 = .96$. In other words, the data indicate that people fall into about two equal classes, those who are satisfied and those who are not satisfied with their jobs; the frequencies in the two classes are respectively $v_1 = .482$ and $v_2 = .517$. A third class of completely insignificant size is characterized by very extreme satisfaction.

Step 7: Evaluation of the fit. After we have found the latent parameters we can ask: how well do the data agree with the fitted model? Since the data will never fit the conditions of reducibility exactly, we cannot expect that the "fitted frequencies" will be identical with the data. On the other hand, we should expect the differences to be small, and randomly distributed.

In most latent structure models, the method of solving the accounting equations proceeds in a manner similar to that described for the linear traceline model: the manifest frequencies are taken into account level by level. In our example the first-order frequencies p_i did not require any manipulation; we simply accepted them as estimates of the latent parameters a_i^0 . Then, by a certain averaging process we found the second traceline parameters a_i^1 from the cross products. But now we can already compute fitted cross products and fitted second-order frequencies from the traceline parameters; and before going on to find further latent parameters involving higher-order manifest frequencies, we can make some evaluation of how well the data up to the second-order frequencies are in agreement with the model. If we decide that the fit at this level is good enough, we go on to find other latent parameters, using wherever possible fitted frequencies instead of manifest data. For some models, including the linear traceline model, it is possible to make successive evaluations of the fit after each higher level of data has been utilized in the computation.

Since from Eq. (19)

$$[ij] = a_i^1 a_j^1$$

we can compute a fitted cross product matrix from the parameters a_i^{-1} . In our example we obtained the following:

TABLE 7. FITTED CROSS PRODUCT MATRIX $a_i^{-1}a_j^{-1}$

	1	2	3	4	5	6
1	—	.047	.057	.064	.056	.033
2	.047	—	.078	.089	.077	.046
3	.057	.078	—	.108	.093	.056
4	.064	.089	.108	—	.105	.063
5	.056	.077	.093	.105	—	.054
6	.033	.046	.056	.063	.054	—

Is the fitted cross product matrix “close enough” to the matrix of cross products obtained from the data? The simplest way to compare the two is to subtract one from the other, and to consider the size of the residuals:

TABLE 8. MATRIX OF RESIDUALS $[ij] - a_i^{-1}a_j^{-1}$

	1	2	3	4	5	6
1	—	-.006	.005	.005	.002	-.004
2	-.006	—	.002	-.001	.001	.005
3	.005	.002	—	-.001	-.005	-.002
4	.005	-.001	-.001	—	-.002	-.002
5	.002	.001	-.005	-.002	—	.003
6	-.004	.005	-.002	-.002	.003	—

The relatively largest residual is $-.006$; the corresponding empirical cross product is $.041$, which makes the residual just about 15 per cent. It can be shown that these residuals have a concrete meaning, which can be understood best by reference to Fig. 1. There we saw that if we use a raw score to simulate a latent continuum, the associations within each of the partial fourfold tables do not vanish. If our data fitted the linear traceline model perfectly, the principle of local independence would assure us that no residual associations of this kind remain. If they do, they indicate the average cross product remaining between two items after the latent continuum “has been taken out.”

Once the model parameters are found, we can compute the fit of the model on any level. Table 9 gives the actual and the fitted positive joint frequencies on the fifth level. As can be seen, the divergencies are very small; but no theory of error yet exists to permit a rigorous test.

TABLE 9. ACTUAL AND FITTED JOINT FREQUENCIES OF ITEM QUINTUPLETS

Combination of items	Actual joint frequencies	Joint frequencies required by model
12345	.127	.129
12346	.132	.134
12356	.113	.115
12456	.112	.103
13456	.119	.114
23456	.174	.178

Step 8: The recruitment pattern. The trace lines tell for each point on the latent continuum the probability with which any response pattern will occur.

Now we want to raise the reverse problem. Given that a person exhibits this response pattern, where in the latent space is he located? The answer is somewhat surprising. He can come from anywhere in the latent space. But the probabilities are great that he will come from certain sections and slight that he will come from others. Each response pattern has its recruitment pattern, a distribution of "inverse probabilities"; it indicates for every point x the probability that a respondent with the given response pattern comes from this place in the latent space. It is important to understand the difference between traceline probabilities and recruitment probabilities. Perhaps the simplest way to illustrate the difference is to consider a cross classification of the population of respondents by response pattern and location in the latent continuum—the distribution of people being, for the sake of illustration, considered as a set of discrete classes. In the table below let n_g^s be the number of individuals giving the response pattern g who are in latent class s , n_g being the total number of individuals giving the response pattern g , and n^s being the total number of individuals in class s , and n being the total number of individuals in the population. The traceline probabilities are the

n_1^1	n_1^2	...	n_1^s	...	n_1	
...	Manifest response pattern frequency
...	n_g^s	...	n_g	
n^1	n^2	...	n^s	...	n	
Latent class frequency						

probabilities of response pattern g at given points of the latent continuum, in our table the probabilities in a particular latent class:

$$p_g^s = \frac{n_g^s}{n^s}$$

On the other hand, the recruitment probabilities are the probabilities of a latent class s , given the response pattern g :

$$r_g^s = \frac{n_g^s}{n_g}$$

Obviously

$$r_g^s = \frac{n^s}{n_g} \cdot p_g^s$$

The probability of a response pattern g coming from a class s is directly proportional to the size of class s and the latent probability of g at s , and inversely to the frequency of the response pattern. If we have a continuous distribution, we can define

$$\Psi_g(x) = \frac{\varphi(x)f_g(x)}{p_g} \quad (30)$$

where p_g is the proportion of all respondents giving response pattern g , and $f_g(x)$ is its composite trace line. Equation (30) tells us to what extent respondents of type g are recruited from each point x of the latent continuum. But we know only the moments of $\varphi(x)$. Therefore, we can only compute the moments of $\Psi_g(x)$. As an example, let us consider the two-item pattern of positive responses to both items 4 and 6. The mean position may be computed as follows:

$$\begin{aligned} \int x\Psi_{46}(x) dx &= \frac{1}{p_{46}} \int xf_{46}(x)\phi(x) dx \\ &= \frac{1}{.339} \int x(.481 + .345x)(.576 + .183x)\phi(x) dx \\ &= .817M_1 + .847M_2 + .186M_3 = \mu_{46} \end{aligned}$$

and substituting the numerical values which we have found for the moments

$$\mu_{46} = .835$$

Similarly we would find that

$$\mu_{4\bar{6}} = \frac{1}{p_{4\bar{6}}} \int xf_4(x)[1 - f_6(x)]\varphi(x) dx = .441$$

and

$$\mu_{\bar{4}6} = -.433\mu_{4\bar{6}} = -.859$$

The values of these expected recruitment positions are indicated in Fig. 7. It is important to notice that we need $(m + 1)$ moments and therefore $(m + 1)$ items, if we want to compute this position for a response pattern based on m items. It is also possible to develop a measure for the discriminating power of a response pattern g in the form of

$$\int x^2 \Psi_g(x) dx$$

but its discussion would lead us too far afield.

Step 9: Classification and scores. When we have found out as much as we can about the distribution of respondents over the latent continuum for each pattern of responses, we may then ask how we might assign the individuals who have responded in a particular way to a point in the latent continuum. We might ask about the most typical, the most likely, or the "average" position of the respondents of a particular type; or we might be satisfied with an ordering of the response patterns on the basis of some such criterion. In the preceding section we talked about the mean values of x for given response patterns, and they are often convenient as indicators of "typical position" for respondents, or as indicators of the rank of the response pattern along the latent continuum. An alternative is to ask, what is the most probable position for an individual who has responded in a given way. This question clearly can be answered only in those cases where we know not just a few moments but the whole distribution of recruitment probabilities given by Eq. (30).

Mean values, or modal values, such as we have here discussed may be looked upon as *scores* to be assigned to the respondents on the basis of their response patterns. But whenever we construct scores for respondents or their responses, there is a corollary problem of scoring the items—how much does each contribute to the score of respondents, how much does each item help to discriminate between individuals located at different points of the latent space? We remark only that in the linear traceline model it is clear that the steeper the trace line the better it can discriminate between two extremes of the range of the latent variable. The steepness of a linear trace line is indicated by its slope, a_i^1 . For other models, similar indices may be constructed, though they usually do not fall out of the latent structure in such a simple fashion. [See 14, p. 377, for more detailed discussion.]

Summary. We have now gone through the nine steps of latent structure investigation for the linear traceline model. The outline is the same for all latent structure models and is presented schematically in the diagram following.

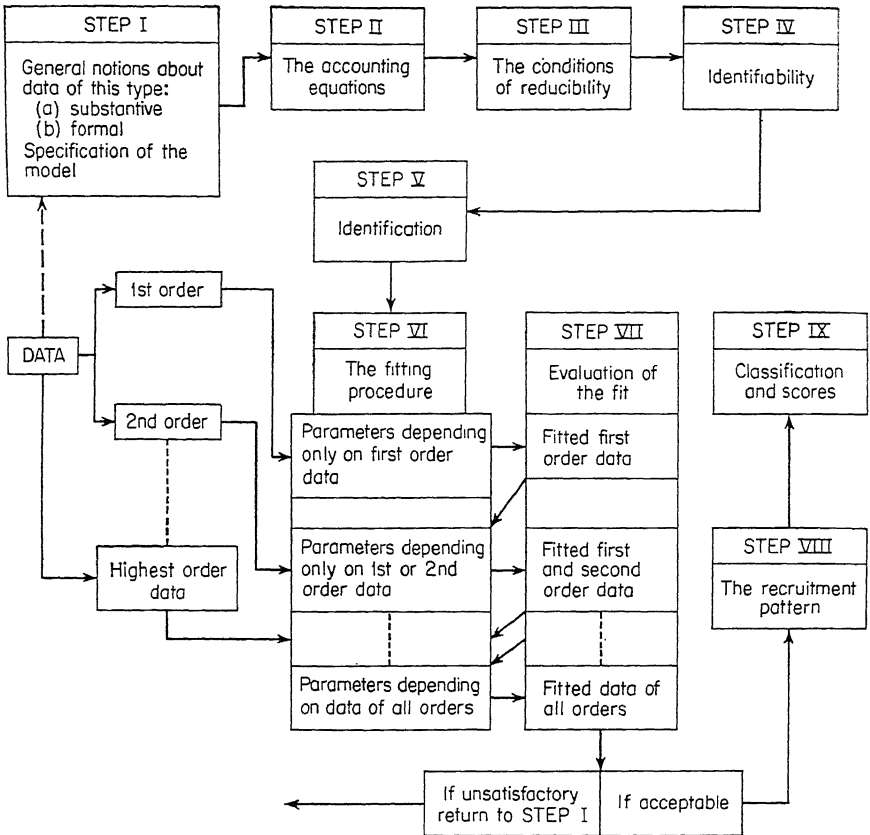


FIG. 8. Summary of latent structure steps.

IV. THE PROMISES AND LIMITATIONS OF LATENT STRUCTURE ANALYSIS

Although the linear model is of unusual mathematical simplicity, the steps which were described in the preceding section are typical for all models; so are the findings. A complete solution consists of the following elements: (1) the coefficients of the trace lines, (2) information on the distribution of people over the latent space, (3) indications as to how well the assumed model fits the empirical data, (4) procedure to score response patterns if such scores are desirable.

The question now arises: what scientific contributions can latent structure analysis (LSA) make? Two aspects have to be distinguished. One is the possible contributions to the logic of empirical research. This is best discussed by means of a comparison with procedures which have a similar intent; we shall presently turn to such a comparison of

LSA with factor analysis and formal test theory. The other aspect is the practical usefulness of the numerical results obtained from a specific model.

The meaning of trace lines. At this point it is necessary to warn against overrating models of this kind. They do not represent a theory in a strict sense, but a set of organizing principles. A typical investigation of an attitude or trait begins with the assumption that certain indicators will be useful to classify people for a given purpose. LSA can only clarify intrinsically the meaning of these indicators. It cannot tell whether the general purpose of the investigation has been reached. The nature of such an intrinsic analysis is best approached through a concrete example taken from a somewhat more complicated model.

In a study of academic freedom, a so-called apprehension test was developed. Social science teachers were asked a number of questions, of which the following four are characteristic examples:

1. Have you worried about the possibility that some students might inadvertently pass on a warped version of what you have said and lead to false ideas about your political views?

2. Do you ever find yourself wondering if because of your politics or something political you said or did that you might be a subject of gossip in the community?

3. If you are considering a move to another college, have you wondered if that college would inquire at your present college about your political views?

4. Have you toned down anything you have written lately because you were worried that it might cause too much controversy?

The model applied to these and a number of similar items was a so-called latent content model. The trace lines of this model are a special case of the following equation:

$$y = a + b(x - c)^d$$

If in this equation the exponent d approaches infinity, then the model formalizes the well-known social distance scale developed by Bogardus. If we specify further that $a = 0$ and $b = 1$, we have what is called a perfect Guttman scale [26].

On the other hand, if we set $c = 0$, we have a trace line with three parameters which is able to summarize a great deal of useful information.⁸ Figure 8 shows the trace lines for the four items in the apprehension index just quoted.

The interpretation of these trace lines is almost obvious. The first three items happen to have almost the same marginal response fre-

⁸ For the details of this model, see [25].

quency—about 40 per cent. But the relation to the latent continuum varies. The gossip item (2) is represented by a practically straight line. As teachers become more apprehensive, they are more likely to be concerned about the repercussions of their political views in the community; and this probability increases quite proportionately to the increase in apprehension. Concern about one's future job (3) behaves differently. Its probability rises much more quickly; already at a low degree of apprehension teachers are likely to worry that their chances to move to another college would be jeopardized by some opinion they have expressed at their previous job. On the other hand,

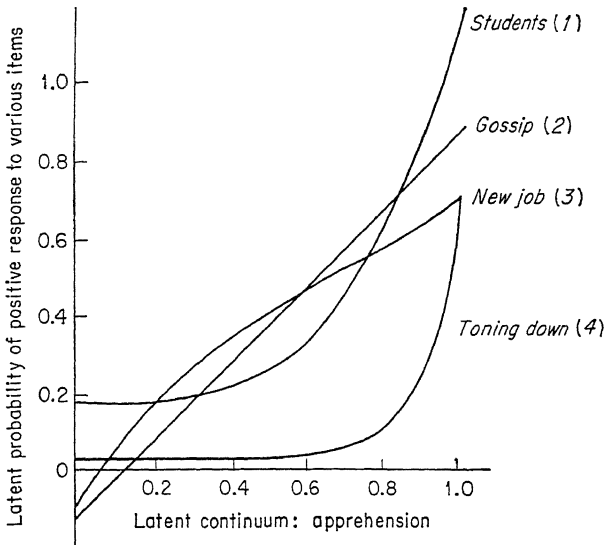


FIG. 9. The trace lines for four items of an apprehension index.

even at a high degree of apprehension this worry is not as general as the concern with gossip. After all, gossip is an ever-present danger, although many people do not think far into the future or do not expect that they will ever have to move to another college.

The item regarding student misrepresentation (1) is different in two respects. First, we notice that the onset of its trace line is higher. Even people who are not apprehensive at all reckon with students' misrepresentation as part of the necessary hazards of their occupation; for quite a while as apprehension goes up, the probability of this concern does not increase very much. But at a very high level of apprehension it suddenly shoots up and becomes rather dominant. The fourth item deals with the toning down of one's own writing. Here, too, the probability that a teacher tells about such a precautionary

move appears only on the right side of the graph and then rises very rapidly. But compared with the others, the manifest frequency of this item is much lower. It will be remembered that the marginal frequency is essentially an integral over the trace line and therefore in the graph represented by the area below it.⁹

By now the reader should realize clearly that this graph is not the result of a conventional item analysis. The underlying continuum of apprehension is not represented by a raw score of any kind. The parameters of the trace lines are derived from the higher-order joint frequencies, the manifest data which describe the interrelation between the items. The trace lines, so to say, define the meaning of the underlying dimension. But, at the same time, they clarify the meaning of the various items in relation to each other. Each of the three coefficients a , b , and d makes a different contribution to the shape of the trace line $y = a + bx^d$. b indicates something like the expressive value of the indicator. The larger b is, the greater is the difference in probability between the left and the right side of the graph; this means that in respect to this indicator, apprehensive and nonapprehensive teachers are especially different. The coefficient a tells to what extent an affirmative answer is common to all teachers irrespective of their own apprehension. The curvature of the trace line is approximately indicated by d ; it could be called the severity of an item: whether an affirmative answer is given easily or whether it needs a great deal of apprehension to reach it. Here we have an obvious parallel to the notion of difficulty in knowledge tests.¹⁰

The clarification of meaning is then one of the major results of a latent structure analysis, and this turns out to be a rather complex procedure. The underlying continuum and the psychological meaning of specific questionnaire items define each other. The same item combined with the different set of others could have a different trace line, and therefore contribute different meanings to the whole structure. This, however, is exactly what we should expect in material which has a strong projective element. After all, whether a teacher is worried about misrepresentation by students can be the indicator of a politically endangered professional situation. But it could also be the expression of a general trait of anxiety. With questions pertaining

⁹ The latent content model includes the assumption of a uniform distribution of the population of respondents over the latent continuum. See discussion below.

¹⁰ LSA refines the traditional notion of difficulty. Two items in a test can have the same manifest frequency of correct answers and, therefore, the same area under the trace line. They, however, could differ in shape, like items 1 and 3 in our graph. Item 1 would be more difficult in terms of the specific ability on the test but easier as far as common knowledge goes.

to professional situations, an affirmative answer might have a different significance than if the item is combined with other questions pertaining to nonprofessional concerns. Consequently, the value of a latent structure analysis is considerably greater with somewhat ambiguous matter than with questions where the content is clear-cut—for instance, the items in an arithmetic test.¹¹

Before we leave the example, one other aspect of this traseline graph should be mentioned. At the left and the right end, some of the probabilities go below 0 and above 1, respectively. This, of course, is a mathematical absurdity. The extent to which it happens indicates that the model is not quite appropriate to the data, either because it is too simple or because of sampling errors. A sampling theory has been developed for some models, but by no means as yet for the system as a whole [3, 19].

Distribution in the latent space. So much for the practical import of the trace lines. The information provided by the distribution curve is more obvious, but it is worth while to distinguish four types of situations. In the model discussed in section III, no assumption was made about the latent distribution. We were able to compute some of its moments; and it became clear that the more items are available in the manifest data, the more moments could we compute. In a more generalized form the trace lines in this model would be polynomials of any degree, and the latent space could be multidimensional. The algebra of this model has been solved completely and represents the most advanced point of LSA at the moment [20, 22].

In a second group of models one assumes a rectangular distribution of the population, say between the limits of 0 and 1. This is really tantamount to abandoning any effort to find a metric in the latent space and looking for only an ordinal ordering of response patterns. One might call this group scale models. The classical example is the Guttman scale and a variety of possible generalizations corresponding to what Guttman calls quasi scales. The most manageable model of this kind is the so-called latent distance scale [20, 26].

A third type of assumption is very suggestive but leads to extraordinary mathematical difficulties. Certain algebraic forms for trace lines are chosen because they approximate what we think is the behavior of people who differ in their position on the latent continuum. In the same spirit, we really are interested only in some general information on the distribution of these people: whether it is right or left skewed, steep or flat, etc. Such knowledge could be obtained by giving the distribution function a predetermined algebraic form

¹¹ This is the reason why the model leading to Fig. 5 is referred to as a latent content model.

with just a few parameters, to be computed from manifest data. For instance, the form

$$\varphi(x) = kx^a(1 - x)^b$$

has a great deal of flexibility. The trouble however is, as we know from the previous section, that such a function gets multiplied by the formulas of the trace lines. In order to be able to evaluate the ensuing integrals, the trace lines and the distribution function have to have somewhat comparable form. Quite a number of combinations have been tried, but none turned out very successfully. So far this is the point where the least progress has been made.

From a practical point of view the most useful case thus far studied is the discrete class model. It is best understood as a kind of latent typology. The assumption is that people are divided into homogeneous classes without implying any special ordering in advance. W. Gibson, for instance, has taken data on preferences of radio listeners for thirteen types of evening programs [23]. He has shown that the manifest joint response frequencies can be reduced very successfully by assuming six types of listeners. To simplify the presentation we reproduce the latent probabilities for four of the latent classes pertaining to six of the programs. The data of Table 10 suggested to Gibson a rather convincing interpretation.

TABLE 10. LATENT PROBABILITIES FOR PROGRAM PREFERENCES IN FOUR LATENT CLASSES AND MARGINAL MANIFEST FREQUENCIES (p_i) FOR A SAMPLE OF 2,200 RADIO LISTENERS

Preferred program type	Class				Manifest marginal
	A	D	E	F	
Comedy.....	.37	.58	.85	.96	.62
Mystery.....	.34	.29	.83	.22	.46
Semi-classical music.....	.08	.92	.41	.09	.37
Classical music.....	.10	.89	.25	.27	.34
Religion.....	.04	.32	.55	.61	.21
News.....	.40	.84	.80	.96	.77
Proportion of people in class.....	.27	.21	.10	.11	1.00

The most outstanding single characteristic of Class A is that none of its latent marginals is greater than .50. This class must, therefore, consist principally of people who do not care much for listening to the radio in the evening.

Class D is also not difficult to identify, for a very high proportion of its members, in contrast to those of other classes, like to listen to semi-classical and classical music. (They are also fairly high on talks on public issues.)

These are undoubtedly the sophisticates or "high-brows." Characteristically, they have also little interest in mystery programs.

In contrast with Class A, Class E is characterized primarily by consistently high latent marginals, none of which is lower than .25. (Substantial proportions of this class even like to listen to serials and hillbilly music, which are quite unpopular with all other classes.) It is interesting to note further that the two more serious music programs liked least by this group are the same ones that are liked most (except for news) by the sophisticates. All of these characteristics suggest that this might be the "low-brow" group.

Finally we come to Class F, which is distinguished by the highest latent marginal for religious programs (the lowest for popular music), quite low ones for comedy and mystery, and no extremely strong likes other than for news. This combination of a religious component, a lack of interest in what might be regarded as a younger type of program, and a somewhat subdued enjoyment in radio in general, except for news programs, points toward one large group of radio listeners—that of older and/or small town people.

To test his interpretation, Gibson selected respondents who had a high recruitment probability of coming from one of these classes and studied their demographic characteristics and other information available about them. He found indeed a clear educational difference between Classes D and E, many more older people in Class F, and so on. It should be noticed, incidentally, that, correlative to this interpretation of the classes, Table 10 also throws light on the latent appeals of the programs themselves; the rather universally high preference for news programs is here the best example.

The discrete unlocated class model has the advantage that only up to third-order joint frequencies are needed to compute all latent parameters. The procedure requires essentially the solution of determinantal equations of the type used in section III, step 6. A corresponding shortcoming consists in the fact that (in contrast, e.g., to the linear traceline model) no ordering of the classes can be derived from this model. Sometimes, however, an order is strongly suggested by the *content* of the items together with an inspection of the latent probabilities. The following example is revealing.

The reader is certainly acquainted with the type of attitude measurement developed by Thurstone and Chave. The procedure consists in submitting to judges a series of statements regarding, for instance, Negroes. The judges classify these statements in an order which ranges from strongly favorable to strongly unfavorable. From this range obtained by the *judges* a scale value for each item is derived. Then a group of *respondents* checks the statements corresponding most nearly to their own attitudes. The final attitude score of each respondent is the average scale value of all the statements he checks.

It is obviously possible to treat the responses of such a test like

any other set of response patterns and to submit them to latent structure analysis. Eight such items were selected and a latent three-class structure fitted the manifest data well [8]. For our present purpose we shall select five of these items. In Table 11 we report the wording of the statements and the scale value which they got from the judging procedure.

TABLE 11

<i>Statement</i>	<i>Scale values</i>
1. The idea of contact with the black or dark skin of the Negro excites horror and disgust in me.	10.2
2. I am not interested in the Negro or in his relations to the white man because I think that in the end economic factors will decide his fate.	5.6
3. Sometimes I feel that the Negro is not getting a square deal, but at other times I feel he has a lot to be thankful for.	5.4
4. Until the Negro has been given more time and opportunity of showing what he is capable of doing, I think that it is foolish to try to judge him.	5.1
5. I think that the Negro ought to be given every opportunity of education and development—just like the white man.	1.1

It will be seen that two of these statements belong to the anti-Negro and pro-Negro extremes, respectively. The other three, according to the judges, have about the same middle position.

From latent structure analysis we can infer whether the respondent actually did interpret the statement in the way the scale value indicates. Table 12 gives the latent marginals for these five statements in three latent classes: Class I is the most anti-Negro, and Class III is the most pro-Negro.

TABLE 12. LATENT STRUCTURE OF AN ANTI-NEGRO TEST

Reaction to item	I (anti)	II	III (pro)	Judges' scale values
1. Horror486	.236	.072	10.2
2. Economics124	.199	.021	5.6
3. Sometimes385	.963	.276	5.4
4. Don't judge084	.489	.422	5.1
5. Give opportunity068	.667	.916	1.1

We see that the latent class structure corroborates the scale value on the two extreme items. The idea that contact with a dark skin excites horror gets its largest endorsement in Class I, and hardly appears in Class III. The willingness to give the Negro all possible opportunity is inversely endorsed by Class III and rejected by Class I. On both items Class II takes an intermediate position.

But how about the three middle items? In regard to item 3, Class II has an almost complete propensity to endorse it. The probability

of endorsement is small in Classes I and III. Looking at the wording of item 3, we are not surprised. It really states in so many words that the respondent cannot make up his mind on the Negro question.

Item 2 and item 4 however show a different structure from each other and from item 3. In both of these items the latent marginals in the middle class are not much higher than in one of the others. Item 4 has a high marginal in Class III while item 1 is relatively high in Class I. What lead do these figures give for a better understanding of the content of items 4 and 2? Item 4 states that it is too early to judge the capabilities of Negroes. This could mean one of three things: One should not judge him too hastily as capable, not too hastily as incapable, or just not judge him at all. The latent structure suggests that the respondents in this group endorse this question mainly when they want to prevent an unfavorable judgment. Item 4, in spite of the medium scale value, really belongs on the more favorable side of the presumed scale. This incidentally, will be understood if we add that this test was given in the South where the prevailing trend is, of course, unfavorable judgment.

Item 2 also contains two elements. One expresses a lack of interest in the whole question; the other expresses faith that economic factors will settle the issue. One might have expected that this statement would have a greater appeal to liberal respondents who would be likely to stress the importance of economic factors. This seems not the case, however. It is the more discriminatory group which is likely to endorse item 2. A possible interpretation is this: the statement starts with the phrase "I am not interested in the Negro . . ."; for many respondents the lack of interest in the issue might have been the leading element in their interpretation. And in the South an unwillingness to discuss the Negro question would, of course, be an endorsement of the present bad situation.

Three questions then, which seemed to a group of judges to represent a very similar position, were not experienced in this way by the respondents. Item 3, which explicitly expresses doubt, was a real middle item. But item 2, which really was compounded of two statements, seems to have given to the respondents more emphasis to one of its elements, while the judges considered it balanced. Item 4 was answered in a context which made it function less as a middle item than its grammatical form led the judges to expect.

It deserves notice that Thurstone was aware that something like LSA had to be tried. After presenting his judging procedure he wrote [27]:

We shall mention here in passing the possibility of determining the scale-values of the statements without the rather laborious sorting process. It may

be possible to scale the statements directly from the voting records of a large group of subjects provided that a considerable range of attitudes is represented in the group of subjects used for this purpose. The principle involved is that if two statements are close together on the scale, then the people who vote for one of them should be quite likely to vote for the other one also. If the statements are very different, spaced far apart on the scale, then those who vote for one of the statements should not be very likely to vote for the other one also. It might be possible to reverse this reasoning. We might then be able to infer the scale separation between two statements in terms of the number of subjects who indorse both statements, $n_{1,2}$, the number who indorse the first, n_1 , and the number who indorse the second, n_2 .

The discrete unlocated class model serves well also if one wants to analyze the type of work sponsored by Lloyd Warner. He, as is well known, assumed that six classes are necessary to give an approximate idea of American social structure. He uses itemized material like people's properties, reading habits, and organizational membership to allocate them into proper classes [30]. By using the joint higher-order frequencies of these items, one can decide how many classes reproduce the empirical data. For the material he has provided so far, three classes seem to be sufficient [24].

So far all the examples have dealt with attitudes and therefore necessarily pertained to individuals. It is easy to see how we would extend LSA to collectivities. Suppose we were interested in the "cohesiveness" of a number of small groups. We might ask their members a series of questions: Are most of their friends inside the group? Do they like the group activities? What other group would they rather belong to? By proper manipulation of data each group could be classified in a manifest dichotomous property space according to whether it was, say above or below the average on these criteria. The ensuing response patterns could then be analyzed by any latent structure model; the only difference being that the statistical unit is a group and not a person. The same would be true if we took cities and wanted to classify them by "goodness of life," taking as indicators, e.g., the number of playgrounds, the number of libraries, the juvenile delinquency rate, etc. Once these indicators are somehow dichotomized the analytical machinery is exactly the same, whether applied to people or cities.

It is more difficult to predict whether LSA will be useful if applied to conceptually more complex intervening variables as they appear, e.g., in learning theory. No effort in this direction has yet been made. There is, however, considerable material available resulting from the application of LSA to repeated observations. Processes going on in time can be clarified this way. A simple example can be taken from a

readership study. Three times, at intervals of a month, 986 people were asked whether they had read the last issue of a weekly magazine *W*. The manifest data were as follows:

TABLE 13. READERSHIP OF MAGAZINE *W*

Query period	Readership			
	Reader		Nonreader	
Time 1.....	Reader	Nonreader	Reader	Nonreader
Time 2.....	Reader	Nonreader	Reader	Nonreader
Time 3:				
Reader.....	30	11	17	64
Nonreader.....	14	60	56	734

It can be seen that 30 people had read each of the three issues under study and 734 read neither; the others read some. From such data the latent parameters of the following model can be computed. It is assumed that people fall into two groups: readers and nonreaders of magazine *W*. The (latent) readers have a probability to read a single issue p^1 , which will be large but not quite unity. The nonreaders will have a small (latent) probability p^2 ; this means that even nonreaders look at an issue occasionally. Under this assumption it is possible to derive that 6 per cent of the sample are latent readers and 94 per cent are not. The former have a probability of $p^1 = .75$ to read a single issue, for the latter $p^2 = .08$. This means that nonreaders behave more predictably than readers, which makes intuitive sense. L. Wiggins has developed quite complex dynamic models and has applied them to repeated observations on voters, consumers, etc. [31].¹²

Comparison with factor analysis. One obvious difference between LSA and factor analysis lies in the manifest data with which they deal. The raw material of factor analysis is the quantitative score which presumes already that a number of qualitative items have been combined into a test. The latent structure analyst starts with the test items themselves. Now it is true that factor analysis has been used for qualitative items by applying various coefficients to measure associations between fourfold tables. But it is well known that a factor structure can come out differently, according to whether one uses tetrachoric correlations, point coefficients, or any other such device. Of this difficulty, LSA is free because only independence enters the picture, and this can be defined without using any measure of association. The principle of local independence reformulates the whole problem so that we never need to use any measure of association between the manifest data. It is important to realize that the cross products and

¹² The main results are to be reported in a forthcoming book on panel analysis.

similar combinations of manifest data are only transient algebraic devices to achieve the final result and in no way appear as substitutes for correlation coefficients.

Green has shown that factor analysis can be reformulated so that it turns out to be a special case of a general latent structure scheme [9]. In this version the factors form a latent space. At each point of the latent space people have latent test scores and the Pearson correlation between them vanishes. The actually observed correlations between manifest test scores are derived by the mixing of latently homogeneous groups. There even exists a complete parallel to trace lines: they are formed by the average test scores at each point of the latent space.¹³ This reformulation of factor analysis would permit its extension to nonlinear material. For nonlinearity is one of the main features of LSA and this point deserves some further discussion.

The origin of factor analysis was Spearman's one-factor theory. He assumed the scores of all tests to be linear functions of this one factor. The ensuing structure is very similar to the model we used all through section III. When Thurstone extended Spearman's theory, he added more factors, but the test scores remained linear functions of them. It is however obvious that a different kind of extension of the original Spearman theory would be possible. We could retain the restriction to one factor, but the test scores could be nonlinear functions of it. An example is the model exemplified by Fig. 8. Finally, one can combine several latent dimensions with nonlinear trace lines. In LSA it is possible to develop separate criteria, for the number of dimensions and for the degree of nonlinearity of trace lines. The ascending matrices mentioned in step 5, section III, are the crucial device for this distinction, but it is not possible to extend this paper to such detail [22].

Even a cursory reading of section III will have shown another way in which LSA, while using the basic logic of factor analysis, extends its range. In factor analysis only zero-order correlations among manifest data enter the picture; they correspond to our second order joint frequencies or cross products. Because of certain basic assumptions, partial correlations do not add new information for the factor analyst; they are an arithmetical derivation of zero-order correlations. To put it differently, factor analysis does not make use of higher-order covariances between test scores. Remembering the definition of a correlation coefficient r_{12} , one realizes that a form r_{123} does not appear in factor analysis; its definition would be

$$r_{123} = \Sigma stw$$

¹³ W. Gibson has extended this idea to a large number of empirical examples in a series of yet unpublished papers.

where the letters stand for standardized score for *three* tests. In LSA, however, such forms exist in terms of the higher-order frequencies. The use of manifest higher-order frequencies p_{ik} , p_{ijk} , and so on, makes for the much greater flexibility of the latent structure model. In step 5 of section III we have seen how the higher-order frequencies permit the computation of the latent moments. The coefficients of the linear trace lines, however, could be computed by going up only to second-order frequencies. In models with nonlinear trace lines the higher-order frequencies are also needed to compute the trace line coefficients. In a rather crude generalization one can say that the number of coefficients in the trace lines determines the level of manifest joint frequencies which enter the accounting equations.

Comparison with test theory. The full use of manifest data also characterizes the difference between LSA and test theory. A test score is a combination of a number of response patterns. All the people who give an affirmative answer to say three out of ten items have the same score. In LSA a distinction is made between the proportion of people who give a positive response to any specific combination of three items out of ten. In a test of n items, test theory uses only $n + 1$ manifest frequencies while actually 2^n are available. The notion of a response-pattern score was explained in section III, step 9: it is the expected position on the latent continuum of a person who gives a certain response pattern. The test score, on the other hand, is the number of items to which this person gives an affirmative reply. Interestingly enough, a mathematical relation can be established between the latent continuum and the test score. For a test score is a manifest datum which has its own trace line. To show this important fact we shall assume a test of just two items and use the data of Fig. 6 pertaining to items 4 and 6.

At a given point of the latent continuum the probability of two, one, or no affirmative answer is by simple probability considerations, respectively:

$$\begin{aligned} s_2(x) &= f_{46}(x) \\ s_1(x) &= f_{\bar{4}6}(x) + f_{4\bar{6}}(x) \\ s_0(x) &= f_{\bar{4}\bar{6}}(x) \end{aligned}$$

The values of these functions can correctly be called the latent scores. Integrals of the form $(1/p_\theta) \int s_\theta(x) x \varphi(x) dx$ give their expected values. This is nothing else than the scoring procedure developed in steps 8 and 9, section III, extended to groups of response patterns which have the same number of affirmative answers. To each test score then corresponds a position on the latent continuum or, as we might call it, a latent score. In our special example, these latent scores turn out to be

the values μ_{46} and $\mu_{4\bar{6}}$ of Fig. 8 for the scores 2 and 0, respectively; corresponding value for score 1 is $(p_{46}\mu_{46} + p_{4\bar{6}}\mu_{4\bar{6}})/(p_{46} + p_{4\bar{6}})$. Thus, there correspond to the three test scores the following points on the latent continuum:

$$s_2 = .835 \quad s_1 = .008 \quad s_0 = -.859$$

They are not equidistant and in general the test score gives a distorted picture of the latent continuum. From a practical point of view, the distortion is often unimportant. But the finding is a good example of the way a more general formalization of a problem throws new light on well-established procedures [17].¹⁴

One more relation between test theory and LSA deserves mention. The difference between an item curve and a trace line was discussed in section II. The x axis for an item curve (see Fig. 5) is the raw score, a manifest piece of information; for a trace line the x axis is a construct, the latent continuum (see Fig. 8).

The consequences of this difference can be seen easily in the case of a one-dimensional test. Within the framework of test theory we have to assume in advance that the test items are indicators of a one-dimensional continuum. This continuum is roughly approximated by the raw score, and the shape of the empirical item curve is then used to select items. In LSA, in the course of computing trace lines, we obtain a simultaneous test of unidimensionality. It so happens that in the linear traceline model this test is algebraically identical with the way one would test a Spearman one-factor structure. This, however, is by no means true for all one-dimensional models. In the case of the latent content model, for example, one-dimensionality requires the matrix of the reciprocals $1/[ij]$ of the cross products have rank two.¹⁵ Such tests are, of course, only special cases of what in section III, step 3, were discussed as conditions of reducibility.

The reader should keep in mind the difference between this comparison with test theory and the earlier comparison with factor analysis. In comparing the latter with LSA, we mainly discussed formal analogies and differences. Substantively the two procedures deal with different material, except in the unfortunate case when factor analysis is applied to qualitative data. But fundamental test theory

¹⁴ The relation between latent continuum and test score is mathematically simple in idea but cumbersome in expression. Frederic Lord has investigated it for a specific model. His discussion and his concrete examples show many more implications of the problem than we could touch upon here [17].

¹⁵ In [22], it is shown that this condition makes the model an interesting counterpart of a two-factor Thurstone model. The "rotations" come about on hyperbolas instead of circles and are determined by third-order frequencies.

does not deal so much with relations between tests, but with the role of the individual items within the same test; to this extent, it substantively overlaps with LSA and more work should be done in deriving notions like reliability or attenuation from the general properties of trace lines, latent probabilities, and latent spaces.

Be it repeated, however, that the application to one-dimensional tests is not the only, and probably not the main, theoretical contribution which LSA tries to make. It looks at test construction and scaling only as a special case of the larger problem of latent classifications derived from manifest qualitative data. In the last analysis it aims at attacking the broad issue of the relations between concept formation and empirical research in the behavioral sciences.

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WORK-EMOTIONALITY THEORY OF THE GROUP AS ORGANISM

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INTRODUCTION

The basic concepts of the theory were suggested and explored by W. R. Bion [3-9], then of Tavistock Institute, in a series of seven articles called *Experiences in Groups*. Bion's concepts gave the research discussed here its starting point.¹ During 1951-1955, various technical reports have presented our results to the Group Psychology Branch of the Office of Naval Research. The major corpus of method and findings is available in two monographs: one deals with methods [54], the other is concerned with theory and findings [44]. The work was also presented in 1952 at an American Psychological Association symposium [55], and it is discussed in *The State of the Social Sciences* [52].

BACKGROUND FACTORS

Research strategy. W. R. Bion [3] developed the concepts of work and emotionality. Our work attempts to take the next steps of operational definition, refinement, and prediction.

The development of a body of principles requires the contributions of men of different temperaments who can contribute what is needed

¹ At nearly all stages the work has been discussed and thought about by the research team. Nevertheless, major responsibilities have been accepted by individuals and subgroups. The team as a whole is mainly responsible for the sequential analysis method, although previous explorations had been conducted by deHaan [13]. Ben-Zeev [2] alone developed the system of unitization, however. The development and validation of the sentence-completion test was primarily the responsibility of Stock, I. Gradolph, and P. Gradolph [54]; the first explorations, in 1949 were by Rosenthal and Soskin. The method for identifying functional subgroups was primarily the responsibility of Stock and Hill [20, 54], with the help of Stephenson [42] who largely developed the *Q*-sort method. The various studies of productivity were conducted by different people: comparison of flight and work-pairing groups, by I. Gradolph and P. Gradolph [44]; problem solving of 50 groups was studied by Glidewell [18, 44]; comparison of the two training groups was by a team headed by I. Gradolph [44]; and the study of "trainability" was by Mathis [33, 44]. Several further studies of individual-group relations are in progress by Liebermann [31]. These and other subsidiary investigations, with full references to prior studies, will be found in the monographs [44, 54].

at each step. There is probably a range of involvement within which each of us works best. We need to reach out to the man with intuition to help us conceptualize the content of the experience we wish to study, and to the formalist or mathematician to see how to systematize and give form and elegance to our body of intermediate principles.

The sequential, cooperative development may be illustrated by three doctoral studies from the Human Dynamics Laboratory. The first [56] set out to try to hypothesize basic categories of teacher behavior in the classroom. The first step was to observe a couple of hundred hours of classroom interaction. This was recorded by sound and by time-lapse photography. These two records together enabled the observer, Withall, to obtain almost perfect recall of his experience. Hour after hour he spent studying the records. For each comment of the teacher, he asked himself: "What, in my opinion, is the teacher communicating to the class?" This is a subjective judgment. "If my judgment is correct," the observer inquired then, "What sort of behavior should the students now engage in?" This called for prediction of a general class of behaviors, and this prediction had to be made from previously learned principles. Thus if the teacher is judged to be making a punitive remark, then the students should in general act like students who felt they were being punished.

On the basis of this kind of substantially inductive thinking, Withall arrived at 24 categories. These were then discussed by the research seminar, and through more precise definition and theoretical examination, reduced to 6 basic categories so defined as to refer to distinguishably different psychological motivations of the teacher. (We might have trained ourselves to use all 24 categories, make many observations, and treat the data with factor analysis; but it seemed more economical to treat the matter conceptually—especially since there was already a body of theory to go on.) Withall tested the applicability of his categories in a couple of simple experiments—enough to persuade us that these categories were significant in the sense of being independent and interpretable.

Flanders [15] took the second step. He set up a simple laboratory experiment, replicated seven times, in which he used the Withall categories as independent variables and then predicted consequences in terms of a wide range of dependent variables. In other words, he trained teachers to produce two quite different "styles" of teaching, as judged by the Withall categories. Then he had them teach in an experimental situation and measured the consequences of the differences of the two styles with respect to a variety of physiological, recall, achievement, and affective variables. The fact that differences were found in the predicted directions supported the notion that the independent variables actually

did index the psychological mechanisms used for prediction and, further, that these mechanisms were properly conceptualized.

The third step was one of further demonstration that in a typical everyday situation the same dynamics would occur. Perkins [36] selected six classes of adults (all the preceding work had been with children) and assigned teachers to them. Three of the teachers had been observed to have one clear-cut style as measured by Withall's categories; the other three had styles similar among themselves but very different from that of the first three. The dependent variables were confined to categories of verbal response from members of the class. The experimenter collected data from 10 meetings of each of the 6 groups, ending with some 15,000 responses. These were rated in 30 carefully defined categories, and predictions were made as to the differences to be found in frequencies of each category as a result of the two different styles. Twenty-one of the predictions were borne out.

Thus we may represent scientific development in the study of such problems as, in general, beginning with inductive-intuitive hypotheses obtained from firsthand experience with the phenomena. Then, in a restricted situation, we have intensive study of a wide range of consequences. Finally the hypotheses are demonstrated, using a narrower range of variables but in a much more extensive situation, to make sense in terms of "real" groups doing their regular work.

All that we require to minimize disputes over methodology is (*a*) a large over-all view of the strategy of scientific development, (*b*) accurate representation of where our particular work fits into this larger strategy, (*c*) constant effort to reach out to make connections between our work and that of others. The values animating the study reported on in these volumes provide a further illustration of such a point of view.

Current approaches. Researchers operating with differing various approaches to the problem will be found at some 15 or 20 "centers" for group study. Each of these centers has its own goals, traditions, cohesiveness, standards, degree of individual freedom, differentiated roles, etc. Although their communication varies both in volume and quality, by and large, most researchers would probably agree that the work of the various centers is complementary and overlapping.

Studies of groups have been made on many levels of complexity, comprehensiveness, sophistication, and penetration, for different researchers develop characteristic methodological and conceptual approaches [see 11 and 19 for attempts to sample major recent work in the field]. To give some idea of the more prominent among these, I shall follow the formulation of Cartwright and Zander [11].

Basic dimensions. Cattell and his associates use factor analysis to determine the "major dimensions of groups." The factors are developed

from three kinds of variables: (a) measurable characteristic of individuals, (b) structural characteristics of the group: the system of positional interrelations shown in rank orders along various status continua, (c) syntality variables, representing the performance of the group as a whole. The dimensions represent, for the three kinds of variables, stable patternings in which, for different groups in different situations, the same variables enter into the same dimensions, but with different loadings.

Interaction. Bales, Homans, Chapple, White, and Arensberg are considered as having a common interest in the way the group develops and changes as the result of interactions among members and between the group and its environment. These two kinds of interactions occur in the "internal" system and the "external" system respectively. Bales's theory relates patternings of 12 behavior categories to the processes of developing group structure. Homans shows how the complex of activities, interactions, and sentiments develops the internal and external systems in their relationships to each other and to environmental factors.

Organizational leadership. Stogdill, Shartle, and Hemphill consider the organizational aspects of group life: the functions and responsibilities of individuals with reference to achievement of group goals. There are both formal and informal networks of relationships among these factors. The formal network defines expectations of role performance. The informal network defines actual role performance. Leadership influences both networks and finds problems in the discrepancies between them. Leadership is understood in operational terms as an aspect of work performance, work methods, and working relationships.

Psychoanalytic. Scheidlinger, Slavson, Redl, Bion, and Ezriel are seen working with Freud's notion that "group cohesiveness arises through common identifications of the members with one another." The meaning of a particular behavior to the actor has to be understood both as fitting into his genetically developed mode of adjustment and as involving him in present reactions to the external world. "Personality" is the habitual mode of synthesizing into a pattern of adjustment the aims of drives, conscience, and physical and social reality (environment). Personality develops through social interaction, especially in the family, and there are discernible phases in its development. Opportunity for needed social interaction is found in groups, and these form through common identifications, such as with the leader. Redl has identified 10 types of "central" people with whom, under various conditions, members of the group may identify and thus maintain the group. According to Bion's notion of "valency," at different times unconscious subgroupings form through "combination" in support of a particular emotionalized mode of group operation. The group ethos is the organizing principle through which individual strivings are coordinated in common effort. Role differentia-

tions and social structure are produced in accordance with the capacities of the individuals for interaction in the group.

Sociometric. Moreno, Jennings, Barker, Criswell, and others postulate that the "social space" within which an individual lives is delimited by his range of interaction with others and that this space is structured by his feelings of attraction or repulsion for others. Such feeling bonds are the bases of groups. Groups may be formed spontaneously from free choices of others (psychogroup) or they may be formed by social demands that require people to work together (sociogroup). The working out of interpersonal needs, as reflected in the choice pattern, is a major aspect of group process, and the choice pattern at any time reveals significant cleavages, subgroups, and group structure in general.

Force field. Kurt Lewin [28], often considered the founder of "group dynamics," saw that behavior arises out of the "life space" of the individual (cf. "social space," above). The life space contains perceptions of behavioral alternatives, such as different activities in which the individual might engage. Some alternatives are definite and clear (structured), others vague and unstructured. The alternatives have different degrees of attractiveness and repulsion (valence) depending upon their usefulness in meeting current needs. In addition, there may be permeable or impermeable "barriers" to "locomotion" into the chosen activity region, and these barriers also have a negative or positive valence. Thus the individual, represented as a point within his life space, is subject to a variety of forces which tend to influence him in a variety of directions. Applied to groups, these concepts lead to the notion that the distribution of leadership depends upon the degree of overlap or communality of the individual life spaces; cooperation and competition are viewed [e.g., by Deutch, 14] as conditions under which the efforts of an individual to locomote into a chosen activity region either facilitate or hinder similar efforts by other individuals. The primary data for studies within this frame of reference have been perceptions by members of themselves, each other, their group, and its activities.

Common elements. One can sense many similarities in the assumptions which seem to underlie the various approaches. Although no single rigorous integrated system can yet be set up, we can see dimly what sort of propositions will probably be required. System building is a series of mental operations, and we shall attempt to show how the various elements of the system emerge from successive and different operations. While doing this, we shall also attempt to present in relatively summary form—and with considerable diffidence—a set of theoretical elements which may represent a composite view of the various approaches.

All living organisms engage in activity; and all living organisms have organization in the sense of coordinated functioning of the different

parts. When we watch an organism, be it an insect or group, we can collect three kinds of information: (*a*) we can describe the obviously different physical parts, such as legs or members; (*b*) we can describe what the parts do—their overt behaviors; and (*c*) we can describe to some extent how the whole organism moves: the insect toward a puddle of water, or the group through an agenda.

On more careful examination, we may note that there are three kinds of overt behaviors: (*a*) individual, as when a leg twitches or a member fidgets; (*b*) interpersonal, as when two legs tangle or two members argue; and (*c*) member-environment, as when the leg pushes the ground or a member proposes a solution to a problem.

Over time, we become aware of regularities or consistencies between behavior of each part and particular states of the organism and of the environment. We draw inferences from comparison of the behavior of the leg or member at different times and in different situations; and we speculate about “what” might account for these inferred relationships.

At the heart of all our efforts to “explain” is the concept of purpose. Other terms having the same kind of usefulness are need, drive, motivation, and tension (to be reduced). We “explain” by saying that the insect or group or leg or member acts “as if” it were trying to go somewhere, accomplish something, or deal with some condition external to itself. We then infer that different parts (legs and mouth, Joe and Mary) participate differently in accomplishing a particular purpose (satisfy a need, reduce a drive or tension). We say that they have different functions or take different roles and that the functions and roles are coordinated in response to the over-all purpose of the organism.

At this point, however, our insect and group part company. There are, after all, some differences between physiology and psychology. For the question arises: why do the parts act together but differentially to accomplish a purpose of the organism? And, more generally, how does it maintain itself as an organism? This is the thorny problem of part-whole relationships.

The differences between insect and group lie in the distribution of sinews and nervous system. In the insect, the parts are connected physically: one nervous system coordinates movements; one brain directs the whole enterprise. In the group, however, the parts are not connected physically: the musculature is distributed among the members; instead of one central directing agent there are as many agents as there are members. Each member must then choose to belong to the group and to participate under particular conditions and in particular ways. And this process of choosing has both conscious and unconscious aspects.

At the conscious or nearly conscious level, we may take the utilitarian approach, saying that a member finds the group or activity at-

tractive either because he expects to get some personal reward from his participation or because he wants to avoid some perceived possibility of punishment. The reward might take the form of feeling better about himself, some cherished person, or some valued idea; reward might be a set of anticipations of new or additional opportunities, it might be what he perceives to be the better opinion of others, etc. Similarly, punishment might be perceived as coming from himself, others, or outsiders. In general, we tend to relate reward and punishment in the group to relative position: to be rewarded is to move into a more central position, to be punished is to be pushed toward the outside. The concept of centrality is related to esteem of the group, influence, personal adequacy in the group situation, support of other people.

At the unconscious level, the key concept is the notion of identification with others. This permits several possibilities. Everybody may feel a tie to some one "central person," and through this communality, individuals identify with each other [37]. Everybody might have a common interest, in the sense of being attracted toward the same activity (perceived in overlapping parts of each person's life space) [29]. There may be no central object-tie at all but simply a web of interpersonal attraction, in which the persons mutually choosing each other form overlapping subgroups [23]. Again, the formative bond may represent a tendency to act with others who are felt to desire the same emotionalized mode of operation in the group [8]. But, whatever the tie that binds, these various networks are perceived as belonging to the group as a whole; the body of agreements, values, common perceptions, and common expectations developed through communication while working together constitute the culture of the group. This culture, including the shared self-concept of the group as a unitary whole, becomes in itself an object to promote intermember and member-group identification (loyalty).

Included in the culture of the group is a set of goals with respect to its environment. If we can say that the opportunity for individuals to satisfy personality needs is the psychological *raison d'être* for the group, then we may also add that the common desire to change the environment (either social or physical) in some way is the social *raison d'être* for the group. Thus the group maintains itself for two quite different kinds of reasons, and the behaviors in response to these two group needs are not necessarily compatible. The psychological purposes are best served by being a psyche-group, acting voluntarily on the basis of internal demands; the social purposes are best served by being a socio-group, organizing to meet demands perceived as coming from outside (from the nature of the problem, from the orders of a higher institutional authority, etc.). This notion of the dual nature of the group is expressed

in various ways in different theories. We have mentioned the psychosocio-group distinction [24]. In addition, we can speak of the "internal" system and the "external" system [21], the "informal" organization and the "formal" organization [46], the aims of drives and the aims of reality [41], the tendencies toward emotionality and the tendencies toward work [5]. These dualities are not equivalent; i.e., they are not synonymous words referring to the same thing. They are, however, different ways of conceptualizing the dual nature of the group as being simultaneously inner- and outer-directed.

We can now see that the fundamental problem of the group is to satisfy simultaneously both sides of its nature, to meet individual needs and to solve problems (change the environment). The group thus mediates between the psychological, inner-directed needs of individuals and the demands of the environment (as perceived by individuals), i.e., their social and environmental, outer-directed needs.

But when we say the *group* does something or the *group* has a need, what do we mean? Have we now invented some superobject, the group, and given it the biological properties of the insect? In operational terms, this is in some ways the most difficult question of all. The problem boils down to this: the *member* believes there is a group. He feels pressures, expectations, and punishments, and he says they come from the "group." He feels a need to "belong," not in the specific sense of having relationship to particular individuals, but in a broader sense of feeling part of a larger whole. He has a sense of place in this larger whole, and usually he even can state his place in it. When talking to nonmembers, he refers to the group as a unitary body, which has purposes, agenda, attitudes, leadership, and so on; and he defends this body from attack. In the mind of the member, then, there is no doubt that the group exists and that it does have properties analogous to the biological. The "biological" group exists because the members think it does, because they assume the others also think so, and because their behavior is different as a result of these beliefs. And what makes a difference is "real."

The objective observer—if we may invent an idealized role that does not exist—is baffled by the group. All he can put down on his record is that Jim said something, then Jerry, and so on. To such an observer, only overt behaviors exist; some one person is the actor and some other person—usually the one who talks next—responds with further action. And that is it.

The interpretative observer—and this is a role that does exist—goes beyond the objective observer in his selection of "units" to observe. He still tallies individual behaviors, but he conceives of these in sequences and periods of time. During one such period, the behaviors may indicate to him that the participants are confused: they make suggestions but

do not follow them up; they express a range of feelings, from dependency to aggression. During the next period, however, the participants seem to be listening to each other. They summarize their discussion; they ask each other questions; they look comfortable; their feelings are warm. It is as if the same thing had "gotten into" all of the participants at the same time, and this is different from whatever was bothering them earlier. This "thing" does not "cause" all the participants to behave the same way: there is here no Greek chorus chanting in unison; each person is still recognizable as the same person in both periods; each person in many ways is behaving differently from each other person. Yet there is something to be accounted for that affects them all; and since all the participants are affected by it, this thing must be communal.

What about the nonparticipants? Are they also affected by this communal thing? Our cautious observer would have to admit (*a*) he has no data on the nonparticipants except possibly for some nonverbal gesturing which is difficult to interpret; (*b*) people cannot all talk at once, and the selection of those who do talk and the inhibition of those who do not may be opposite sides of this same communal "thing"; (*c*) during the next period, these nonparticipants become the talkers, and the best hunch is that they were getting ready for this during their preceding silence. In other words, not talking is *not* nonparticipation.

The problem now becomes: what is this communal thing? Two points stand out: it is probably something that exists in some way in the minds or nervous systems of the members; it develops and changes through processes of interaction and communication among them. Moreover, since this common thing seems to affect ideas, emotions, actions, and values, it is a total pattern which contains either these things itself or the *anlage* or precursor of them.

All the varied theoretical approaches must deal with this problem in some way. The cultural anthropologist, studying the group much as he would study a South Sea tribe, may find the common thing in basic assumptions or organizing principles in the culture, and these are the key ideas through which he understands the "way of life." To a "structural" sociologist, the common thing may be the "shared" expectations members have for each other's behavior or roles. To the "functional" sociologist, this common thing may be shared interests and purposes. To the field psychologist, this common thing may be an imbalance of "forces," with strains and stresses to which all are responsive. To the psychoanalytically oriented observer, this common thing may be an "internalization" of the group as a common conscience, or as an extension of the individual's ego. To some extent, most researchers find themselves consciously emphasizing one of these notions, but continually assuming additional elements from other approaches as well.

Perhaps we can conclude only that a group is a number of people who think they are a group and act like one. Nevertheless, a "group" is a real thing, even though it is a theoretical construct rather than a physical object. Hereafter, then, we shall use the word "group" to stand for the common "thing" in any or all of the senses just indicated.

The dual nature of the group—what we called its inner- and outer-directed systems—means the possibility of conflict, tension, ambivalence, and ambiguity; and it also means such reactions as anxiety to these states. Moreover, it means efforts to resolve, reconcile, or harmonize these warring elements. The group needs to do this, not only because these states are painful and punishing on the whole, but also because people need the security that comes from a defined situation. In addition, they need a tolerably orderly society so that they can employ their intelligence to guide their behaviors in the light of predictable consequences.

But efforts to this end only spell out some further ramifications of the problem of maintaining the group. The familiar devices are employed: there are shifts in central people, changes in group structure, redefinitions of purposes, activation of different, hitherto merely potential individual needs, shifts in the formal organization, new feelings of intermember attraction and repulsion, and so on. Out of this come changes or reorganization of the culture (usually lagging somewhat behind); and the group moves into the next phase.

Some of these components of change are consciously guided; others occur at "deeper" levels. In so far as man is master of his fate, it is through the conscious use of intelligence; hence, he must either work directly on the elements in consciousness or he must consciously set up conditions in such a way that troublesome elements can emerge into consciousness. This latter method centers around removing the obstacles, such as personal threat and fear, that tend to keep disturbing elements suppressed. Elements for the most part already in consciousness, and which can be dealt with by means easily available, are the objective conditions usually seen as lying "outside" the group. In other words, intelligence may be more easily directed to problem solving or if you like, to achieving the group's publicly stated purposes or tasks. Moreover, this task-activity, with its clear-cut requirements of information to be secured, roles to be played, methods to be used, provides a set of "givens" against which the group can diagnose difficulties and evaluate and rectify its internal conditions. Thus work on tasks provides both "feedback" to the group about its own adequacy as a group and a clear-cut target for channeling expressions of personal feelings. The progress of problem solving, together with the attendant need frustrations or satisfactions, is something each person can judge independently and have opinions about—and this is the grist for the problem-solving mill. Thus in the

group's conscious operations, the outside demands for particular ways of structuring the group tend to be perceived as fundamental; and the internal structure must accommodate to these.

At the unconscious level, however, the situation is reversed: it is the process problems connected with the inner system that most affect motivation, identifications, "place," and so on. The problem-solving operation merely provides the vehicle for these higher-priority "emotionalized" human purposes [50]. In operations at the deeper level, there is no doubt that different individuals become "central" from time to time; Bion [8] has even described one situation in which an absent member was central.

It is at this point we must leave the group. We have attempted to show what the researchers are working with and to indicate what seem to us to be rather generally held or assumed notions about groups. Many things have been omitted, e.g. overlapping memberships, genetic development, group growth, and so on; but perhaps we have indicated the central problems most directly involved in group operation as well as some major concepts currently being applied to these problems.

Research on work-emotionality. We shall introduce the particular research [44, 54] which is the subject of this paper later, under Structure of the System as Thus Far Developed. But it does seem appropriate at this point to note briefly where it fits within the methodological and conceptual framework which we have sketched so far.

The basic dramatic theme is that proposed by Bion: the group is seen as a miniature society which has conflicts within itself over the basic assumptions on which it operates. The two general types of assumptions refer to different major purposes, to "work," in the sense of dealing with reality factors diagnosed as creating problems to be solved, and to "emotionality," which attempts to avoid certain reality factors but which at the same time serves to help maintain the group.

The structure of the group as a series of status hierarchies is dealt with only when the members are themselves actively concerned with problems from this source. The structure of the group as emotional interpersonal alignments comes into the picture as necessary to explain participation or inhibition or interpersonal conflict. The structure of the group as functional relationships comes in to the extent that the need for definition or modification of such relationships influences group activity.

The basis of group formation and groupness in general is "combination" [8] with others in the support of particular basic assumptions. Thus the group is seen as a shifting network of actual and potential subgroups which changes in response to the altered needs of the group.

Individual needs are understood as necessary to explain the ways individuals participate to influence the basic assumptions operating during

each phase of group life. The conditions within the group as a whole activate each person's behavior. Although individual behavior is an expression of individual need, the conditions under which it arises and the way in which it is expressed is symptomatic of group need. In this sense, the individual is always to some extent speaking for the needs of the group.

The group is seen as working simultaneously on task and process problems and as providing the conditions for individuals to work on unique personal problems. Task problems are equivalent to external problems or problems in the external structure, but they come from needs arising from within the group and are projected into the environment (externalized) by action of the group itself. In other words, environmental factors are brought into the group by each individual and they eventuate in group tasks through processes of opinion exchange and group decision. Process problems arise in the efforts of the group to organize itself to work on task problems and in its efforts to maintain itself as a group. The task problems provide the necessary vehicle and frame of reference for processes of group maintenance. By maintaining the group we mean developing and stabilizing during each phase a situation of dynamic interplay such that the most imperative needs of individuals can be met.

Methodologically, these notions lead to observation of group process as the source of the most fundamental data. Behavior is seen as resulting from the momentary mixture of tendencies toward work and toward emotionality. These are recorded in categories developed for the purpose. The observer is emotionally involved, and identifies himself with the group as a whole, its sense of conflict, tension, and so on. In effect, he records the contribution of members to the prevailing emotion-work "climate." Emotion is directly felt and recorded as it is expressed; it is not introduced later as a theory to account for "objective" behavior. The observed categories of each member's contribution are plotted as a sequence in time, and this sequence is divided into "natural units" or phases.

During each phase, the task and process problems are identified. The task problem is usually explicit; the implicit process problem is diagnosed through interpretation. Both formal (task) and hidden (process) agendas of the group during a phase, a meeting, or during its whole life are thus revealed.

The explanation of these group-level phenomena is attempted through use of a second kind of information, namely, the tendencies of individuals (in group situations) to support or oppose each of the possible basic assumptions and also the tendencies of individuals to react in defined alternative ways to such assumptions when they exist in the

group. Tests have been devised for obtaining this information. We have also attempted to explain the group-level phenomena as the interplay between subgroups representing "combinations" of individuals for the support or inhibition of particular basic assumptions. These subgroups are identified through factor analysis of self-perceptual items arranged in a *Q* sort. Some of the subgroups tend to correspond to sociometric subgroups, whereas others do not.

Members' own reports of their reactions to events in group life (e.g., postmeeting questionnaires) are used at points where we are interested in their own theories about what is going on. Such reports are never taken at "face" value; they must be interpreted in light of other data about the needs of individuals in the situations on which they are reporting reactions.

In general, there is a close parallel between our methods of research and those of a somewhat psychiatrically oriented group-centered leader or member. The research group has found these ideas helpful in the practical business of leading groups (especially those for training in human relations), and the hunches of group leaders have been found to be rather readily translatable into hypotheses for study within this framework of concepts and methods.

ORIENTING ATTITUDES

Prediction. Research involves theorizing and reality testing. By theorizing, I mean formulating and systematizing ideas which "explain" our experiences. By reality testing, I mean demonstrating that conceptual relationships correspond to behavior in the "real" (i.e. nonsubjective) world. It is important to accept the fact that theorizing is a way of meeting the experimenter's need for "closure," for "wrapping up" parts of his own experience.

Reality testing, on the other hand, is the effort to externalize ideas hitherto in the researcher's subjective world by showing that they "fit" the experiences of other people as well. The pursuit of ideas, necessarily individual and self-centered, becomes socialized through the socially prescribed methods of testing hypotheses.

Thus the scientific enterprise represents a movement from subjectivity to objectivity. If an idea survives after being squeezed through the scientific wringer, then it is entitled to be accepted as a present approximation of the "truth" within a carefully described field of designated phenomena. Such an idea will have currency until another, more fashionable type of wringer that would tear the idea to shreds is perfected, or until another idea, no more "true" but more convenient or useful, comes along, or until the idea is incorporated in larger systems of ideas.

To carry through the two basic parts of research requires two quite different roles: the researcher acting as a human being to get closure from his own experience; and the researcher operating as a member of a group of scientists in whose behalf he must submit his brain child to cold test.

The crucial test is *prediction*. The classical question runs: given situation at time a , what will it be like at future time b ? The operations required to answer this question are somewhat as follows: study situation X, name its "parts" and describe the part-to-part and part-to-whole relationships within the situation: this is called determining the structure of situation X. Next, study this structure, and ascertain that it contains (as all human systems must) certain suspicious-looking stresses and strains: this is diagnosis or analysis of growth tendencies as revealed symptomatically in present dislocations. Consider what sorts of rearrangements of parts or changes within parts would reduce the stresses and strains: this is the making of models which portray the system in more "stable equilibrium." Then estimate the probability that each of these rearrangements under the given conditions might occur. (This is equivalent to describing the potentials for flow of "energy" in different directions within the system, but the probability-states concept seems less likely to embroil us in bad analogies to physical science.) Having selected the most probable state, the next question is: how far will it have been realized by time b ? The answer to this question, preferably conveyed in the form of a picture of the structure at time b , is our prediction, and all that remains is to wait until time b and then look for evidence that the predicted and observed structures are alike.

Unfortunately, even successful carrying through of these steps is not enough. The researcher must also be able to make explicit all the conceptual relationships he uses and show us the relationships between these and his data.

A comforting human fact is at the same time awkward for science: the wisdom amassed from experience is greater than the contents of explicit knowledge. This makes it possible to short-circuit one or more of the steps required by our idealized model. The successful researcher on groups usually arcs across these steps in a flash of insight. This may make him a bad scientist but an excellent companion for a seeker-after-hypotheses who knows a good thing when he sees it. Denied the easy and "human" way out, the scientist may delimit the experimental situation to a few measurable aspects whose relationships are clear-cut (as in questionnaire studies of perceptions reported by group members). He may limit his own aspiration to doing a thorough job with one step, leaving the rest to others (as in "perfecting" a measuring instrument). He may take on the whole job, repeating it over and over and gradually

making the concepts more and more explicit as the whole framework becomes clearer (as in developing a psychoanalytic theory of leadership). Or, finally, he may reconsider the model for prediction and reformulate it to fit each situation.

To the extent that research is learning, the first three approaches overlap. The perceptionist's experiments do not seem empty to him because he has an implicit frame of reference that enables him to consider perceptual data as indexing more important underlying dynamics. The test constructor likewise has some broader set of principles which enable him to try to validate his instrument. And the developmentalist must pay his respects to the others along the way. These conditions stem from the facts of learning: that behavior arises out of a broader subjective world and that adaptation requires conscious contact with reality. Nevertheless advance would come faster if we could free ourselves to encourage the implicit to become explicit and if advocates of the various approaches could be a little more confident of their need for cooperation.

Probably, use of the final approach suggested will make the researcher as learner most effective. Let us reconnoiter the possibilities of making our experimental designs more suitable to the degree of fragility or case-hardening of the researcher's concepts and intentions.

The possibilities may be set up logically through a simple analysis of the number of kinds of relationships that can be developed with reference to a situation. We offer, at a rather high level of abstraction, one such analysis to show what we mean.

1. Prediction may be made to something in the past, present, or future, depending on whether one is concerned with "causes," correlates, or "effects."

2. The "something" predicted from and/or to has the character of either structure or process.

3. Structural variables may represent either wholes or parts of structures.

4. Process variables may represent either events (homogeneous units of interaction over time) or specific behaviors (seen as parts of events).

5. On this basis, there are 34 types of predictive statements. The breakdown is

- a. From a structure to one or more parts, an event, or one or more specific behaviors.
- b. From one or more parts to a structure, an event, or one or more specific behaviors.
- c. From an event to one or more behaviors, a structure, or one or more parts.

- d.* From one or more specific behaviors to an event, a structure, or one or more parts.
- e.* This makes 12 possibilities.
- f.* Each possibility may refer to past, present, or future, with the exception that event-to-event and structure-to-structure predictions (whole-to-the-same-whole) are identities rather than predictions. Thus we have 34 types of prediction.

6. The purpose of prediction is to test instances of application of general hypotheses. Hypotheses are statements of relationships between parts and whole, whole and whole, and/or parts and parts. The relationship may indicate antecedence, simultaneity, or future consequence. Thus there are as many forms of hypotheses as there are types of predictive statements.

This perhaps suggests my attitude toward prediction: experimentalists should analyze concepts dimensionally to see what formal relationships are involved in our hypotheses. Secondly, we should validate our hypotheses by creating prediction situations (experimental designs) of the appropriate types. Systematic development of our own metatheories along these lines would enable us to convert the scientific wringer from a fashionable gadget to a feedback device for improving and developing ideas, regardless of their stage of maturity, fragility, or harmony with current fashion.

These are the major types of predictive statements made by our own research:

1. Prediction of present structure of personality as a whole from a limited number of selected and quantified aspects (parts).
2. Prediction of future functional role (participation pattern over a designated period of time) from personality structure.
3. Prediction of present events (underlying group problems) from observed specific behaviors.
4. Prediction of specific behaviors within future classes of events (modes of group operation) from present personality structure.
5. Prediction of group structure (future whole) from present personality structures (parts).
6. Prediction of future events in group life (whole, modes of operation) from present group structure (whole).

Level of analysis. Whether as structures or events, wholes are not directly measurable. Wholes are theoretical constructs. By using them, one can organize a large mass of specific information and give it coherence. If their internal organizing principle can be made explicit, these constructs also can be used as bases of prediction. Organizing principles within structures are such notions as the existence of differ-

entiation of members along interrelated dimensions of prestige, influence, resistance, etc. More abstractly, the structure may be given coherence through some concept of stability, scope, or extent (e.g., life space, social space). Organizing principles within events tend to portray basic tendencies to move in some direction; thus these principles are projected, "as-if" purposes or dynamic themes as discussed earlier.

The concept of the whole must represent a much lower degree of specificity than the data from the parts. Presumably the course of investigation is designed to help us oscillate between data and constructs. In this fashion, we move up and down the ladder of abstraction from the observer's sensory responses to organizing ideas which pattern these responses, to specific responses in defined (controlled) situations, to further patterns, and so on. If one looks at the design in terms of the phenomena to which the researcher's behavior refers and presumably corresponds, then the same movement is seen moving from parts to whole to parts to whole.

This kind of movement is the essence of finding "meaning" because these levels have different properties. The specific level is descriptive; the construct level is explanatory. And both description and explanation are required for "meaning," i.e., for the kind of internalized insight that we call comprehension. Laws of human behavior can be found only at the construct level; e.g., frustration leads to aggression. These are "whole" emotionalized states rather than specific behaviors. If you ascertain that a man is frustrated, you can predict not what he will do specifically, but what *sort* of thing he will do. If you add further information about the *sort* of man this particular individual is—information about his typical ways of dealing with stress situations—then you can narrow the prediction to a few kinds of response tendencies typical of this person. If to this you add information about specific aspects of the situation, and if you have a theory about how the person selects which of the possible kinds of response he will produce, then you can predict his behavior more specifically. For completely specific prediction, you would have to know all the behaviors in a sequence leading toward the precise moment you are trying to predict to; and at this point prediction and observation would be practically synonymous. In general, the more particular the prediction one wishes to make, the more principles he must know, the more specific the data he must have, and the shorter the time over which he must attempt prediction.

In our own work, the basic categories are of behavioral tendencies whether they be seen in personality (intraindividual) or in observed behaviors (interpersonal). We use eight fundamental categories; four refer to "emotionality" and four to "work." Thus our conceptualizing is at a rather high level of abstraction, with rather coarse instruments

as compared, say, to the use of up to 50 "traits" or 12 "dimensions." Our description is at a very low level of abstraction: we record every word spoken, and we rate every contribution of an individual.

Models. Behavior is purposive. The purposes may be conscious, as in problem solving, or implied, as in the consistent effort of a person to "dominate." Behavior is also part of a stream of experience and particular behavior is an emergent event within a larger personal-social-physical system. Thus we speak of a particular behavior as "symptomatic" of underlying conditions or states. When we respond to a behavior of someone else, we are responding also to whatever the behavior signifies to us.

Hence, any category for classifying behavior—particularly at a relatively high level of abstraction—is likely to imply more than it specifies. It is also likely to imply somewhat different things to different users of the category. We acknowledge this when we work to avoid using "color" words in our operational definitions, but what word does not have at least a faint hue for someone? The fact that most descriptive and most active words have color illustrates the difficulty of plucking an overt act out of its context of relationships and sentiments.

Faced with this difficulty, the category maker has available two courses of action. He may concentrate on physical description, which he can do only by enumerating all the possible instances of the category and then using this as a check list. Secondly, he may describe the sort of behavior he means, and then say what he means by it: under what conditions it is likely to arise, how others may react to it, what may be the apparent intention behind it, and so on. The first procedure amounts to describing the physical and audible aspects of behavior; the second amounts to describing action in a situation. Each has its own drawbacks.

Thus, when we try to categorize behavior through its "overt" aspects, we find that it is impossible to list all the instances of the category. Many behaviors are not pure instances, although they have some elements of the aspects we have listed. To list many instances within a general category does require some high-level concepts. Fourthly, if the observer has only physical aspects to guide him, then the context may be too small for objectivity. He tends to fill out some of the missing dimensions and then make the judgment fit; for if the cues one is looking for are too minimal and unmeaningful, it is easy to overlook or misperceive them. For example, we found that our observers agreed more completely on the judgment of the amount of work per contribution during a meeting than on the number of times each person spoke. Yet the latter item is perfectly "objective" and the former requires a relatively complex judgment.

The difficulties of the second type of category, in which both the behavior and its context are spelled out, are first, that one cannot be sure just which of the criteria were used to classify any particular behavior. The theory must be fairly fully developed before such "full" categories can be provided, and, in that case, the category is not really just a category of overt behavior: it is actually a hypothesized dynamism. Finally, since the category does not denote particular behaviors specifically as belonging or not belonging to it, the observer must do more than observe and check: he must interpret as well.

When one tries to use categories of the first sort, they get perverted into the second sort. That is, observers elaborate the simple description with their own private meanings. They cannot succeed in attempts not to respond to affect and intention. Endeavoring to cut off their involvement in the phenomena leads either to psychological withdrawal of the observer, which tends to produce carelessness, or to efforts to suppress and deny involvement—which tends to distort perceptions. Hence, we have preferred to fill out the context as part of the definition of the category, and simply to accept the fact that we are not checking a specific behavior at all; what we are checking is a symptom of a general mode of response.

This means that our categories for behavior are actually models of different modes of response or action. They denote not only a class of specific actions in a generally defined situation; they also connote "as-if" purposes and expected consequences. Thus, building categories of "behavior" is actually a process of developing subtheories about dynamic processes which are—and which must always be—the actual objects of any investigation of human behavior.

We conclude, then, that observation is an activity which involves thinking along with the group. The categories of observation, however defined, become distinguishable "dynamics" or modes of adjustment or accommodation. If we attempt to make the definition of the categories correspond to the definitions we actually use, then our categories become alternative models.

As we move to higher interpretative levels, we find increasing recognition of the use of models. For the higher the degree of abstraction, the more wholistically we understand what is going on, especially in regard to the social directions (as apart from the psychological dynamism) toward which we think the system is moving.

As social creatures, at this point (if not earlier in some "moralistic" sense), our evaluative reactions come into play. A scientist may claim that he has no evaluative reactions to such things. But probably it is more useful to recognize evaluative reactions and analyze them, for the fact that we feel as we do provides another kind of datum for

analysis and another route to comprehension of the situation. Just as we cannot avoid using models for activity, so we cannot avoid using models for structures and systems of part-whole relationships. Words like cooperation, competition, social order, freedom are not simple concepts; they are foci for the association and organization of a great many relationships, models toward or away from which we perceive the system to be moving. It is the existence of these models, and the values we impute to them, that causes us to have evaluative reactions in the first place.

We regard our basic categories of emotionality and work as alternative models for modes of adjustment and for states of group culture. We look at the behavior, but we see it in terms of a situation and in terms of probable drives and group needs.

Comprehensiveness of empirical reference. The study of a face-to-face group is the study of interaction between personality and culture. The group is an intermediary body for bringing these two kinds of phenomena into relationship. The group's mode of adjustment represents a set of assumptions which govern the mediation process at any particular time. The study of groups is also the study of man and society; events in group life illuminate the biological-psychological nature of man and the sociological-anthropological values and organization of the larger society. Hence, it is scarcely surprising that a goodly number of the men investigating groups are also concerned with the possibilities of moving toward a larger, all-embracing science of human behavior.

Our concepts have "surplus" meanings in terms of universals. The concept of "dependency" as a mode of adjustment is comprehended in a model which incorporates our ideas about the intentions and needs of a dependent person along with ideas about the system of group control and the group's goal directions; these are related in the model to expectations about the immediate situation and to trends in the larger society. No model is ever fully spelled out. Some of these ideas are explicitly related, as in mathematical laws, but some stand at a verbal enumerative generalization level, whereas others remain at various distances below consciousness, waiting for the proper combination of cue and drive to make them emerge into consciousness. The value of the model lies in the very fact that it is full of surplus meanings; it implies potentially a great deal more than it ever states. In my opinion, research is concerned with discovering these surplus meanings, making them explicit, reorganizing the model as needed to tighten its structure through penetration of its organizing principles, and, finally, using these principles for prediction. The predictions that pay off at the 1 per cent level may contribute most to the organized body of science (at least they are more likely to be "accepted"); but the 20 per cent level can be far more

interesting. For it may show that we have correctly grasped the general idea but are overlooking factors which invite investigation although we had not thought of them.

All of this means that the "comprehensiveness of empirical reference" is potentially very broad. Thinking about groups embraces almost the whole range of ideas about individuals and society. And every human interaction—intrapersonal, interpersonal, group, and community—presents the opportunity to draw on the complete range of ideas. Thus Homans [21] applies the same over-all model to a small face-to-face group on the one hand and to a whole community on the other. After all, science must assume that the whole world is organized along the lines of discoverable principles, and that each part, in some way or another, fits in with these principles. Otherwise it would be impossible to generalize from the situation studied to other situations. In the early stages, when we lack confidence in our general principles or, perhaps, in the language we use to try to express these principles, it is reasonable to demand that we restrict our generalizing to the cases we have actually studied. This is the counsel of caution and the requirement of public demonstration, it is not the way we think nor does it express correctly the long-range goals and the basic assumptions of science.

Degree and mode of quantitative and mensurational specificity. The problem of language is critical. We must communicate the central idea of any concept by surrounding it with qualifying phrases, as ifs, ands, and buts. Our grammar, with its simple subject-object relationships, and its adjective and adverbs to modify these relationships, seems better suited to discuss objective phenomena than psychological events. We have much more the feeling of "pulling phenomena apart," and of "reifying" images like the image of a group when we operate in the human sphere. Time relationships are still more difficult as are ideas of cause and effect. Language may derive from the need to describe objects. It offers a serviceable substitute for pointing, but in the realm of human behavior, language may be less adequate. Thus Harry does not react to Joe. This our language can handle easily. But it is apt to falter in dealing with complexity. Instead of reacting to Joe, Harry reacts to Harry's feelings, which are mobilized in a situation in which Joe is also present, as a result of some behavior of Joe's which, in the context of the general relationships of mutual expectation and attraction between Joe and Harry, cues off the mobilization of feelings specific to the situation.

To meet problems of this sort, one may invent new words and try to make their meanings stick. One seeks to develop a kind of shorthand which, it is hoped, will be both more terse and more precise than the phrases it renders obsolete. Or, one may use an entirely different kind of language—the language of mathematics. Words are the language of

essences and entities; mathematics is the language of relationships. In the sense that we probably respond to behaviors and activities and then try to define the entities that fit into these relationships, mathematics seems more suitable. Thus, for example, we do not "know" Harry's personality. This is merely a theory to account for the fact that there are certain consistencies in the way things happen when Harry is part of a situation and that these consistencies of relationship between Harry and other people, objects, values, etc., are located in Harry—or rather, in Harry's personality. Harry thus is the locus of one end of an infinite number of relationships; and it is the relationships we experience. Harry is just a theorized entity—even if we can borrow money from him.

When we talk about relationships we are talking the language of mathematics, and the mathematicians may have grounds for their constant wonder why we do not go the whole way. But going the whole way is not easy. It requires the development of a set of dimensions which represent continua and which, taken together, enable us to build even the most complicated variables. Thus in mechanics, the three fundamental dimensions are distance, mass, and time. These are all measurable along continua, and more complex variables such as inertia and force are synthesized as unequivocal relationships among these dimensions.

The effort to find univocal relationships implies a deterministic view of behavior: if we could measure the magnitude of a number of chosen variables, and if we knew the mathematical relationships of covariation among them, then, given new values (as at another time) of any one of them, the values of the others could be computed. And, what is more important, they would fit the new facts in the situation. As scientists we must believe in determinism; that is what science is about. But when we deal with the human system there are such embarrassing factors as feedback, which modifies the system as a function of certain consequences of its own operation; choice, which means that more than one alternative route can be perceived; analysis and consciousness, which mean that more alternatives can be felt and formulated, and so on. All these processes must be reduced to lawful behavior and incorporated also into the determinist's system. Finally there is that old friend of the statistician, the "randomly distributed uncontrolled factor," which I tend to think of as the ignorance factor, but which at least calls our attention to the point of view that prediction is possible only within limits and that whatever prediction is made should be accompanied with a probability tag.

The usefulness of mathematics as a language raises the fundamental question whether by adding more and more deterministic elements (including the determination of probabilities) one can ever approximate the facts of a human system. Certainly we can only expect

the mathematicians to find more and more ways to remove from any given event all the systematically varying elements, and to reduce to an increasingly manageable size the part of the event's determination which cannot be put into formulas. But we have still to see how well Korzybski's [25] notion that "the structure of the universe and the structure of mathematics is the same" will stand up in human affairs.

We have said that we react to dynamisms which are constituted by relationships among the behaviors involved; and that it is easier to sense the relationships than to define the entities, e.g., Joe and Harry, thus related. It is easy to know that a group today seems to be behaving very differently than it did yesterday but how do you measure a group or group behavior? Group and group behavior are both constructs. This means they must be synthesized from a host of smaller interrelated measurable elements. What then do we measure? One researcher measures perceptions of members, which he can then intercorrelate and factor-analyze. Another counts the number of times he sees instances of behavior of up to a dozen or more "types." A third researcher pays attention to "group" productivity, by which he means and measures such things as the time needed to work a puzzle, or in industry, the number of relays assembled during successive specified periods. The "empirical" approach thus boils down to measuring everything you can and hoping there will be relationships among these things; whereas the theoretical approach involves formulating, almost by aesthetic criteria, the idea of different kinds of movements, trying to express them in words, and then hoping that some way can be found to measure what one thinks he has in mind. In our opinion, the best strategy involves both approaches: constant effort to guide empirical trial and errors by ideas of sensed relationships, and, when a measured relationship holds firm, effort to see what it means within the larger theory.

In our own work, our measurements are frequencies of appearance of different categories of behavior in test and group situations described as well as we can describe them. Our measured quantities are only indices for the most part—symptoms of a state of affairs which can be comprehended only through theoretical reconstruction. The degree of specificity depends upon the question we are investigating, which may range from "under what conditions will Johnny participate in x type of discussion" to "what changes occur in a training group over the course of 15 meetings?" In both cases, the raw data are similar, but the collation of the data, the use of sequential analysis and field graphs, and the balance of qualitative to quantitative operations with the data—all these depend upon the question.

In general, the specificity desired is obtained by taking into account an appropriate number of indexed tendencies. Uniqueness represents, in

effect, the convergence of a large number of different tendencies at a particular time and place.

Formal organization of the system. A researcher is a human being engaged in inquiry. He must have a goal, a frame of reference, a means of generating hypotheses from the frame of reference, and experimental methods for testing his hypotheses. Finally, he must develop a body of theory in which the terms are defined through their relationships to each other, and for which the surviving hypotheses provide reality contact adequate to support the whole system.

When this last or theory-building stage is accomplished, the researcher has summarized his whole enterprise within a formally organized system. From such a system, he and others can "deduce" many further hypotheses which further specify the system. If the concepts in the theory were constructed from a limited number of operationally defined dimensions, then new concepts (and new theory) can be added indefinitely through the systematic consideration of all the possible ways of mathematically combining these dimensions—as in the case of CGS system in physical science. At this point, the system might be said to have attained relatively advanced "hypothetico-deductive" status. (Whether this dimensional breakdown and build-up is actually possible in social science, I leave in the realm of questioning.)

In viewing research as inquiry, we are implying that the researcher guides himself consciously through the steps given earlier. Current fashion may lump everything leading up to the statement of hypotheses as "creating hypotheses" and regard this as the domain of private inspiration. Before we agree to this, however, we should study a bit more fully the relationships between "frame of reference" and "hypotheses"—and also the relationships between physical and social science. It is possible that we have been too long and too much influenced by Kekulé who, so the story goes, became frustrated, withdrew to a beer garden, drank too much, and, in a dream, saw six snakes grab each other's tails in their mouths and thus form a benzene ring. Many major contributions have been made to physical science by men in their twenties and thirties, whereas contributors to social theory average at least twenty years older. I interpret this to mean that the frame of reference in physical science is easy to communicate and internalize, so that one can begin life as a theorist; in social science, it takes about twenty years more experience and maturity to develop the background for theorizing.

During this long training period, one finds his goals and develops a personally meaningful frame of reference. This frame of reference is made explicit to oneself as a series of propositions about human behavior—a set of agreements with oneself to look at human behavior from some defined point of view. Such agreements can carry conviction for

a person only if they come from his experience of working with and puzzling over the phenomena he is studying. And I believe that, unlike theory building, the development of proposition proceeds most effectively through interpersonal stimulation, speculation, and challenge.

The problem of characterizing where the field of group study now stands in its development is complicated by the fact that no two researchers are at the same point. At the present time, it might be valuable to take each other's propositions seriously. It is much easier to work together at this initial stage than later, when researchers consider that they are well on the way to formal systems and therefore have something to feel defensive about. As I read the nicely organized systems of hypotheses, derivations, and corollaries so far published, my major feeling is that, regardless of the amount of supporting research, I wish I knew more about the origins of those systems. With more metatheory—the propositions which summarize the researcher's preconceptions—we would probably find each other's sets of propositions rather similar, even though expressed in quite different vocabularies. Given such reassurance, each would investigate in his own way but with expectation that the resulting hypotheses could be fitted together.

It is the *propositions* whose fruitfulness has been tested in theory building that ultimately become incorporated in man's cultural view of the world, even though it is the *theories* themselves that have most immediate value for problem solving and social invention.

With regard to the particular research to be discussed in the remainder of this paper, I should say that a series of propositions has defined our approach and has led to a method of analyzing group processes. As this method is applied to experimental situations, a constant stream of questions is raised, and these are tentatively answered in specific hypotheses. Further data are considered in an effort to test the hypotheses. Generalizations emerge as summaries from many such experiences, and on the strength of these, the propositions are worked over. Through this method, the set of propositions is gradually transformed into a body of theory of rather comprehensive scope (since the propositions were quite general to start with). Thus we seem to be reaching for a general theory of human interaction rather than for a special theory of "group behavior." The experimental situations focus on groups, however, because, as mentioned earlier, it is in groups that one sees an acting out of the relationships between personality and societal factors.

STRUCTURE OF THE SYSTEM AS THUS FAR DEVELOPED

The nature of the groups studied. However generalized a system may ultimately become, it begins in the need of the researcher to comprehend

particular phenomena with which he is involved; hence the basic terms of the system are necessitated by these phenomena. Although we believe that the system to be described and analyzed is applicable to the whole gamut of face-to-face operations of all sorts of groups, we must admit that the reasons for developing the system along present lines become clearer when one can visualize the sorts of groups we were most concerned with.

Ours have been "human relations training groups." These are groups of 12 to 25 adults who have come together to learn "what goes on in group" and "how to participate more effectively in groups." The method of training in these groups has been hammered out in a variety of two- and three-week workshops conducted each summer by at least a dozen organizations in the United States and Europe. The basic ideas of training were first put into practice at the National Training Laboratory, held at Bethel, Maine, in 1947; and most of the other workshops have been conducted by staff members who have "been to Bethel." Although no two "trainers" would portray their methods of training in exactly the same language, there has been basic agreement on the fundamental basis of operation: that the group studies its own problems of operation; and that these problems arise from the stresses produced by lack of structure or definition of the situation in which the group finds itself. Some trainers produce this lack of structure by taking an almost completely passive role. Others may alternate withdrawal during some periods with strong leadership at other times. Still others follow certain criteria for deciding when and how to intervene.

These groups are designedly heterogeneous with respect to occupation; "shop talk" is impossible. The one thing the members have in common is an interest in group operation. There are no demands imposed on the group in the sense of particular problems they are to solve; they must produce their own agenda and deal with it (or not) in whatever way they can with whatever sort of "help" the trainer gives them. From time to time they are led into description and analysis of their own experiences in trying to make or avoid decisions, to deal with nonparticipants, "problem" members, or leaders, to plan and carry out activities, and to study the effects of various individual behaviors on the course of the meeting.

The groups usually meet for two to three hours at a time, and they usually hold 10 to 15 sessions during the workshop or "laboratory." Frequently there will be an assistant trainer, and the relationships between the two staff members often contribute to the "dynamics" (I am tempted to say "problems") of the groups. The private motivations of the members range all the way from finding out "whether my analysis seventeen years ago was successful" and learning "how to be a more effective

wheeler-and-dealer" to developing self-insight, skill, and understanding of group phenomena [50].

Our decision to study these groups was based on the following facts: since there is great permissiveness, a wide range of behaviors can be seen. The group's effort to understand itself produces much data of the sort the researcher needs; furthermore, the groups change very markedly over the course of their meetings.

The development of postulates to guide research. From our vantage points of trainer or observer in such groups, we began to develop rather strongly internalized feelings about what would have to be involved in the effort to understand them. These "feelings" have gradually become explicit as a set of postulates, and they constitute the metatheory of the system. The postulates, given in the first chapter of the Stock-Thelen monograph [44], are

1. Units of interaction exist.

2. Group life can be seen as an adaptive process; and our accounts of it are descriptions of the changing stresses in the group and of the ways in which group members respond to these stresses.

3. The emotional aspects of group life, and particularly the use the group makes of its "feelings" will be direct evidence for diagnosing tensions and the stress conditions which give rise to the tensions.

4. "Individual" factors (e.g., in "personality") will be categories descriptive of stresses to which individuals are sensitive and of means individuals use to deal with these stresses.

5. "Group" factors exist by virtue of the interactive networks among individuals rather than by virtue of what each individual independently "as a person" brings into the group.

6. The nature of the problems for investigation is given in the interaction between "personality" and "group."

These postulates are both empirical and strategic. In the sense that they express conclusions from a great deal of firsthand experience, they are empirical. They are strategic in that they are required bases for studying the phenomena of group interaction. The first postulate is required to make scientific study possible at the molar level. The second is required to give structure to the models which we must inevitably use. The third is a commitment to a psychiatric approach. The fourth and fifth indicate criteria for a basic distinction to be made throughout the system. The sixth points toward what we consider the fundamental nature of the phenomena to be studied, or toward the kind of study which we think will throw light on all the preceding factors so far implied.

These postulates are without "content." They are simply agreements with ourselves as to how we are going to approach the study of groups.

They may be compared with the other approaches mentioned earlier under Current approaches.

Propositions about the nature of phenomena being studied. Over the years, research guided by these postulates has led to an organized body of propositions from which hypotheses could be generated. The continual development and modification of these propositions has been a major goal of our work. The more specific theoretical concepts and researches will, we think, be most intelligible when seen against the background of these propositions.

Since we think that the study of "groups" is only a selected aspect of the study of human behavior in general, we first offer a set of propositions about the latter. This then provides a frame of reference within which the propositions about groups can be formulated. The following discussion of our propositions is quoted from the most recent summary [52]:

Propositions about human behavior in general

1. Man is always trying to live beyond his means. Life is a sequence of reactions to stress: man is continually meeting situations with which he cannot quite cope.²

² Proposition 1 conceives behavior as purposive. If purpose be assigned to man as actor then there is required further the concept of something acted on, e.g., environment. From this distinction of inner-outer flows also the possibility of the self-concept as distinguished from the object- or other-concept; and also the acceptance of a priori realities apart from man. A rigid phenomenological view is thus inadequate, as is also a strictly interactionist view *if* the interacting entities are alleged to be similar in kind.

We see the group as a whole as a system surrounded by an environment and containing individual subsystems. "Personality" is the term for the unique patterning of drives or predispositions of an individual subsystem; "tension" is the term for the tonus or state of mobilization of drive-pattern. The group as a whole is a "social system" which exerts control over interpersonal and person-environment (or, more precisely, Dewey's internal-objective) interactions. This control is exerted through the group "culture," consisting of agreements, perceptual biases, values, threats, etc., which are imputed or ascribed by the individuals to the "group" (as superego) for the sake of maintaining order to the extent required for meeting individual needs and accomplishing convening purposes.

These public or task purposes are achieved through attack on problems to be "solved," that is, through taking action to change particular conditions perceived as lying "outside" the group. To bring about these changes, the group must define and accept two kinds of reality demands: (a) demands for a particular character of action dictated by the "logic" of the problem and directed against the conditions to be changed; and (b) demands for reorganization of the culture so that the necessary participant roles can be developed and the needed human resources mobilized. This latter problem is complicated by the existence of many internal-objective relationships (such as loyalty to one's ethnic, class, family, or institutional groups) which are to be maintained while changing the particular internal-objective relationships whose unsatisfactoriness led to identification of the problem

2. In stress situations, energy is mobilized and a state of tension is produced.

3. The state of tension tends to be disturbing, and man seeks to reduce the tension.

4. He has direct impulses to take action, and there appear to be a limited number of kinds of impulses (or drive-actualizations). Bion proposed four major purposes or needs of groups and societies to which impulses contributed, and he labeled these fight, flight, dependency, and pairing. We have since found that the same categories can be used to describe tendencies within the personalities of individuals.³

5. Impulses may be translated directly into action. This may reduce the tension and render a person temporarily incapable of further reaction to the initiating stress. If the stress has objective basis in "real" danger, then the person remains in danger, and the behavior is nonadaptive. If the stress is projected from the subjective domain (such as a threat to the self-concept), then the emotional discharge may be a prelude to reflection; and the behavior, although not itself adaptive, may make adaptive sequelae possible.

6. Direct *acting out* of impulses has varying consequences, depending on the nature of the impulse. Pairing increases adequacy to cope, without reducing objective dangers. Dependency neither increases nor decreases adequacy nor removes the danger; its effectiveness depends upon whether the sought protection is forthcoming. If successful, fight destroys the danger, but it also tears up the lawn and makes enemies out of middle-class persons. Flight gets one out of danger without increasing adequacy or removing stress from the situation. From a long-range point of view, all these kinds of acting out are mostly nonadaptive because little or nothing is learned from the acting-out experience.

and purposes. These "hidden" or "process" problems are products of the group as a system—they come from the social interrelations within the group, and not from the internal individual subsystem per se.

In our view, the demands of the "hidden" problems, like the demands of the task problems, result in stresses lying outside the individual subsystem. The group may or may not have public awareness of a particular stress; different members may respond in different ways and have different thresholds of sensitivity to a particular stress. But the "underlying condition," capable of mobilizing each person's tensions at a given time, is comprehended as a hypothesized stress.

³Our confidence in the generalizability of these concepts has been increased through perception of an evolutionary basis for four kinds of impulses. This is discernible in La Barre, *The Human Animal* [26]. Fight and flight impulses are as ancient as the nervous system, and predate the present species of man. Dependency and pairing impulses probably developed much later as part of the psychic equipment for maintenance and reinforcement of familial and societal (or communal) relationships.

7. Impulses may be temporarily thwarted or blocked, and the emergent feelings or other behaviors diagnosed. The nature of the stress is made known by the behaviors it tends to engender. Instead of acting out, there is *inquiry* or "reality seeking." Behavior is mediated by thought processes in which previous experience is brought to bear, and alternatives formulated, selected, and evaluated in action.

8. All human events contain a blend of acting-out and inquiry modes. There is acting out in the sense of spontaneous, involuntary expression of impulse; there is inquiry in the sense of developing awareness of factors in the situation and in the sense that something is learned from experience. Bion's concepts of work and emotionality are essentially concepts of inquiry and acting out, respectively, referred to the group as a whole.⁴

9. We note that man is capable of using both acting out and inquiry to meet the stresses of trying to live beyond his means, and we anticipate that "successful" adaptation, in both short- and long-range terms, in each situation requires a particular blending of elements of acting-out and inquiry modes.

10. The major theoretical question with respect to human behavior in general is what conditions tend to predispose men toward modes of acting out or inquiry; what is involved dynamically in producing and maintaining an adaptive blend of the two modes?

In our research we are concerned with this question primarily as applied to the behaviors of groups of people rather than of single ("isolated") individuals or communities. We have further stipulated that we shall use (initially, at least) the terms suggested by Bion, and that our method of investigation is to be experimental and observational.

Propositions about the "group." In general our predilections have been toward an interactive or dynamic approach. Behavior is not literally a *response*; it is an *event* which arises out of a complex system of part-whole relationships. By "personality" we mean the tendencies for the individual to be involved in certain kinds or qualities of events. Psychologically, at least, "individuals" are the loci or centers of strains within the total system. The relief of strain within one part of the system tends to cause strains in other parts, and this communication or transmission of strain is mutually influenced by properties of the system as a whole. According to Bion, the most significant property of the group as a whole is its "basic assumption of group purpose or need." For about this basic

⁴Bion suggests that some amount of work is always present, but that emotionality may or may not be present. Bion is dealing with the culture of a group rather than with the behaviors of an individual, with molar rather than with microscopic episodes. Individual affect is expressed even though the culture of the group may be work-oriented.

assumption the group organizes its expectations, standards, and roles (i.e., its culture). As researchers, then, we are concerned with (a) the group-relevant tendencies of individuals, (b) the way these tendencies produce a matrix of forces in the group, (c) the dynamics through which the "basic assumptions" emerge from the forces and shift from time to time, (d) the characteristics of the control system developed to implement the basic assumptions. Substantively, we are especially interested in these matters as they relate to the central theoretical problem of dealing with simultaneous capacities and tendencies toward "acting out" and "inquiry" with each change of the stresses internal to and imposed on the system.

1. Each person has the capability for meeting stress by "acting out" and by "inquiry." The capabilities differ from person to person.

2. Which particular capabilities or tendencies will be actualized in the behavior of a particular person depends in part upon the situation in which he finds himself. There is, however, enough consistency in his behavior from situation to situation that he is recognizable as the same personality.

3. Persons come together in the expectancy of mutual benefit in coping with objective problems and meeting their personal needs.

4. When persons get together, tensions are mobilized⁵ and interaction results. Out of the interaction emerge mutual identifications which determine the characteristics of "groupness," including a social order and structure.

5. The social order exerts control over the interactions among individuals and gives the interactions a discernible pattern and sequence; this in turn can be comprehended as necessitated by the group as a whole.

6. This pattern and sequence change in character from time to time, thus creating the appearance of different units or phases of interaction. The organizing principle for interpretation of each phase is that the group culture has shifted distinctively to a different configuration of "basic assumptions."

7. The culture-units differ in the quality of their blend of "acting out" and "inquiry"; hence they differ in the nature of their contribution to the group's adaptation to the "inner" and "outer" stresses which were present initially and which are created as the members live together.

8. The intensity of stress developed in each situation during the release of tension depends upon the extent to which the mode of tension release is "acting out." "Acting out" tends in itself to be nonadaptive, but it builds stress; "inquiry" tends to be adaptive, but it reduces tension with the minimum development of stress. The problem of the group

⁵ Consider, for example, the fact that rather clear sociometric differentiations are made during the first few minutes of a meeting of strangers.

as a whole is to maintain the "appropriate" blend, balance, or oscillation between these two modes of behavior.

9. As the group continues to meet, individuals adapt to the group and they adapt the group (culture and basic assumptions) to the individuals. Thus changes occur in the modal tendencies of the units of interaction. The amount of change depends primarily on the extent to which inquiry is the dominant mode, for inquiry is associated with learning. The amount of "group growth" is primarily determined by the amount of energy flowing into inquiry components of adaptive process.⁶

10. In general, the potentialities for amount and adaptiveness of cultural development, and the range of "problems" (stresses) with which it can deal, are limited ultimately by the "composition" of the group. The extent to which and the rate with which the group actualizes these potentialities depends upon its "leadership," i.e., its development of means for controlling and selecting and actualizing needed contributions. In view of the basic theoretical problem, optimum leadership would strike a balance between encouragement and support of direct expressions of affect (so that the existence of stresses could be known) and diagnosis and bringing into awareness (through problem redefinition) of the factors giving rise to the stresses to which the group was reacting.

The research tasks. The propositions listed above developed along with the research investigations; the formulation presented here actually represents reflection on a great deal of the experience to date. We shall now move to a somewhat lower level of abstraction and with the research operations and strategy of investigation through which the theoretical concepts have been gradually developed and clarified [47, 44, 54].

The first research task was to devise some way to record "what goes on in a group." We wanted a scheme which would enable us to see what each individual had contributed, what "phases" or units of interaction the group went through, with what kinds of tasks, explicit and "hidden," the group seemed to be concerned. We wanted a method which would enable us to deal in the same terms with interchanges between two in-

⁶These statements hold best when inquiry is thought of as a conscious process; for then change would certainly be accompanied by learning and adaptation. The statements are more tentative in cases where there seems to be change but little or no learning. One group, for example, developed a culture in which there was considerable freedom to "fight" but no freedom to "work"—the amount of conscious inquiry was practically zero. The group fought for 15 meetings, and apparently never resolved any of its problems. At the same time, however, there were changes in the way it fought and in its perception that it was fighting, e.g., the unspontaneous planning and dogged engagement in "social" activities whose purpose seemed to be denial of their hostilities. A precise statement would probably be that the amount of *adaptive* change is related to the amount of inquiry but that nonadaptive changes can occur without inquiry.

dividuals, interaction within a group over the period of a phase or meeting, and the whole life of the group.

The second task was to devise some way to relate individual personality to group operation. We wanted a scheme that would enable us to predict what role individuals would play, and what effects different combinations of personalities (composition) would have on the operation of the group.

The third task was to devise some way of identifying subgroups within the total group structure. We wanted to know how far group operation can be understood as the interaction between subgroups and what part subgroups play in determining the nature of group operation on the one hand and of individual participation on the other.

The fourth task was to demonstrate relationships between group operation and "productivity" or results. We were interested in such matters as who learns the most from training groups in human relations, and what sorts of groups reach the "best" solutions to problems.

The sequential method for analysis of group operation. The starting point was the observation that groups pass through different phases. This one can feel in his viscera. At one time the group is tense, confused, easily frustrated; at another time the group is happy, relaxed, creative. These are differences in moods, and they represent different emotion-alized states of being. It was clear, then, that we needed data on the emotional or affective aspects of behaviors. In addition, however, these phases differed, it appeared, in the kind of work that was going on. At some times, it seemed that every man was intent on his own inquiry; at other times there appeared to be a genuinely cooperative effort, in which a whole structure of ideas and conclusions was being erected in an orderly way. Thus it also seemed clear that we must get data on the kind of work or thinking that was going on.

In order to reproduce the meetings we wanted also to record all individual contributions in sequence and along a time axis. And, of course, we wanted a sound record to fill in the content of ideas that were expressed.

For a variety of reasons which will be discussed later, we wanted to get our data on emotion and work by direct observation—meaning interaction of trained observers with the live situation—rather than from the sound record.

About this time, Bion's articles, to which I have already referred, began to appear, and it seemed to us that he was putting into words the basic concepts we needed. Our job was to see if we could give these concepts operational definitions so that they could be used for categories of observation, and a great deal of effort went into this. Bion's basic ideas were that the mode of operation of a group changes from time to time,

i.e., it passes through distinguishable phases, and that each phase represents a particular combination of basic tendencies to "work" and toward "emotionality." Work is sophisticated reality seeking, using conscious problem-solving methods, and the ability to work is learned. "Emotionality" is "primitive" unlearned direct reaction, and responds to unconscious needs of the group to maintain itself in the face of stress from within or from without.

Bion [5] suggested three basic categories of emotionality: fight-flight, dependency, and pairing; these he saw as "basic assumptions" dominating periods of interaction. We decided to use these categories for specific individual behaviors as well. We separated fight and flight into two categories because they are distinguishable individual behaviors. Fight is any expression of aggression toward the problem, the group, an outside agency, the leader, the self, or anything else. Flight is any behavior of "running away" from stress by such means as joking, breaking up the meeting, daydreaming, incoherent rambling discussion, "academic" presentation, etc. Dependency is shown in any behavior which seeks aid from outside the person: from the leader, the minutes of the last meeting, traditions, experts, and so on. Usually, such seeking of aid is accompanied by expressions of weakness and inadequacy: "the job is too big," "we don't have the resources," etc. Pairing behavior includes intimate remarks made privately to another individual, "reaching out" to others with expression of warmth, approval, or agreement, or even, by extension, a warm statement to the group.

We divided Bion's concept of work into four distinguishable kinds: individual work, in which the person is intent on his own private interest and concerns; group "housekeeping," in which the group is making routine decisions about what topic to discuss, how to appoint a chairman, how long to meet, etc.; task- or goal-directed work, in which the group has a clear purpose and is trying to collect information, make suggestions, select a course of action, evaluate the suggestions, and so forth; and finally, "integrative" work, which pulls the whole enterprise together. In integrative work we see the effort to relate what the group is doing to the "kind" of group it is and wants to be; the statements are thoughtful and insightful interpretations which tend to give the group its "bearings." (Looking back to preceding discussion under Common elements we may point out that the first and second kinds of work refer to the internal system, the third kind to the external system, and the fourth kind to the relationships between internal and external systems.)

Thus we have four categories of emotionality and four of work. After considerable practice, we finally defined the categories by trying to characterize the intentions and the behaviors typical of each category. Then we added a good many illustrations of the kinds of behaviors we

meant. When observers were trained to use the categories, it was found that each tends to have a consistent bias—one sees more fight, another more dependency, and so on. Since there is no “right” amount of fight or dependency, the bias simply meant that we should keep the same observers with the same group during all its meetings. We finally decided to have two observers rate the behaviors independently, and then, later on, compare notes and argue out their disagreements, using the sound record to help them recall the situation.

During a meeting, each observer records the code number of the participant, the kind of work contribution (one of four categories), and the kind of emotionality (none, or one of four categories). He also jots down a few words so that the participant’s comment can be found on the sound record. And every minute he draws a line so that the interaction is chopped up into equal-sized periods.

The data are then graphed. After much experimentation, we finally decided to show four broken lines, with a point on each line at the end of each minute. One line shows how many speakers contributed during each minute. This we called rate of interaction. Another line showed “how much” emotionality was expressed each minute—or, more accurately, how many times the observers detected one of the kinds of emotionality during each minute. We decided, quite arbitrarily, to distinguish between “big” and “little” emotionality, and to weight these 2 and 1 respectively. Big emotionality is a direct expression; e.g., “Beat it!” “Little” emotionality is a muted expression which the speaker might even deny, e.g. “I wonder if Joe wouldn’t feel happier somewhere else.” The amount of emotionality each minute was the sum of the weighted ratings made by the observer each minute. Not all statements were perceived as having “emotionality.” The categories of emotionality were entered in the margin of the graph; only the “total amount” was plotted.

The third line represented work during each minute. The four kinds of work were weighted with numbers from one to four. This again was an arbitrary decision and it reflects the feeling that these four kinds of work represent a continuum, from “individually oriented” to “integrative.” The continuum is one of “maturity” or “group cohesiveness.” During the period of 10 meetings, for example, we find a lower average work level during the first meetings than during the last; and the concepts of work were first suggested as “developmental levels.”

The fourth line is put in for easy interpretation of the work line. This line represents level two work (group “housekeeping”) as a kind of mundane standard. It is obtained by multiplying the number of participants each minute by two. The categories of work are also entered in the margin, and, in addition, the code numbers of the participants.

Having figured out a way to plot the data, we became fascinated with our graphs, and we tried to see if we could detect different phases just by inspection of the graphs. We found that to some extent we could. Thus a period of 10 minutes might be found in which the lines were all close together, and another in which they were spread apart. The closely set lines would indicate rather equal rates of emotionality, work, and interaction per minute, i.e., individually oriented work which was highly charged emotionally. If all the lines were close to the bottom, it would suggest tension and inhibition; if they were all fairly high up it would mean excitement. When the lines were wide apart, it would tend to mean a high work orientation accompanied by low emotionality; but if the emotionality line was lying on the "floor," the high work might be pretty academic and uninvolved.

At any rate, we found the graphs, with their rapid overview of the meeting, quite useful in taking the next step, the observers' effort to interpret what was going on during each minute. These interpretations were, in effect, the specific hypotheses which later on would have to be checked from other data such as the sound record, questionnaires to the participants, or interviews.

We still needed to find some way to divide the interaction sequence into phases. If these phases existed and if, as Bion suggested, they represented different states of being or operation on different "basic assumptions," then it was essential that we be able to identify them so they could be compared among themselves. A very elaborate analytical job was done on one series of 10 meetings of one group. We considered the minute-by-minute interpretations, the appearance of the graphs, the interviews with the leader and his assistant after each meeting, our own reactions as observers—and we divided the sequence over the 10 meetings into 120 periods or phases. The whole process was repeated a year later, when memories were colder, and the differences between the two "unitizings" were thrashed out into one "best" picture. In the meanwhile, Ben-Zeev [2], a member of the team, worked out an extremely clever scheme for unitizing the meetings simply from the code numbers of the participants. He started from the assumption that a somewhat different set of people would contribute during each phase, and he set out to tabulate the participants and identify the points at which the pattern noticeably changed. The procedure, which is too complicated to permit summarizing here, is completely described in [54]. The unitizing by Ben-Zeev's method was compared with the highly complex unitizing obtained by analysis, and 74 per cent of his units corresponded exactly with those obtained by analysis. Allowing a leeway of two minutes at the beginning and end of each unit, the correspondence climbed to 95 per cent.

Now that we had a systematic and objective procedure for unitizing

the sequences of interaction, it was possible to take the next step, plotting the units on an emotion-work field. For each unit, we calculated the average work and emotionality per statement and then located it on a graph of emotion vs. work. Instead of entering the unit as a point, we entered it as a rectangle whose center represented the averages; the length was made proportional to the number of minutes duration of the unit, and the breadth was made proportional to the average number of participants per minute. Thus the area of the rectangle indicated the total number of contributions during the period. Each rectangle was numbered in the time sequence, and each was connected by a straight line to the preceding and subsequent units. Thus we were able to see how the group shifted in its work and emotionality orientations during an entire 2-hr meeting; we could form some notion of the relative stability of each unit, locate transitional units, and note which subgroups of participants tended to operate in different parts of the work-emotionality field.

Thus ended the first task: the development of a procedure by which we could systematically move from individual contributions, to phases, to meetings—viewing all with the same set of concepts and noting how one led to another.

The measurement of valence and individual-group relationships. The notion that different phases of group operation would have different participants suggests that the phases originate in some kind of common tendency shared within the “combination” of participants. During a “fight” period, for example, we might find five people most actively carrying the ball; during a “dependency” period, four others might be the active people. One might reasonably think of the five participants in fight as “fighters”—in the particular situation of the group at that time.

With his psychoanalytic concern over group formation and maintenance through object-tic identification, Bion [8] put the matter a little more complexly, suggesting that certain individuals tend to “combine” with each other to maintain each particular mode or pattern of modes. This tendency he called their “valency.” Thus in a particular group with the other particular members, a person might have a high valency for fight; i.e., in some way he would “combine” with certain others in the group to shift the group into operation on the assumption that their purpose at that point was to fight.

The concept of “combination” seemed to us rather difficult to penetrate, and we decided to begin by simply trying to work out a procedure for estimating tendencies toward fight, pairing, dependency, flight, and work in the group. These tendencies we thought of as an oversimplified kind of “valency,” and we referred to them as the “group-relevant aspects of personality.”

These tendencies are what each individual contributes to the dynamics of the group, and as a result of a good deal of experimentation we developed a test for assessing them. The test presents the subject with incomplete sentences each of which he is to complete as rapidly as possible so that he cannot actually think about what he is writing: we want him to follow his impulses. The most useful sentence stubs were found to portray some situation commonly experienced in the group. "When George attacked the group, Bob _____." "When the leader offered to help him, Pete _____." "Since Jack liked some members more than others, he _____." "When Jerry was joking, the group _____." "When Sam said 'Let's get to the problem,' I _____." The stubs also incorporated the various emotionalities and work. Thus the examples given are keyed respectively to fight, dependency, pairing, flight, and work. The various items were chosen to sample each of these modalities in a range of instances.

The test is scored "quantitatively." In addition, it is capable of considerable "qualitative" penetration. To compare compositions of two different groups, the quantitative scores are sufficient; but for prediction of the behavior of one individual, the protocol must be studied at some "depth."

The most obvious first score is simply a count of the number of each kind of item the person completes with the same modality it suggests; e.g., how many of the "fight" items are accepted? Of the six fight items a person may accept all; of the dependency items, he may accept three and not accept the other three, and so on. By "nonaccept" we mean that the person completes the sentence with a different modality than that given in the stimulus. "When George attacked the group, Bob attacked George" is a clear acceptance of the fight stimulus. "When George attacked the group, Bob fell silent" suggests psychological withdrawal, or flight. A second score, therefore, is the count of the number of times the person introduces each of the modalities into the sentences.

Now just as we found the distinction between "little" and "big" expressions of emotionality useful in the group, so we found a similar distinction useful with this test. We often have the feeling that the "little" expressions on the test are probably unconscious, and might even be denied. Thus some fighting directed against the leader seems to be tinged with dependency—the person is fighting the leader as a way to hide from himself his own feeling of dependency; but he would probably refer to himself as an "independent" sort of person and would resent the interpretation just given. At any rate, such overtones could be detected frequently, and we counted them, too.

In addition, it is interesting to note the form of the response. We noted (and counted) responses in terms of action, feeling, and ideation,

as illustrated respectively in the following completions of the item "Since Jack liked some members more than others, he _____": "invited them to have a drink with him," "felt supported," and "wondered whether it was all right."

In the qualitative analysis, we can go beyond the scores to make shrewd guesses about the conditions under which a person will have each of these impulses, the extent of his ambivalence toward or acceptance of the impulse, the way he is likely to express it, and with luck, something about his mechanism of impulse control.

As a result of our investigations with this instrument we have noted that even our simplified concept of valency contained concepts of three distinguishable tendencies: (a) tendency to express the modality in the group (e.g., a person with a "fight" tendency actually fights in the group), (b) tendency to become actively participant when the modality is established in the group (e.g., the group is in a "fight" phase and the person participates, not to express fight himself, but to flee, pair, or actively "respond" in some other way), (c) tendency to become disturbed or anxious, whether participant or not, when the modality is established in the group (e.g., the group is in a "fight" phase and the person becomes immobilized and anxious; in this case he may respond with several modalities or he may be nonparticipant).

The sentence completion protocols, inspected by a trained nervous system, can lead to quite accurate predictions about how most people will participate in groups. Further refinements of the test are in the direction of "building in" more situational dimensions so as to increase the load of interpretation possible from "quantitative" analysis.

The identification of functional subgroups. We found that the hidden problems of group process—and these are the ones most related to emotional phenomena—could be viewed as representing a struggle to establish the work-emotionality assumptions on which the group would operate. But there were two problems that we felt needed investigation. As diagnosticians, we needed to conceptualize the struggle, not only in terms of emotional tendencies in group culture, but also in terms of much more precisely defined purposes. One wants to know not only that there is a struggle between the fighters and nonfighters but also what the struggle is about; e.g., those who want a "strong leader" vs. those who would like to be leaders themselves, know that they cannot, and therefore compromise by trying to prevent anyone else from becoming "leader." Such concepts of purpose would enable us to bridge between individual emotional tendencies on the one hand and the content of discussion on the other.

The second problem was the unsolved part of Bion's notion of valency, namely the idea that persons "combine" to maintain the balance

of work and emotionality characteristic of each phase. What is the basis of this combination? Although it might be ties to the leader, this would not account for differences in participation patterns. The notion that the participant subgroups represent sociometric cliques was found to be true under certain conditions, e.g., when the basic assumption is pairing, and untrue for others, e.g., when the basic assumption is fight. The notion that there is identification with some "central person" [37] or "spokesman" [6, 9] seemed reasonable, but we found the concept difficult to use in any confident or systematic way.

We decided to investigate the basis of formation of the shifting subgroups of participants during the various phases. The first problem was to find some way to represent individual purpose at a level less "deep" and nonspecific than emotional tendencies but more "deep" and fundamental than consistencies in overt behavior. In terms of "levels" or "depths" we needed something analogous to the TAT whose usual interpretation lies between the deeper Rorschach on the one hand and what the man says on the other.

For this purpose we adopted the self-perceptual *Q* sort [42]. The individual is handed a pack of cards each of which contains one possible self-perception; e.g., "I tend to dominate when the group gets confused." Each item was keyed to the more fundamental modalities, so that ultimately the data could be related to the deeper need system. The individual sorts the pack into a specified number of piles, with a specified number of cards in each pile. Thus he is forced to distribute the items within a normal distribution curve, which makes the statistics easier. At one end of the distribution are the cards "most characteristic" of himself; at the other end, the "least characteristic" items—as judged by himself. Through factor analysis it is possible to find subgroups of people whose distributions show the same common factors, which in this case are themes or purposes. Through further work, involving analysis of the contribution of each item to each factor, it is possible to describe with some accuracy what the common factors characteristic of each "subgroup" are. With this information, it is then possible to view the events in the group as the acting out of inter-subgroup dynamics. Conceptually, not statistically, this is an immense simplification as compared to trying to account for all of the interpersonal dynamics; and we believe we are on the track of some highly practical notions. The data obtained through this method of approach make possible a close *rapprochement* with the concepts of group and subgroup culture as understood by the sociologist, and may thus further more explicit use of sociological concepts in the system of thought.

Demonstration of relations to "productivity." We, too, are interested in the problem of whether certain kinds of group operation and indi-

vidual valency patterns lead to more learning, better problem solving, and the production of canned peas at a lower cost. Efforts to answer such questions would invest the whole structure of ideas with social value. Since evaluative assumptions frequently are smuggled into the study of groups, we considered it effort well spent to study whether, and to what extent, evidence actually supported these implied value judgments.

The groups we have studied were "human relations training groups." They were selected for study because the normal course of their operation provided considerable material about feelings, anxieties, ideas of emotional blocks, and other data often hard to secure. The productivity of such groups lies in changes within individuals, however; and these changes in attitude, self-perception, and world view are harder to measure than, say, educational achievement.

Despite these difficulties, however, there have been sporadic forays into the realm of the practical. The nature and efficiency of decision making was compared for two groups, one of "flight" people, the other of "work-pairing" people (the latter group was "superior"). The work-emotionality cultures of 50 groups were compared and related to the quality of their problem solving. (The fight-work group with "integrative control systems" were "superior"). Two training groups were composed alike but "pairing" tendencies were left out of one (it had a most difficult time). Individual valency patterns were also related to many fragments of evidence about "trainability." Positive indicators were acceptance of tendencies toward fight and pairing and existence of undenied emotional conflict; negative indicators were tendencies toward dependency and flight and immobilization.

As far as they go, these studies are instructive. They are valid in the sense that predicted differences in outcomes were found. And they confirm our evaluative reactions. But a great many more, developed within a systematic program of inquiry and demonstration, are needed to establish the social utility of the system.

From private to public domains: "Blind analysis" and theory construction. As indicated earlier, these studies have been used to refine and tighten the system of propositions underlying the research. In the course of the studies many specific hypotheses have been generated and tested, but so far this experience has been fed back into the propositions rather than built into a system of theory as usually represented.

This practice is related to the problem of moving from private to public worlds. It is not enough to make and confirm specific predictions (hypotheses). One must also show how these specific hypotheses are derived from general principles, and this he must do in such fashion that other competent people can do the same. Here lies the major scientific problem of clinical psychology, a problem which is ours as well.

How, for instance, does one publicly demonstrate his movement from Rorschach data to predictions of behavior?

There are two approaches to this kind of problem. One may note the principles he seems to be using, record them, and then try to organize the most frequently used principles into a coherent body of main and subpoints, e.g., general hypotheses and corollaries.

Or one may use a second approach, one we have explored far enough to consider it worth reporting. This approach assumes that the key to coherence lies in the relationships within the phenomena and that conceptual relationships should reflect these coherences. For the criterion of social utility (frequency of use) implied in the first approach, we substitute the criterion of conceptual necessity. We suggest doing this through "blind analysis." The proposed operations are as follows:

First draw the graph of a meeting, as explained earlier, (see *The sequential method for analysis . . .*). Then analyze the "group-relevant personality tendencies" of each member, using the sentence completion test administered individually and privately; and also identify the subgroups and their purposes (see *The identification of functional subgroups*). Then hand these sets of data, *not* including the sound record or any other record of "content," to another researcher with instructions to reconstruct the dialogue of the meeting. After he has done this, his reconstruction can be compared with the actual dialogue, and checked, contribution by contribution, with the record. The instructions to the researchers at all four steps are to "think out loud," preferably in front of a recording machine. The thinking they must do to accomplish the job should then be analyzed and organized to produce the body of "theory."

The logic of the approach might be put in this form: if our propositions which gave birth to the research methods are correct and if our methods truly reflect these propositions, we may assume that all the necessary data are on hand. It follows then that thinking is the task still to do; the body of propositions and concepts (with their large surplus meanings) gives the general orientation, sequence, and conceptual tools for the thought required. The product of analyzing the thought process should be the desired theoretical structure. To meet the argument that this is too long a chain of thought between firm data, further experiments can test particular, crucial principles in much more limited settings.

We have not carried this proposal through in all its particulars. We have, however, very successfully reconstructed, as a "blind" operation, the dialogue of several short sequences of meetings. We used the graphs of the meetings but substituted for personality data from the tests, knowledge of the participants gathered from our own observation of them in meetings. We have been able to expose our thought processes in putting

the two kinds of information together. In other words, the "principles" to be organized in theory are close at hand. This preliminary check persuades us that we have properly identified and understood the required data—and certainly increases our confidence in the usefulness of our propositions.

The systematic independent, intervening, and dependent variables. Inquiry into human behaving proceeds in several stages. First one observes behavior, these observations are his data. Then one looks for patterns, consistencies, or themes within the behavior, e.g., directions of growth, disequilibria, projected goals, etc.; these ideas of coherent organization of behavior are the independent variables, and they summarize tendencies toward change. Then one reflects on these tendencies, tries to "explain" them and indicate what sorts of things will happen next. The concepts used for reflection and explanation are intervening variables; the various sorts of things which might happen next are dependent variables. The researcher selects certain aspects of the possible next events that interest him and tries to define what he would actually observe if his guesses were right. And this prediction now is of the actual data he expects to collect.

All of this is guided by previous thinking, in which the researcher has asserted probable relationships between the independent and dependent variables. Such hypotheses are general. From these he could also state very specific hypotheses, that certain behaviors will be followed by other equally specific behaviors, but such hypotheses are not useful unless the particular behaviors selected are clearly symptomatic of organismic purposes; that is, unless such behaviors are meaningful.

In our work, then, we can say two things about our variables:

1. The independent variables are *tendencies* and therefore imply further behaviors. The intervening variables are constructions of organismic *purpose*, usually in terms of "as if." The dependent variables are *classes of behavior* that will follow as the organism moves on to achieve its purposes.

2. The "organism" may be either an individual or a group, in the sense indicated under Common elements.

Listing the three kinds of variables presents difficulties, in my opinion, because the classification independent-intervening-dependent arises from the course of inquiry rather than from the nature of the system. Certainly the variables called "dependent" in one investigation might be "independent" in another. As explanatory constructs, the intervening variables, however, could never be anything else. Thus one can start with test scores on the sentence completion test described earlier and predict behavior in the group, but one could start from behavior in the group and predict test scores. This sort of reversibility would hold in any investiga-

tion that tries to predict behavior in one situation from behavior in another situation, and this is what our researches typically boil down to. We may limit an investigation to a part of this process, as when we try, from specific behaviors, to infer the structure of the group; yet sooner or later somebody must ask the question: if my picture of the structure is correct, then what should I now observe? So that we end with behavior, if only to validate our intermediate larger conceptions.

With this orientation, then, I may analyze the variables in the four actual research designs described on the preceding pages. We may then examine in more detail the nature of some of the variables that are crucial in the development of the system.

The independent variables. The table shows four independent variables. Two of them are applied to the "live" observed group, and two to questionnaire or test results, obtained from each individual. One of the variables from each situation has to do with tendencies toward the various modalities of emotion and work. These two variables are alike in concept, as will be seen from the description in the preceding section. The variable "explicit purposes" is a low-level generalization from the content of discussion. It comprehends the explicit ideas expressed by members as to what the group is doing or trying to do. The fourth variable is the set of relationships, obtained by intercorrelation, among the Q -sort distributions of the members of the group. It implies a whole series of specific overlappings (to various degrees) of the perceptual fields or "life spaces" of the members of the group.

The intervening variables. These are more interesting because they represent key theoretical ideas. They do two things: they collate the items of the independent variables into wholes or patterns; and they enable us to shift from one situation to another. That is, they move from individual properties to group properties, or from individual behavior in a private situation to individual behavior in a group situation.

Thus "basic assumptions during unit" is a diagnostic generalization about the e-w pattern of the group culture—the assumptions on which the group seems to be operating; but it is obtained from generalizations of the minute-by-minute relationships between collected individual expressions of work and emotionality. When these basic assumptions, which imply unconscious "purposes" of the group (mainly in the realm of self-maintenance) are put together with generalizations about the explicitly stated purposes of the group, one is enabled to make a fair diagnosis of problems the group is working on, and the conflicts and strains within the group as a whole. These "intervening variables" then, stand for the application of a good many principles from individual and group psychology; the diagnostic mind is not a machine for automatically processing the input of independent variables. For rigorous system-

TABLE 1. INDEPENDENT, INTERVENING, AND DEPENDENT VARIABLES IN FOUR INVESTIGATIONS

Investigation	Independent		Intervening	Dependent	
	Data	Construct	Construct	Construct	Data
Sequential analysis	Observer's ratings, e. and w.*	Shifting relationships between e. and w. trends in unit	Basic assumptions during unit	Predicted e. and w. trends Predicted group purposes	E. and w. ratings and activities
	Verbal behavior	Explicit purposes	Group needs (diagnosis)		
Valence	Responses on s. c. test†	Trends toward e. and w. modes	Valence	e-w role in group	Ratings
Functional subgroups	Responses on s. c. test	Trends toward e. and w. modes	Valence	Sociometric structure	Choices
	Q sort of self-perceptual items	Intercorrelation relationships	Factors of commonality	Perceptions of meeting, others, etc.	Questionnaire
Predicted "productivity"	Responses on s. c. test (20 people)	Trends toward e. and w. modes	Valences	Identified subgroups: members "themes"	Coparticipation
	Responses on s. c. test (members of 50 groups)	Trends toward e. and w. modes	"Averaged" e-w trends = group "basic assumptions" (by group)	Relative "trainability"	Interaction content
	Responses on s. c. test (100 people)	Trends toward e. and w. modes	Similar e-w trends = group "basic assumptions" (selected groups)	Rank order on 21 problem-solving criteria (12 selected groups)	Perceptions of members and associates
				General differences in problem solving	Problem solutions dictated by group
					Observed and rated behaviors

* e. and w.: emotionality and work.

† s. c.: sentence completion.

atization a great deal of breakdown of ideas is required, as suggested in design 5. And tearing ideas out of their gestalts does not impress me as easy; it may even be perilous.

The intervening variable of "valence," which has been discussed before, moves from private behavior to public behavior expected in the group. Our concept of "valence" tends to operate with two degrees of complexity: through rather low-level interpretation, one can tell from the "quantitative" analysis a good deal about the circumstances of probable participation as well as the e-w nature of the participation—for most people tested. When one adds the "qualitative" analysis, the aim is to penetrate the threat-defense system of the individual and to make richer inferences about his relationships or identifications with others in the group. It was more in this latter sense that Bion suggested the term, but the former, simpler sense has been adequate for some purposes.

To my knowledge an outstanding property of valence is not shared with any other variable thus far suggested. Valence seems to be the one variable whose *individual* measures can be averaged arithmetically to get "basic assumptions" of the group—and this is a *group* tendency. This finding is extremely useful because it enables us to compose groups for various purposes almost at will, an important practical achievement. This property derives from the fact that individual emotional tendencies directly influence—in fact, produce—group emotional tendencies. Nevertheless a more adequate explanation must wait until we know more about the way affect is "shared" and about the nature of intermember identification.

The factors of "commonality" refer to the third design described earlier. They enable us to move from consideration of the individual as a person to consideration of the individual as a member of a subgroup.

Behind these intervening variables are basic propositions about stress and tension in human behavior. As referred to members, the intervening variables describe *tension* within the member; as referred to the group, they describe *stress* within the group. For the latter concept Bion uses the term *culture* (basic assumptions) which describes the psychological purpose (whether conscious or unconscious, accepted or denied) of the group as a whole.

Following Bion, stress arises from capacity for two basically different types of behavior: emotionality and work. In each situation, the group is confronted with the problem of working out a relationship between these two aspects of its functioning. On the one hand, group members have the need to listen to each other, to bring previous experience to bear on their problems, to think critically, to diagnose problems and deal with them explicitly; this is work. On the other hand, the group has the need to maintain itself as a group: to flee from emotion it can not

handle, to develop interpersonal relationships so that individuals may deal with their own private anxieties, to find leadership on which it can depend for maintaining social order, to express its aggressions and resistances in situations with which it cannot cope. Different individuals have different valencies for these kinds of behavior, and somehow a balance must be struck among them. This balance, macroscopically described as the pattern of "basic assumption" on which the group is operating, is its "culture" during each period of operation.

Working out the relationships between emotionality and work means dealing with ambivalence, conflict, and anxieties. In other words, it is accompanied by *stress*. Each individual reacts to this stress in his own way. Thus we may notice that during a certain period, Mary is expressing dependency, Joe fight, and Harry is pairing with Tom. These are individual modes of reaction. We inquire, under what conditions will each of these individuals behave in this way? We can list (from the sentence completion test) a number of hypotheses about different conditions which would be likely to evoke these responses. The one condition common to Mary's dependency, Joe's fight, and Tom's pairing would seem to be the group's culture. This, in fact, is the safest way to diagnose the basic assumptions operative in the group. Thus, this concept of basic assumption is also a concept of a stress or pressure on all the members of the group, or, if you prefer, a group need to which all the members react. Yet this concept of group need or stress may not be within the perceptual field of a member; we note that some members are usually quite unaware of this sort of influence even though they appear to react to it.

If we extend the notion of group need to the idea that the need may be within an internalized group rather than only in a face-to-face actual group, then we have described something very similar to Lewin's "alien factors" which are outside the perceptual field of the individual even though they influence his behavior. The notion of a basic assumption within an internalized group might also be a "press" in Murray's sense.

The individual, with his own predispositions, finds that the stresses in the group act as triggers or cues for selection of the particular predispositions that will be translated into behavior. This need for selection among his various capabilities and desires puts the group member under *tension*. The tension may represent ambivalence, anxiety, fear, anticipation of reward, and so on. The individual may step in boldly to establish whatever *modus vivendi* is most effective for reducing his tension. He may withdraw from the group or he may adopt some middle ground. Whatever he does, there will be other individuals in the group who in whole or in part "want" to adopt the same mode. Subgroups thus may emerge, one seeming to advocate discharge of tension through fight,

others through work, flight, or some particular blend of these. The dominant subgroup is the one most participant at that time; its preferred mode becomes, in effect, the basic assumption ruling the group's culture.

The matter is further complicated by mechanisms of denial and other secondary dynamics that are themselves productive of further tension for individuals and of further stress in the group. Thus when fight, for example, becomes established as the basic assumption, there are those who at some level would like to fight but who repress this desire—often with the expenditure of much psychic energy. In general, the person or subgroup seen as a “problem” by other members is one who expresses impulses that others are trying to repress.

Dependent variables. These are behaviors elicited during or after the meeting. During the meeting they are cognitive and affective contributions in the various categories of work and emotionality. After the meeting they are postmeeting perceptions of self, others, leader, task, group, critical incidents, periods of success and failure, and so on. These may also be elicited in special test situations for the group, such as the picture projection test designed by Horwitz [22].

The dependent variables shown in the analysis of experimental designs are, as previously noted, classes of behavior in a new situation or under the new circumstances to which we are trying to predict.

Mode of definition of representative variables of each category.

1. *Independent.* The independent variables represent general tendencies for emotionality and work responses to stress situations. They have been arrived at both empirically and by interpretation of widely varied knowledge. Fight and flight, for example, are primitive states of mobilization of the nervous system in stress situations. Dependency probably became internalized with the development of the family. Pairing can be seen both as a sexual and a societal manifestation in the development of new families and in the establishment of the sort of societal interdependence increasingly required for survival of the species. These basic modes, then, are defined at a high level of abstraction. Operationally they are defined as the “purpose” or “intention” motivating a wide variety of behaviors.

The definition of the independent variables is based upon one of two assumptions: (a) behavior is purposive and at a high level of abstraction there are a limited number of basic purposes of human behavior; (b) we cannot understand behavior (because of our own human constitution) until we have named its intent, and the intents we note are those we are capable of noting because we are concerned with them in ourselves. In either case, the result is the same; the “definition” is somewhere internalized within the researcher, and through his research experience, he gradually tries to make it explicit in language. The prob-

lems of definition in our work are essentially similar to those in clinical personality study.

Any practical means for controlling the valencies in the group uses empirical independent variables. Thus the "composition" of the group can be controlled by deliberately selecting people whose individual valency patterns are alike or different in specified respects. Or, the composition may be built up out of people who will be "effective" together on a task or in evading a task; here one begins with some one person's valency pattern, and then considers the sorts of additional patterns that would be supportive or threatening. Even without specific knowledge of individual valency patterns, a great deal can be done roughly with class and ethnic background factors. Over time, we hope through cross-cultural and class studies to spell out some cultural characteristics in these terms.

Besides controlling the opportunity of members to "combine" with others (through choice of those others), further control can be exerted by setting up institutional expectancies for the group and by defining its task. We have, for example, worked with "human relations training" groups because these operate in a climate of the broadest range of expectations. In a way, anything goes; hence we can get fairly intense expressions of many tendencies that might otherwise remain suppressed. Comparisons can be made of a group with wide limits to expression as against one in which all autistic behaviors are "out of line." This sort of comparison involves not the selection of the initial tendencies but rather the control of which among them will be allowed expression and thus contribute data.

2. *Intervening.* The intervening variables are concepts of arrangement of tendencies. In a final development, systematization of these variables might well imply "force field" analysis of the sort Lewin was driving toward. Currently, the possible interactions between two tendencies are considered to be reinforcement, conflict, or repression; in addition there are reactions to these possibilities: action, anxiety, ambivalence, immobilization, and the like. The intervening variables become the key concepts in the theoretical reconstruction of events in the group. This reconstruction is complete when it explains why each participant in the situation interprets it the way he does.

With intervening even more than with independent variables, the researcher's sensitivity and training provide definition. The constructs of "valence," with its innumerable possible ramifications through "qualitative" analysis, and of group basic assumptions, with their implications of underlying psychological dynamics, mean more to some researchers than to others. In other words the constructs have "surplus meanings." A theory, with carefully defined intervening variable constructs may

in the hands of one person "mean" little; in the hands of another, it will explain much. For a theory is not a written document, it is an internalized set of tools for dealing with the practical business of meeting certain needs. This is particularly true in the study of human behavior, which we must always to some extent see from the "inside" and be personally involved with.

3. *Dependent.* The dependent variables lead into predicted specific behaviors and are measured by counting frequencies in broadly defined categories. They are defined usually by setting up continua of behaviors, illustrating at one end an extreme manifestation, at the middle an indifference, and at the other end extreme denial or rejection. Such continua can be set up for aspects of behavior relevant to whatever group or individual product is being used to supply the performance criterion. In so far as possible, we select dependent variables which are relevant to the purposes of the group, for such variables produce many more behaviors which can be recorded as data. Purposes may include change of individual skills, acquisition of knowledge, and change of value-attitudes of individuals, development of group leadership, group culture, or group efficiency of operation, and finally, the production of minutes, decisions, project-activities (such as role-playing), and so on. In all instances the data are behaviors, but the process of interpretation is guided by the methods of abstracting and collating described in the monograph mentioned earlier [54].

Major interrelations among constructs. At this point, we may attempt a more concise statement of the interrelationships among the constructs. From data, through independent variables, to intervening variables, we are constructing part-whole relationships. From intervening variables to dependent variables to predicted outcomes, we are constructing whole-part relationships. In this fashion, we move up and then down the "ladder of abstraction."

Specific behaviors elicited in response to a large number of specific situations (as in the sentence completion test) can be viewed as parts of larger tendencies to deal with stress through emotion-work modalities. These tendencies, as generalizations, collect specifics into a more abstract whole, a pattern of probabilities; these are represented as "independent variables." Both the specific behaviors and the tendencies refer to the same thing: a particular individual acting alone.

As we move to the intervening variable of "valence," we move to another domain, the group, rather than the individual. The independent variables, referring to each individual, become parts in a larger, more abstract whole, the group. The concept of valence includes (nonexplicitly as yet) the concept of identification through common object ties—which

in this case serves the purpose of maintaining the group through establishing a particular combination of "basic assumptions" of group purpose. From the larger whole, the organizing principles of the group culture, the chain of deduction begins. The dependent variables identify categories of behaviors that will, presumably, be found from the inferred pattern of basic assumptions. The data at this end of the bridge are once again specific behaviors, but they are behaviors to be observed in a particular group composed of given individuals. Thus the over-all movement has been from specific behavior of individuals in private situations to specific behaviors of individuals in the group, or, from individual tendencies in "personality" to tendencies of members as part of an interdependent system.

INITIAL EVIDENTIAL GROUNDS FOR ASSUMPTIONS OF SYSTEM

It is clear by now that we tend to consider research as merely one way for a researcher to meet real and important needs of his own. The development of a theory may, therefore, reflect quite basic factors within the researcher's nervous system. In any case, preliminary work and practice as "trainer" in many groups led to certain "convictions" which had the force almost of axioms. These notions led to the decision to see what could be done in experimental situations with Bion's concepts. The "convictions" were

1. That groups do have periods in which they are dominated by different moods.
2. That the concept of group qua group rather than as group qua collection, although a theoretical construct, is essential to our thinking and reacting as social beings.
3. That the "laws" governing group life will be "laws" of change: that is, they will (when developed) be concerned with the continually shifting balance of forces in the group, and with the continually shifting "culture" (in the sense of unconscious values and purposes) of the group.
4. That affective behaviors communicate directly and nonverbally and are sensed directly, i.e., that "emotion" should be recorded as primary data rather than used to "explain" so-called objective behavior.

These convictions seemed to summarize our experience with groups. They served as specifications which the research would have to meet: (a) for sequential analysis, or analysis of "flow" of experience through time, (b) for finding concepts that would fit the group as a whole—and by this I do *not* mean simply analogies to individual personality, (c) for seeing in the group a dynamic interplay between conflicting tenden-

cies, i.e., for looking for some dynamic, dramatic theme in group life, and (*d*) for categorizing emotion directly through paying attention to our own visceral responses to goings on in the group.

All four of these convictions pointed toward a "psychiatric" approach to group dynamics as distinguished from sociological, perceptual, sociometric, and other approaches. In Bion's stimulating articles, we found many of the concepts we needed. Bion's terms fight-flight, pairing, dependency, and work were put forth to describe the moods that groups sustain at different periods. The notion of applying these terms to characterize specific individual *behaviors* was not suggested by Bion; he was primarily concerned with group modalities and individual *tendencies*.

To us, the concept of the group as a group seemed also to be more convincingly suggested by Bion than by any other theorist we knew. Especially useful were Bion's ideas that the individual in some sense is always reflecting needs of the group, at least during some periods, it is as if the group were speaking through many voices and the particular individual whose vocal cords are thus utilized is relatively unimportant as an individual. Then, too, the notion that at times different people are "spokesmen" for the group, although frequently discussed by others in connection with "what is leadership," seemed in Bion's thinking to be extended and generalized to cover a great many other possible roles as well. It also appeared that Bion's concept of unconscious identification with subgroups attempting to maintain or promote particular basic assumptions (of emotion-work) made room for a subgroup structure which fitted the facts of group life more adequately than do more sociological or sociometric concepts (although these clearly are useful at times). But primarily Bion located an idea of the group as an organism in its emotional "sharing," its continually shifting member identifications, and its different moods.

Bion does not discuss the concept of the nature of the laws we should seek, but he does speculate about the problems that such laws would try to encompass. The question of what brings one mood to an end, and what initiates another, seen in terms of group anxiety on the one hand and individual valence on the other, seemed to us to be central. So far as I know, Bales [1] and his associates, using his "interaction process recorder" offer the only other way of looking at this problem; but the failure to identify "natural units" seemed to us to make the system less useful for our purposes.

The request to "identify the chief classes of experimental and/or empirical data which have served as the initial source of evidence on which the system was based, or which have been used in any way to suggest the major assumptions of the system" presents me with difficulties. A system is not based on data; it is based on more primitive systems.

System building, of course, involves the use of data, but the organization of the system lies in meta-theoretical thinking, and it is this which makes a system a system.

During the period 1946–1950 we carried out a number of experiments which now may be seen as preliminary to the present work. Those experiments helped us define “major assumptions,” less by “suggesting” them than by “bringing them into the open.” Thus a whole series of experiments deepened our implicit conviction that data on emotions and attitudes expressed by group members are central and primary. The work of Withall [56], Flanders [15], and Perkins [36] showed that numerous dependent variables can be predicted from knowledge of the teacher’s “intentions” to support and help the pupils as distinguished from supporting and helping himself. (Actually, he needs to do both, but this was a useful initial approach.) Steinzor [41] and Blocksma [10] also developed categories of “leader intention” along the same lines.

The notion that purely objective data were of little value was also tested, primarily because the research problems involved in dealing directly with emotion as data are troublesome, and we wanted to be sure that it would be really necessary to tackle all the problems of bias, subjectivity, and so on. Accordingly, Marks [32] made pictures every 15 sec by time-lapse photography, and then counted such overt behaviors as can be perceived visually. For example, he counted the number of frames which showed each student standing within 3 ft of the teacher during a “creative arts” class. In this class, the expectation is that students are very much on their own, and it seemed reasonable to suppose that a pupil who spends a great deal of time in the vicinity of the teacher—possibly “waiting for” the teacher—is probably a rather dependent child in the arts class situation. The purely objective evidence enabled Marks to pick out several pupils most often found close to the teacher. Marks then had all the teachers of these same children rank them in order of their tendency to “be dependent” on the teacher. The teachers’ judgments showed practically zero correlation with Marks’s ratings. Moreover, interviews with the students showed that many hypotheses other than “dependency” may explain why a student keeps close to the teacher.

Such a study is consonant with the belief that, by themselves, completely objective data are essentially meaningless; hence they must be complemented with other kinds of data which give some clue as to the subjective meaning to the actor of his overt behavior.

At this point, we had a choice as to where, in the chain of thinking, to introduce the concept of purpose, attitude, or intention. The two possibilities seemed to be: (a) collect data, however subjective, in the actual situation—thus introducing the subjective material right at the

beginning; (*b*) regard emotion or purpose as a construct (probably an intervening variable) to account for the observed objective aspects of behavior. We rejected the latter because it seemed to us that no new theory could ever be produced from it.

Thus our empirical independent and dependent variables refer to "what is going on," but this is understood to mean not only "what is the overt behavior" but "what does it appear to mean to the actor" and "what does it appear to mean to the others in the group." Affective aspects of behavior are rated because these overtones imply both how the actor evaluates the situation in which he finds himself and also what his purposes or needs are. In addition, we wanted to have a record of the activity engaged in, that is, the aspects of behavior related to externalized purposes or needs. The categories of emotion and work are the chief empirical variables, and they have been described earlier in this paper.

But emotion and work are not completely empirical categories. They imply much more than a simple, specific, single, univocal, unitary act: they imply a need, an expectation, a kind of feeling about self, and so on. To make a rating is an act of comprehension, not simply an act of recognition. The distinction between "empirical" and "systematic" independent or dependent variables can be made only in the grossest terms, so that I would be quite puzzled over where one begins and the other leaves off in our work. I might say that, since our ratings can be counted and summed and divided by n , all the manipulations that can be done arithmetically define the empirical portion; the place at which the researcher's intelligence becomes necessary then defines the "systematic" part of the construct. This may accord with the spirit of our age of specialization, but it is probably an industrial rather than a methodological argument.

CONSTRUCTION OF FUNCTION FORMS

Generally speaking, the precise mathematical expression of linkages between constructs is the most elegant way to state hypotheses for pinpointed, effective demonstration. This is clearly a sophisticated step which can only be taken after the system has been developed rather thoroughly. Our system has not been developed to the point of precision required for this kind of treatment.

There are two bases for "function forms" within our system as developed so far. One basis is methodological, the other propositional. The methodological basis is a clearly defined sequence of operations that the researcher carries out; that is, the course of inquiry is rather completely described as a series of steps each of which summarizes pre-

ceding steps and anticipates next steps. This has been described at some length earlier. The procedure provides a set of *operational* stipulations among *constructs*; i.e., the constructs are interrelated through what the researcher does with them rather than through "constitutive definitions" apart from the researcher. The propositional basis is a sequence of statements which guide the inquiry conceptually rather than operationally.

Our stipulations with respect to the interrelationships among variables thus tend to be "general adumbrations of the functional relationships," to cite the outline. As for our confidence in this way of formulating stipulations, I can only say that it is akin to the confidence of a parent, who has watched his child sit up, crawl, and then walk, that someday he will talk. At the current stage of development, our confidence must lie in the expectation of a "normal" course of growth, within which we are at a recognizable stage.

During the course of growth, however, it is difficult to know how far growth will proceed. There is a great discontinuity between thinking about research as a publicly conducted need-meeting inquiry and thinking about the organized product of research. Are mathematically expressed function forms, which we tend to accept as the scientific last word, really the ideal of social psychology? Or are they simply conveniences for effective testing of hypotheses? Is social science, like physical science, capable of meaningful formulation in the language of mathematics? Or will it require the development of some other language? Lewin, for example, felt that a nonmetric mathematics, hodology, would be most appropriate for dealing with social-psychological phenomena. We might conclude that the language of social science will be mathematical but new kinds of mathematics may have to be devised. Certainly we mathematical amateurs can sense that some recent developments are reassuring. The work of the Center for Mathematical Biology at Chicago in elucidating mathematical theory of organisms, the studies of the cybernetics people in taking account of feedback, possibly, the research of the Rand Corporation in its pursuit of stochastic applications to decision process—all these hold out hope that a useful, rather than *merely* a formal science can be developed. Meanwhile, the best apparent strategy is to continue studying human beings, to formulate the best backlog of propositions that we can, and try to get into communication with the more approachable theoreticians among the mathematicians. Certainly there seems no good reason for confining our studies to the sorts of things that mathematical amateurs can handle with correlation and covariance.

I have indicated that our intervening variables enable us to move from one domain to another: from individual to group, for example. Such variables are necessary to the extent that we view human enterprise

in terms of part-whole relations, in which every part of a whole is also a whole made of smaller parts. The terms independent-intervening-dependent apparently refer to the course of investigation rather than to the structure of the system. We may find better terms for the latter. Toward this end, terms such as "input, output, internal, and external" may be suggestive.

MENSURATIONAL AND QUANTIFICATIONAL PROCEDURES

Our quantification is actually prequantitative in every sense except that there is usefulness in the simple ideas of "more than" and "less than." We note, for example, three instances of two-level work during a particular minute within a meeting. Literally, this means that at three different times the observer thought he had enough evidence to justify such a rating. It is the observer's opinion that we count, and aside from the fact that he probably has different degrees of confidence in his three judgments, it may be that we could call each judgment a "unit." (As a matter of fact, it works surprisingly well.) Yet, speaking rigorously, we are on shaky ground. Do all the three contributions have equal influence on the group? Are they all equally valid indexes of the group state of affairs? Probably not.

We could ask similar questions about our weightings of four "qualities" of work. Again, the quantitative aspect is simply one of ranking—fourth-level work seems to embody most fully the conscious seeking of "reality" that the concept of "work" connotes. Third-level work is a less complete realization, second-level even less, and so on. But nobody would seriously claim that a fourth-level statement is "worth" two second-level statements. A further source of difficulty is the fact that we count frequencies, which we interpret as intensities.

These problems arise from the interesting fact that our measuring instrument is the nervous system of the observer. Someday, neurology may have much to offer our ideas about quantification. Meanwhile, we shall continue using our current procedures.

The biggest problem of quantification is also the biggest problem of conceptualization: we think we are studying phenomena "out there," objectively, which we cannot do; or we talk as if we were studying our own reactions to the phenomena, which is partly true. But what we need is some way to talk about the interaction between observer and phenomena—for that is what we are really dealing with. If this is the case, should we try to find that imaginary line between observer and phenomena, and subtract the observer from the total? This would give an "objective" science, but what would it mean? Could somebody else come along and use this "objective" science objectively, or would he already

be interacting with the situation so that he must add a liberal dose of self-knowledge to the science in order to be able to reach any reasonable predictions or explanations? I can offer no reply to these questions. Yet they point to what may be a matter of crucial importance; namely, in social psychology we always study phenomena from a position of involvement in them, and this is fundamentally different from our external position with respect to physical phenomena.

FORMAL ORGANIZATION OF THE SYSTEM

First among the ideas comprising the system are the postulates, ideas that guide the processes of research. These postulates, introduced at the beginning of our discussion of the Structure of the System as Thus Far Developed, are the policies by means of which relevant factors are sifted from irrelevant and attention is directed to the problems of research method. The postulates show us how we must go at the job of investigation, and they suggest the methodological problems that we shall have to solve. They help us avoid such fallacies as looking for the lost coin under the street light simply because there it is light enough to see.

When used in this way, postulates specify the nature of the interaction among researcher, phenomena, and classes of concepts. We decide, for example, to act as if interaction were sequential and contained distinguishable "natural units." This notion has apparent validity in the sense that it seems to fit our own past experience; it "feels" right and we can already point to things we have experienced that seem to bear it out. It is not, however, a hypothesis because it is not subject to test; it is actually a criterion of method. We must seek until we can find a method of research which identifies "natural units" within the sequence. If a method does not result in the finding of natural units, the method is abandoned as unsatisfactory; it somehow is not appropriate to our internalized understanding of the nature of the phenomena we are dealing with.

A glance at the postulates will show that we set ourselves a rather knotty set of methodological problems to solve: how to discover "hidden agendas"; how to classify and record "feelings"; how to diagnose stresses to which individuals are susceptible; how to view the group as an "interactive network" even though all the observable behaviors are produced by individuals; how to conceptualize the "interaction between personality and group." Postulates are dredged up from within the prior experiences of the researcher, and they serve as axioms capable of endless elaboration in a large number of experiments. They tell us which ways of proceeding are fruitful and which will not pay off; and they give continuity to diverse investigations over the years. They are formulated through con-

scious effort to become aware of the meaning of one's biases and predilections with respect to research designs.

The propositions, on the other hand, are not instructions to the researcher so much as a portrayal of the broad fabric of human experience. They spell out the "whole" of which the situation studied is a "part"; or they delineate the "ground" against which the universe studied is "figured." Propositions are high-level abstractions which spell out the nature of the phenomena being studied as seen from "within" the phenomena rather than from the point of view of the observer. Perhaps the distinction between postulate and proposition can be made clearer by noting that the postulates imply the researcher's purposes, whereas the propositions imply the rationale of the actors being studied. For us, a single proposition makes no sense; the whole set is required to communicate the conceptual orientation within which theory is to be developed. Propositions communicate the nature of the relationships being studied; they provide the "form" which is then fleshed out in theory.

Our "theoretical" statements are conceptual definitions of the variables, especially the "independent" and "intervening" variables discussed earlier. These are related to behavior through their operational definitions. The same variables are studied in all situations, but their operational definitions may vary from situation to situation in the sense that different behaviors may be counted as evidence for the same variable. Thus there is a large number of behaviors which we call "flight" and in any particular situation of flight only a few of these behaviors appear.

In our work, the body of propositions is rather more coherent than the collection of theoretical generalizations produced directly from research. The reader may believe that actually we are rather more interested in the propositions than in the theories, that we regard our theories mostly as statements that help us spell out and modify our propositions. I do not know how sound this would be, but I am convinced that it is the propositions rather than the theories that men live by. It is propositions rather than theories which become embedded in cultures and thus determine how a given people at a particular time and place will govern their lives. For me, the elegance, difficulty, and technical virtuosity of theory is an object of appreciation but the internalized and homely understandings communicated in propositions represent the more fundamental and significant contact with "reality."

SCOPE OR RANGE OF APPLICATION OF THE SYSTEM

We shall consider that the scope of the system is the same as the scope of the propositions about groups presented earlier. The range can be specified by mentioning the kinds of problems the system can deal

with. The first problem, and the one on which most experimental work has been done, concerns the relationships between individual personality and behavior as a member of a group; the system is especially adequate to predict and explain these connections. The second problem derives from the first: the performance characteristics of groups composed of different combinations of personalities. The third problem is concerned with the formation of subgroups and the explanation of group dynamics as interaction among the subgroups. The fourth problem is the diagnosis of "hidden" agendas and buried purposes within the group. For these types of problems, the methodology is complete; we know how to proceed to explain and, within limits, to make a variety of specific predictions.

As explained earlier, the methods and experiments have been mostly concerned with one kind of group, the "human relations training group." Such groups were used because in the normal course of their business together the members produce a great deal of explicit information about their feelings, the problems of the group, and other relevant content.

There is no reason why the problems described above cannot also be dealt with in respect to all kinds of meetings of all kinds of groups. The differences of applicability of the system would come from problems of accessibility of needed data, not from theoretical difficulties.

There are two areas of application which have been tested in experience, but not formally experimented with. The first is the creation of methods and procedures for groups to use to achieve their own particular purposes. This area of application is suggested because the diagnostic methods are useful for understanding the inner and outer demands that groups must handle. Thus, the research concepts have been practically useful in the creation of a citizen movement for rebuilding the Hyde Park community in Chicago [50, 53]. More recently, the ideas have played a major role in the creation of a "new" program for training elementary school teachers at the University of Chicago [49].

The second area of application is the study of cultural differences. At the present time, for example, I am one of a team engaged in traveling about Europe and organizing workshops in human relations. Our training "philosophy" and methods are the same in various countries, and in most we have two weeks, full time, to work with one or more groups. In effect, we are a uniform probe (or irritant) injected into each country; and the diagnostic elements of the system have enabled us to define in a preliminary way a great many differences among the groups of a sort that are usually thought of as reflections of differences in "national culture [48]." In other words, when studied intensively, the actions of a group may throw considerable light on the basic assumption of the larger culture.

It should be clear by now that there are many overlappings in approach among the various systems now being developed. Work on perception, sociometrics, leadership, and group composition may all be easily assimilated in the framework of propositions we have discussed. We need to reach out in three directions: first, into more elaboration of the external system which, at present, is represented mostly in the categories of work and in some of the hidden problems of the group. We have tended to pay close attention to "process" and to use such structural concepts as were needed to describe the situation within which the processes were occurring. A more sociological approach is to describe the structure as fully as possible, and call on the psychologist only as needed to understand some of the dynamics associated with strains in the structures. These approaches begin at different ends of the phenomena, and they should be pulled together.

Much new development is also required to nail down the demands of tasks. Very little work has been done in any systematic way for the purpose of finding basic dimensions for classifying the task-demands a group must deal with in order to achieve its purposes.

The third direction, in this case for more reaching out as well as for further development within the system, is in the explication of the intermember "identifications" which are at the psychological heart of "groupness." We need to assimilate more psychoanalytic thinking within our generally psychiatric approach.

HISTORY OF SYSTEM TO DATE IN MEDIATING RESEARCH

Several researchers—mostly working for the doctorate—have found a place in the developmental program and have made substantial contributions to it. Most of their original work is incorporated in dissertations in the department of education and psychology, and in the Committee on Human Development at the University of Chicago, but some studies are abstracted in the two monographs [44, 54] referred to in the opening paragraph of this paper; the monographs also include a few additional studies.

It is clear in retrospect that the various researches often initiated in seeming independence from each other, fit together within a developmental series of investigations. This fact derives both from the influence of the student's major adviser and from the influence of the "culture" of the Human Dynamics Laboratory: even when working separately, the students have generally identified themselves as the staff of the laboratory. Although their needs and interests have been different, they have tended to speak a common language, to work together in training

and workshop situations, and to serve as a manpower pool on which researchers could draw for skilled help when needed.

In 1944, Thelen [46] compared the learning of freshman chemistry classes. Half the classes did their laboratory work from typical laboratory manuals; the other half planned their own experiments under the guidance of the teacher. Although not a central object of inquiry in this experiment, the greater self-direction and efficiency in the laboratory of the "planning" groups was noted, but this was primarily a result of better cognitive orientation and prerehearsal, during planning, of the experimental manipulations.

In 1948 and 1949, the experiments of Withall [56], Flanders [15], and Perkins [36] demonstrated the importance of affective communication between teacher and student. Affect was seen as a concomitant of the "intent" of the communicator, and was found to influence recall, anxiety, perceived "feeling," and a number of physiological measures. During this period, Rehage [38], in an experiment involving teacher-class planning, noted the importance of the teacher's response (or lack thereof) to feelings expressed by students; and he also found marked development of cohesion in the sociometric pattern of a class which had shared intense feelings together.

The effort to understand why teachers (and other leaders) responded as they did to the feelings of others led Glidewell [17] to the experimental study of interpersonal anxiety as related to the behavior of the leader. The four members of the group were trained to play the roles of people who had been identified as either anxiety-producing or anxiety-allaying through clinical study of the leader. They were trained to make "supportive" or "threatening" statements, and observers correctly spotted these two types of statements by noting their consequences in the deterioration or "strengthening" of the subject's style of leadership.

About this time (1950) Thelen [47] published a methodological analysis of the postulates required for research on groups, and this forecast with some accuracy the nature of succeeding work in the laboratory.

In the same year, deHaan [13] attempted to use Bion's emotion and work concepts as a basis for sequential plotting of group interaction. The results, though crude, were encouraging and led to considerable further effort at refinement and systematization of the method. During this period there were several other tests of the usefulness of the concepts of emotion and work tendencies in personality. Thus Stock [43] had fair success in predicting sociometric choices; D. McPherson [34] showed that an individual emotion-work sentence completion test was more useful than the TAT for predicting emotion and work behaviors in the group. In connection with some research with the Air Force (1952), J. McPherson [35] showed relationships between e-w personality pat-

terns and tendencies to distort the meaning of close-to-self written materials; B. Sarchet [39] used the same personality data to predict roles of members in groups of officers. Working under the same contract, Glidewell [18] showed that certain characteristics of the solutions to problems, worked out by 50 different 12-man groups of officers, could be differentially predicted from knowledge of the e-w patterns of the groups and from knowledge of the groups' standards controlling the expression of feeling (e.g., labile, constrictive, integrative). These notions of control, incidentally, have yet to be worked into the over-all system. In the same year, Freedman [16] studied the way eight different teachers dealt with emotionally charged discussion following presentation of a provocative standard dramatic story of their classes; and he related their "styles" both to teacher anxiety and to lack of congruences between the perceptions of the teacher and the students.

Beginning in 1951, the experimental program summarized in the two monographs was begun under the auspices of the Group Psychology Branch of the Office of Naval Research. This work was brought to a close officially in July, 1956. The first monograph [54] reports the development and validation of the basic e-w assessment instrument (the Reaction to Group Situation Test) by the Gradolphs, Stock, and Hill, the development of the method of sequential analysis and interpretation by Stock and Thelen, the development of an objective method for "unitizing" interaction by Ben-Zeev, and the method for studying inter-subgroup dynamics by Stock and Hill. The second monograph [44] gives the substantive findings of Stock, Gradolph, Hill, Glidewell, Lieberman, and Mathis with respect to behaviors of individuals in groups, group composition, "trainability," and productivity. In 1956, Thelen [52] published a preliminary over-all statement of the propositions and aims of the research.

As this is written, Stock and Lieberman are applying the system to the study of focal conflicts in therapy groups and Hill is studying growth and development of therapy groups. These workers are located in VA hospitals and training centers.

Probably, these are the aspects of the system which have had most to do with its usefulness for instigating research:

1. It is still developing, so that there is a continual challenge to creativity.

2. Its scope is such that students can use it in a wide variety of situations and for a wide variety of purposes.

3. It deals with problems and uses ideas which are personally involving and exciting.

4. The research has tended over the years to be seen as a team operation, and students have had a feeling of "place" in the program.

EVIDENCE FOR THE SYSTEM

The evidence for the system as a coherent body of useful ideas is probably of three sorts. Firstly, we can make valid predictions with it. The fact that some of the operations of prediction cannot be carried through as yet in the public domain does not operate against the system; it does mean that the theory needs further development. Secondly, as trainers in groups we habitually employ the concepts to help guide the training process, and we find the concepts useful. Thirdly, the major ideas of the system can be communicated to students and they can use them for more effective participation in groups.

I am not aware of any incompatible data. So far we have assumed that an encounter with such data meant merely that our concepts needed further refinement, not that the data were embarrassing. For example, when our initial predictions, using the concept of valency, were not borne out, we discovered that we were using the term in three different senses. Once these had been made explicit and used appropriately, predictions were vastly improved.

The most critical test of the system will be through the method of blind prediction described as experimental design 5, under Structure of the System as Thus Far Developed.

No other system I know gives as adequate or useful a picture of group process: hidden problems, transitions and phases, group climates, and emotional phenomena in the group. Some other systems, such as Homans's, seem more complete in offering a better over-all view of group life in relation to institutional and community factors. Our system is stronger in relation to personality factors.

EXTENSIBILITY OF METHODS AND CONCEPTS, PROGRAMMATICITY, AND STRATEGY FOR DEVELOPMENT OF THE SYSTEM

The work on the system has, if anything, strengthened our initial "convictions" and postulates as discussed earlier under Initial Evidential Grounds for Assumptions and Formal Organization of the System. Hence, I feel that these ideas may be fundamental to social psychology. I further think that the use of sequential analysis as an aid to the diagnostic process is worth considering seriously in a wider range of behavioral studies; and that the frank recognition of the use the researcher makes of his own nervous system might help unleash more creativity.

The system is moving, I think, toward a broad generalized view of human behavior. It will be especially interesting to see how far the group concepts here experimented with can usefully apply to internalized

groups *within* the individual. If they can, then the system will expand to include some new ideas about individual psychology.

The program has been realized almost completely with respect to the research methodology, although simplifications may be possible; it is realized in broad outline as far as the underlying propositions are concerned, although extension is needed into more sociological and psycho-analytical concepts. So far as theory development is concerned it is only partially realized. A great deal of effort will be needed within a systematic program to work out and explicate the numerous cross relations possible among the variables.

With regard to the development of psychology in general, I hesitate to comment on the chief barriers to theoretical advancement. I have sought to make clear the kind of thinking and method that seem essential to me. Other people, with other needs, can make progress with other methods, and should be encouraged to do so. Nevertheless, the following general problems must be dealt with, possibly with fresh approaches, if we are to advance to new levels of reorganization and integration of ideas:

1. The problem of free will vs. determinism. Science must assume a deterministic position, but men resent, for good reason, the notion that they are without choice. We must learn how to work into our systems a variety of concepts taking due account of feedback, of learning, and of creative emergence into consciousness of "new" ideas. I find considerable encouragement in the failure of certain recent applications of stochastics to the decision process. These failures underscore the significance of this problem.

2. The problem of "probability" states. Our designs should stick closer to the facts of behavior: instead of comparing probability of our findings (significance) against "chance" or against regression from initial conditions, we should formulate *several* possible *final* states and assess their relative probabilities under the circumstances of the experiment.

3. The problem of developing mathematical functions more appropriate to studies of human interaction.

4. The problem of dealing with interactions between the researcher and the phenomena he studies.

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AN APPROACH TO PSYCHOLOGICAL THEORY IN TERMS OF THE THEORY OF ACTION

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Part I

INTRODUCTION

This essay is concerned with a rather special type of psychological theory; one which is expressly treated as part of a more general conceptual scheme embracing the processes of social interaction and the patterns of culture as well as the traditional subject matter of psychology. The term "general theory of action" has been used to designate the wider scheme which, in turn, can be broken down into several different parts or subtheories.

The most fundamental of these breakdowns is based on four reference points: organism, personality or psychological system, social system, and cultural system. All four are abstractions from and modes of analyzing the phenomena of the behavior of living organisms. They are not concretely, only analytically, separable. The system's central, but by no means exclusive, interest is in human behavior.²

The same concrete behavior usually involves all four reference points. In a sense to be explained later, the four *interpenetrate* each other. Yet the analytically articulated systems which we have isolated for theoretical analysis and for defining the relevant empirical data are not mutually reducible; the discrimination of the four systems is not merely tautologous.

Action constitutes systems, which involve the relations of one or more actors (i.e., behaving organisms or parts of them or collectivities

² From some points of view the terms action and behavior may be treated as interchangeable. One may suggest, however, that behavior be the term applied to the total complex of observables in a given case, action the term which includes both the observables and the theoretically postulated intervening variables and "covert" processes. It will also be noted that, compared to previous publications, we speak of four rather than three primary subsystems of the general theory of action; the organism, in certain aspects, has been added to personality, social system, and culture. This represents a definite theoretical innovation.

involving a plurality of them) to a situation, composed of other actors and/or "nonsocial" objects. The systems concerned are always *constituted by the relations* between one or more actors and one or more objects in its or their situation. This is a fundamental point: the actor is not conceived as *one* system, which acts in relation to a situation (or environment) which is then treated as *another* system; actor and situation *together* constitute the system of reference. This is as much the case for a psychological system, as a system, as for the other types. A "personality" conceived as devoid or independent of "object-relations" could not be called a system of action in these terms. This is much the same as saying that *behavior* is the empirical subject matter of the theory of action. The properties of a behaving organism, independent of its behavior in actual situations, are of interest to that theory *only* in so far as they condition or are otherwise involved in the behavior.

According to this conception, a social system is a system generated and constituted by the interaction of *two* or more individual actors, whereas a psychological system is a system of action characterized by the fact that all the behavior belonging to it is behavior of *the same* living organism.³ Again, whatever properties the actors may have which are independent of the processes of their specific interaction with each other are no subjects for the analysis of social systems except in so far as those properties bear on the interaction; i.e., are factors contributory to or involved in it as resultants.

Thus *by definition*, all concrete behavior belongs to some psychological system, and a very large part of it *at the same* time belongs to some social system. Yet the same organism participates in a plurality of social systems; conversely, the same social system—over a period of time—may be "composed" of different behaving actors and yet remain "the same system." The two are thus overlapping but also crosscutting modes of organizing the data of behavior for scientific analysis.

A cultural system is a system which defines and maintains patterns of the *meanings* of actions and of objects which function in the orientation of actors in psychological and social systems. Orientation is always the patterning of the relations of one or more actors to one or more objects in a situation. As a *generalized mode* of orientation, a cultural pattern is at least potentially applicable to more than one object and characteristic of more than one actor. Cultural patterns are transmissible

³ It is convenient to reserve the term personality strictly speaking for the *total* behavior system of a given living organism; it is thus parallel to society rather than to social system. Hence we propose that the term psychological system be used as parallel to social system, and personality as parallel to society. In these terms, subhuman organisms would certainly be defined as having personalities.

from one empirical action system to another. As components of psychological systems, they must be learned (from other actors or created through learning processes in the system in question). They may be "diffused" from one social system to another, or finally, embodied in physical objects which function as signs or symbols. When conformity with the "definition of the situation" embodied in cultural patterns becomes an aspect of the "structure of the system," we speak of them as coming to be internalized in psychological systems, i.e., personalities, and institutionalized in social systems. The *same* cultural patterns are both internalized and institutionalized.

In its relevance to the theory of action, the organism is that aspect of the physiologically functioning system which interacts directly with the personality and the other systems of action. It is the source of energy for all processes of action and the source also of a complex of essential facilities and rewards. Although based on a genetic constitution, its own organization is substantially influenced by the processes of conditioning and learning which occur in the life history of the individual. For many purposes, only part of the total concrete organism should be treated as part of the system of action. Later we will refer to this part as the "behavioral organism" and distinguish it as subsystem other than the "vegetative organism."⁴

The general theory of action maintains that these four orders of system, and various others which can be derived by analysis of them and of their interrelations,⁵ should not be treated as independent of each other except in the sense that differentiated parts of the same complex of phenomena are partially independent. Theoretically, the analytical schemes appropriate to the different systems should be derivable from a *common* set of postulates and definitions of fundamental variables and relations. Each subtheory should depend on parametric considerations which define empirical constants in which *the same* fundamental variables operate. It is thus necessary to differentiate within the general framework different classes of system and to relate these *different* systems to each other.

Within each of the four basic types of system, the applicability of the theoretical scheme is not limited to one particular "level" in the microscopic-macroscopic range. In social systems it applies all the way from the small experimental group to the large-scale society; in psychological systems, from a single stimulus-response pattern to the total personality; and in cultural systems, from the specialized set of "understandings" of a married couple, for example, to the total culture of a

⁴ As this term has been used by Franz Alexander. Cf. [1].

⁵ For example, we treat economic theory as dealing with a special type of social system, an economy. Cf. [27].

major civilization. In this crucial respect, the relation of the theory of action to the relevant empirical systems resembles that of mechanics which explains falling apples and the motions of the planets by the same set of laws.

We have noted that these four primary subsystems of action are not mutually reducible. No one of them has ontological priority in the sense that the laws worked out in relation to it have only to be "applied" to the less fundamental levels. On the other hand, the four systems are not arbitrarily juxtaposed so that the order of their relations does not matter. On the contrary there is a quite definite order, clearly an order of levels of organization and control. As seen in this order, psychological systems organize and control the behavioral organism, social systems organize and control psychological systems, and cultural systems organize and control social systems. Looked at from the opposite perspective the order is one of "conditions." Social systems provide the most immediate set of conditions on which the functioning and development of cultural systems depend, psychological systems provide a set of conditions underlying the functioning of social systems, and the organism provides conditions underlying psychological systems.

We postulate a complete continuity between biological systems and systems of action; from this point of view, action is a specialized aspect of life. It is essentially that aspect in which life processes transcend the internal mechanisms of the individual organism and the metabolic interchanges with the environment. The starting point for action is the organic differentiation of perception and locomotion from other functions and the consequent enlargement of the range of adaptation made possible, especially through control by the central nervous system.

The relative importance of the organic, psychological, social, and cultural factors is a function of stages or levels of the evolutionary scale, but in the order just cited the later terms grow more prominent as we move up the scale. All of them are discernible below the human level. Some kind of learning is apparently found well down the evolutionary scale and rapidly becomes more significant with organic development. However important the genetic constitution of the organism, however important, during maturation, may be the "unfolding" of behavioral capacities through the operation of genetically determined mechanisms, behavior comes increasingly under the control of *systematically organized learned processes*. With respect to these, organisms of the same genetic constitution may differ, but within the species there are uniformities determined by relatively uniform conditions of learning. Learned behavior is the focus of what we mean by psychological systems.

With respect to learning, psychological systems originate in the relations of the organism to the *total* environment. A focal point, how-

ever, is *interaction* with other organisms in which the *behavior* of the other organism becomes an essential aspect of the determinants of learned behavior for the organism of reference. Essential functions for the individual organism and for the species thus come to be dependent on the effective regulation of these interactive processes. A crucial intra-species case is sexual reproduction which is never *exclusively* regulated by the triggering of instinctive patterns of behavior even at the sub-human level but always involves some psychosocial regulation of the relations of the partners. To deny this would imply that on the environmental side meetings, including all the detailed conditions of successful joining, were exclusively a matter of chance encounters.⁶

If primary biological functions are dependent on social interaction, there must be mechanisms by which the behavior of interacting organisms is somehow made to *match*, so that there is a probability greater than chance that each, in response to the other, will come to perform the *appropriate* kinds of acts. When a plurality of interacting organisms tend to interact in systematically organized ways in relation to each other as the result of learning, we may speak of a social system.

What is spoken of as perception is clearly a matter of some kind of *organized* sensitivity to environmental conditions. It is not the same as the more simply "reactive" sensitivity of, say, the skin to prolonged exposure to strong sunlight, or the respiratory apparatus to pronounced diminution of the oxygen content of the atmosphere. The distinctive feature of perception is the reaction of the organism to stimuli, organized with reference to environmental events. But when social interaction appears, a further level of the *generalization of the meaning* of such events is added. The behavior of other organisms—and qualities which become associated with their behavior—come to be interpreted as "intentional" *signs* which guide the behavior of the organism of reference.⁷

The generalized patterning of the meanings of environmental objects and events is the focus of what we here mean by culture. In its most elementary forms it is not dependent on social interaction—Tolman's cognitive map [cf. "A Psychological Map," 22], for example, is a "cultural" factor in behavior. But only systems of social interaction provide the conditions necessary for the most distinctive phenomena we associate with culture, notably the *transmission* of systematically patterned meanings from organism to organism without each undergoing independently the original learning experiences by which the meaning-pattern was established. We can clearly speak of sign-behavior, meaning, generalization and communication on subhuman levels. But the organiza-

⁶ Important evidence on these processes will be found in [33].

⁷ A classical study of a rather elementary form of socially interactive sign behavior is that of von Frisch on the behavior of bees.

tion and transmission of meanings, independent of experiential context, seems on a large scale to be specifically human. The focal mechanism of course is language.

Whatever the subhuman antecedents and prototypes, when the subject matter is human sociocultural behavior, there can be no doubt of the involvement of all four of the system levels about which the present discussion will revolve. When we deny that any one of the systems higher in the order of organization and control is "reducible" to determination by a lower-order system, we mean that independent significance must be attributed to the phenomena of *organization* on each level as defined by: (a) *selectivity* of inclusion of and emphasis on components available from lower-order systems, and (b) distinctive *patterning* of the relations of the components selected.

Thus we assume that the same laws govern metabolic processes in "vegetative" tissues, on the one hand, and in eyes, nervous tissue, or great skeletal muscles, on the other. But tissues and organs which are differentiated and specialized with reference to behavioral function in the organism are *not indistinguishable* from those specialized with reference to vegetative function. The same biochemical-physiological components are differently selected and *organized*. Similarly, in one sense, the sensitivities to environmental influence which are brought together under the heading of "capacity to learn" are not different from the common features of the "irritability" of protoplasm. Within the concrete organism, however, learning becomes a specialized function in the operation of which the *proportions* of the basic components are different from those general to all organic functions. Further, *effects* of learning as determinants of the subsequent behavior of the organism come to be perpetuated. These two facts are not simply matters of the "average" functioning of protoplasm, but involve a different selection and mode of organization of the physiological components.

Again, other types of learning and that associated with social interaction, i.e., with continuing sensitivity to the *behavior* of other organisms, have essential features in common. But the behavior focused on social interaction comes to be differentiated from that centering on physical objects alone. Among the general mechanisms of learned behavior there is selectivity and special organization of those appropriate to the regulation of social interaction. Finally there can be distinctive selection and patterning of relations where the primary focus is on a system of meanings as such rather than on the processes of social interaction. The components or "building blocks" out of which cultural systems are constructed are the same as those built into psychological and social systems, but this does not mean that the systems themselves are identical.

The preceding discussion should have made it clear that the degree to which it is important to discriminate among these different levels of system analysis is by no means uniform for all problems. The relative importance of discriminating seems to increase with the level on the evolutionary scale which is being dealt with, and with the degree of theoretical refinement to be reached. For many problems of animal behavior, it has not seemed very important to discriminate behavioral organism and psychological system. Much of the *content* of the human personality system is derived from social interaction, however; i.e., it consists of "internalized" social objects. Hence, when we deal with human personality, serious distortion may arise from the attempt to assimilate this content to a paradigm of structure and function of the organism independent of the structure of social systems.

Traditional social science has tended to rest content with discriminating between heredity and environment, in the sense of general biological theory and then, within the category of environment, distinguishing the factors distinctive to the human environment. This has been the key emphasis in the concept of "culture." In this sense, three of our four systems are primarily "cultural"; for more refined purposes, it becomes necessary to make further discriminations. I shall therefore use the label "culture (I)" for culture as this very general designation of all factors in human behavior except those determined by biological heredity and the physical environment. Culture (II) then labels the sense in which I have used it, which differentiates it both from psychological systems and from systems of social interaction.

The foregoing discussion indicates the broadest frame of reference within which I shall attempt to outline the main structure of the theory of action and its application to psychological theory. Now, however, let us turn briefly to the first rubric of the outline.

BACKGROUND FACTORS AND ORIENTING ATTITUDES

Background factors. Academically, the author of this essay began as and still is a sociologist, not a psychologist. Like any person professionally concerned with the scientific study of human behavior, from the beginning I had a certain level of awareness of and concern with psychological problems. But this did not become what could be called a genuinely technical interest until relatively late in my career.

That career began on the border line between sociology and economics, but with considerable undergraduate background (at Amherst College) in biology and philosophy. Graduate study was at the London School of Economics and at the University of Heidelberg,

Germany. My full commitment to sociology rather than economics did not come until after graduate work—unlike an American doctorate, the German program was sufficiently general to leave both doors open.

Within economics, my primary initial focus was what the 1920s called “institutionalism,” which naturally led to sociological interests. These were reinforced by the atmosphere of the London School, and even more, in Heidelberg, by acquaintance with the work of Max Weber. My dissertation there dealt with a subject in the field of economic institutions, the treatment of “capitalism” as an institutional system in German economic literature, particularly by Marx, Sombart, and Max Weber.

Interest in economic institutions evolved into interest in how leading economists had handled the sociological border lines of their theoretical problems, and conversely, how sociologists had handled the problem of the place of economics in their thinking. On the economic side this led to intensive study of the work of Alfred Marshall; on the sociological, in addition to Weber, to Emile Durkheim, and Vilfredo Pareto, a case spanning both disciplines. The outcome of this series of studies was *The Structure of Social Action* [21], in which the conception of a theoretical system first clearly emerged in my thinking. For all the diversity of background and empirical interest, the work of these writers, the book held, embodied a common conceptual scheme for the analysis of social systems containing at least the beginnings of a generalized theoretical system. Economics had such a generalized scheme; it seemed to be the moment to search for an equivalent in sociology. The basis for such a scheme could be quite different from the older “speculative” evolutionary theories of which Herbert Spencer’s was the prototype.

My focus in sociology was on the comparative treatment of institutions. As a result of the influence of Malinowski and Hobhouse and Ginsberg in London, it also included a strong interest in social anthropology and its treatment of primitive societies.

As yet, however, I had no genuinely technical interest in psychology.⁸ That first developed in connection with a set of problems concerning the treatment of motivation in the traditions of economic theory. Its key conception was that of the “rational pursuit of self-interest,” the most general available statement of what underlies various versions of the “profit motive.” I early became convinced that this could not be treated in the usual sense as mainly a psychological generalization—what has sometimes been called by economists a “propensity of human

⁸On a more methodological level I was, however, considerably influenced about this time by two psychologists, namely, Tolman, through his *Purposive Behavior* [35] and Köhler through the *Mentality of Apes* [14] and *Gestalt Psychology* [15].

nature”—largely because so-called capitalism is not an institutional feature of all human society. Hence I decided to study medical practice as an example of the contemporary professions where, on an ideological level at least, it is categorically denied that economic self-interest does or should be allowed to govern behavior.

One outcome of the study was definitely to confirm the hypothesis that, although the doctrine of self-interest may be a valid empirical generalization about motivation in modern business, it does not necessarily hold for the professions. The difference, however, is not primarily between types of motivation in a psychological sense, but rather between institutional structuring of the situations in which the respective groups act.⁹

In addition, a psychological interest which was both broader and more technical emerged from the same study. Its starting point was the “psychic factor in disease,” as manifested in either psychosomatic or behavioral symptoms, a conception which was coming to be intensively discussed in medical circles (about 1935–1936). In this connection, I first undertook careful and intensive reading of Freud and the work of other writers in the psychiatric field.

This study confirmed my central view about occupational motivation, above all because this type of psychology provided a clue essential to understanding the functional basis of the institutional patterning of medical practice. In analyzing the “irrational” motivational factors in the relationship of doctor and patient I became aware of their reciprocal interaction on unconscious levels and the bearing of this on the patterning of their respective roles. These insights in turn widened into a general interest in the problems of the relation between motivational structures, broadly on the level on which Freud treated them, and the institutional structure of the situation in which action takes place.

Various other writers, above all my own colleagues Allport and Murray, and W. I. Thomas and later G. H. Mead played important parts in this development of psychological thinking and interest, but in a variety of ways I kept coming back to Freud. This motivated seeking as much psychoanalytic training as a nonmedical person was permitted to acquire.¹⁰

Interest in developing relations between the “clinical” level of psychological theory and the sociological analysis of institutional structure was pursued for a considerable period and in a number of directions. First there was an extension of the interest in medical practice as a social

⁹The fullest report of the results of this study will be found in “Modern Medical Practice,” Chap. 10 of my later book, *The Social System* [22].

¹⁰Under the Class C program of the Boston Psychoanalytic Institute. I eventually became, and am now, an affiliate member of the Boston society.

phenomenon and its place in our society as a mechanism of social control. Secondly there was an interest in kinship and family structure, and their relation to the processes of socialization and social control. Finally, there was an interest in the "social psychology" of certain mass phenomena in their relations to macroscopic levels of the analysis of social structure.¹¹

This was a genuinely technical psychological interest, but a specialized one in two respects. It was overwhelmingly defined by the ways in which certain psychological materials fitted into the sociological problem-contexts I have briefly outlined, i.e., their relations to the social system. Secondly, the bodies of psychological theory which I studied most thoroughly were those of Freud and certain derivatives from him (e.g., Murray, Kardiner, Horney, Fromm, etc.) and the special sociologically oriented social psychology of Thomas and Mead. During this period, I was less intensively concerned with matters of general theory. Nevertheless, the extension of theoretical synthesis beyond sociology to include at least certain parts of psychology and of the "cultural" interests of anthropology had been gradually taking place.

A critical set of steps occurred in connection with a program of theoretical stocktaking which was carried out by various members of the Harvard Department of Social Relations with the help of E. C. Tolman and E. A. Shils as visiting collaborators in 1949-1950. One outcome of this project was *Toward a General Theory of Action* [26], whose contributors included, besides the editors, Shils and me, E. C. Tolman, G. W. Allport, Clyde Kluckhohn, H. A. Murray, R. R. Sears, R. C. Sheldon, and S. A. Stouffer.

For me at least, this period of stocktaking resulted in clarification of the fundamental bases of the theory of action, a tighter organization of its various theoretical components, and an extension of its technical relevance into areas about which I had previously had only rather general impressions.

Perhaps the most important single result was clarification of the *relational* reference of all action theory—to actor-object relations which

¹¹ Besides the chapter already cited, the following papers offer examples of the first line of interest: parts of "Motivation of Economic Activities" (1940) and "Propaganda and Social Control" (1942). "Age and Sex in the Social Structure of the United States" (1942), "The Kinship System of the Contemporary United States" (1943), and "Certain Primary Sources and Patterns of Aggression" (1947), present instances of the second line of interest; and with the third there deal: "The Sociology of Modern Anti-Semitism" (1942), "Propaganda and Social Control" (1942), "Democracy and Social Structure in Pre-Nazi Germany" (1942), "Some Sociological Aspects of the Fascist Movements" (1942), "The Problem of Controlled Institutional Change" (1945). My *Essays in Sociological Theory* [23] includes most of these papers and a complete bibliography to 1953.

could not be abstracted from the relationship and ascribed to one or the other relatum apart from it. The central application was to the concept of value—as concerned with the relation of actor and object. For example, Max Weber had placed values in the actor, as “subjective” in that sense (I had tended to follow him in this); whereas W. I. Thomas placed values in the object (as in his well-known distinction between attitudes and values). Neither view seems satisfactory. Once values are treated as relational, however, belonging neither in actor nor object, but characterizing the relation between them, then making values the focus of the *organization* of systems of action becomes immediately feasible. Along this path, a fundamental solution of the problems of the nature of internalization and institutionalization and their relations to each other was made possible.

A second important result, was establishing a clear logical relationship among three fundamental reference points for the analysis of systems, namely, personality, social system, and culture. This was done by showing how they could all be systematically derived from the basic frame of reference of action. Only much more recently have I begun systematically to relate the other systems of action to the organism.

Finally, the most fundamental extension was into the field of “behavior psychology” on the level of animal learning and elsewhere. For clarification of the starting points for this extension I have above all Tolman, but also Sears, to thank, and for a great deal of follow-up—much of which will be included in this essay—James Olds. The reductionist trend of much behaviorist psychology, particularly perhaps of Watson and Hull troubled me. I did not see how behaviorist theory could be so adapted as to recognize that the theoretical contributions of sociology and of personality psychology on the Freudian level dealt with more than epiphenomena.¹²

This more general stocktaking proved also to be the occasion for a further reconsideration on my own part of the status of the sociological branch of the theory of action. This resulted in the publication of *The Social System* almost simultaneously with *Toward a General Theory of Action* [22, 26].

The broad outline of general theory documented in those two publications was still not complete, however; there occurred further internal developments in the main structure which were documented in the *Working Papers in the Theory of Action* [24], written in collaboration with R. F. Bales and E. A. Shils. This represents a position on the more general theoretical levels which has remained essentially stable and pro-

¹² This is one reason why at an earlier period Tolman and Köhler impressed me. They certified that a type of psychological theory different from that of Hull and Watson could be scientifically respectable.

vides the main outline for the present exposition.¹³ Intimate collaboration with Bales and digestion of the implications of his research on small groups was perhaps the most important new influence operating in the intervening period.

In the years since the *Working Papers* appeared, the most important theoretical work has been "application" and refinement through codification of previously available materials and extension of theory into two principal, and widely different, fields. The first of these is the relation between family structure and the socialization of the child as illuminated by the sociology of the family, the analysis of small groups, comparative kinship, Freud's theory of psychosexual stages, and the psychology of learning. This has been documented in *Family, Socialization and Interaction Process* with Bales, Olds, Zelditch, and Slater [25]. The second is a reconsideration of the status of economic theory in its relations to sociology. This study has shown that economic theory is a special case of the general theory of social systems, and hence of the general theory of action, documented in *Economy and Society* [27]. Thus, the same conceptual scheme has proved to organize available facts and empirical generalizations on a detailed level in such divergent fields as the socialization of the child and the functioning of the modern industrial economy. This result increases confidence that the theory of action does, in fact, possess a high level of both generality and power in the analysis of empirical materials. We have made tentative beginnings of a similar exploration of the relations of political theory to the general theory of action, but the results are not yet ready for publication.

Orienting attitudes. One of my most important intellectual impressions was derived from A. N. Whitehead's conception of science, particularly as stated in his *Science and the Modern World* [37]. Three points stand out: first, his strong emphasis on the importance of systematic theory and the special power inherent in a well-integrated theoretical system; second, his views of the nature of the abstraction involved in scientific theory, particularly as related to what he called the "fallacy of misplaced concreteness"; third, his view of the continuity of the whole empirical world including both physical and social-behavioral areas. Thus his use of the concept "society" to refer to phenomena of atomic physics seemed to me more than merely metaphorical. Certain "organic" or in some sense "antiatomistic" features of his views on all these levels have appealed to me. I have never been attracted by theories which have tried to build up behavior systems out of discrete isolated conditioned reflexes alone, or social systems out of discrete isolated "individuals"

¹³ Chap. 3 (with Bales), "The Dimensions of Action Space" and Chap. 5 (with Bales and Shils), "Phase Movement in Relation to Motivation, Symbol, Formation, and Role Structure."

alone. The Whiteheadian views of the importance of relational interconnectedness in systems, of *organization*, have appealed to me profoundly.

Others who have helped to crystallize my views of science and the role of theory in it are L. J. Henderson, James B. Conant, W. B. Cannon, Vilfredo Pareto, and Max Weber. Conant's general views of the role of theory in science are especially important, notably his use of "reduction in the degree of empiricism" [cf. 5, chap. 1] as a fundamental criterion of scientific advance. Also his examples from the history of science which have shown (as in the case of Galileo and the problem of the limitations of the height to which a column of water could be raised by a suction pump) that knowledge even of *all* the critical facts is not sufficient to ensure a "right" or maximally fruitful theoretical explanation of an important phenomenon [cf. 6]. In Cannon's case it was particularly his conception of physiological equilibrium as the homeostasis of a boundary maintaining system which provided the important model [cf. 7].

Both as interpreter of Pareto and in his own right, Henderson was a most important influence with respect to the concept of system and its importance to science, and also to related concepts like equilibrium [cf. 12]. It was through the Henderson-Pareto influence that my conception of social system in a fully technical sense first crystallized. Schumpeter played a similar role with respect to the idea of system in economics. Weber was, in this area, a more diffuse influence, above all in showing the possibilities of strict scientific methodology for dealing with "humanistic" and historical-cultural materials. His ideas of *Verstehen* [cf. 36] helped very much to break the monopolistic claims to scientific standing by behaviorists of the extreme school, who would not grant that data concerning anything but bodily movements could be "objectively" studied.

My general orienting attitudes toward social science, then, have come to center about the problem of the nature and role of systematic theory in this field. Early biological interests, reinforced by later concern with problems of medical practice, gave me a strong conviction of the fundamental continuity between the organic world as studied in the biological sciences on the one hand, and the world of human social and culturally oriented behavior on the other. At the same time, I could not accept the kind of "reductionist" view which maintained that the "real" determinants of all human behavior were to be found in the structure and physiological processes of the organism as treated by early twentieth century biological science—with the implication that the concerns of sociology, economics, etc. were with purely epiphenomenal manifestations of these "real" factors. This is to say that I was deeply

involved in the "war of independence" of the social sciences vis-à-vis the biological. The basic difficulty has been resolved by attempting to place these sociocultural concerns in the context of an evolutionary view in which they represented levels of organization of the processes of life rooted in, but emergent from and to a degree independent of, those which have been the more conventional biological concerns.¹⁴

Closely linked with this set of attitudes have been those concerning the relation of theory to empirical observation and concerning the nature of the systems dealt with in this area. With Whitehead, Conant, and others I have had a strong conviction of the independent significance of theory; I have never been sympathetic to a view of the methodology of science which gave overwhelming emphasis to empiricism and induction and made legitimate theory no more than a set of statements of validated empirical fact, arrived at without benefit of theory. Opposed to this I have set for many years Henderson's well-known definition of fact as "a statement . . . *in terms of a conceptual scheme*" [11]. This is not in the least to derogate the importance of facts but rather to challenge the claim that knowledge of fact has a near-monopoly of scientific importance.

With respect to the nature of systems my essential "prejudice" has been against the common "elementarist" position. This has tended to hold that, if elementary units can be isolated and studied in sufficient detail, then the processes of complex systems built up of such units will become understandable without further ado. In the psychological field, perhaps the most prominent issue has been whether the stimulus-response unit or the conditioned reflex could be made the basis of a complete understanding of psychological systems, so that independent analysis of personality and its subsystems would become unnecessary. Similarly, as a sociologist, I have been sensitive to the common claim that only through understanding "the individual" independent of his social relationships could the understanding of social systems be approached because, after all, "society is composed of individuals." Here the essential point is that *organization* in the sense outlined earlier, must be treated as an independent factor in the functioning of systems, biological or behavioral, a factor which is not reducible to the properties of separately given units.

All these basic orienting attitudes have applied to the general field of analysis of human behavior, without special reference to its psychological aspects. They constitute, however, the framework within which I have approached psychological theory. Having, as a sociologist, been deeply engaged in the battle for the independence of *social science*

¹⁴The approach arrived at from this perspective seems to have converged notably with the development of biological theory itself within the last generation.

from biological science, I could not be attracted by a type of psychology which was simply a branch of human biology in the traditional (early twentieth century) sense, nor by one which dealt with psychology as the "science of behavior" of the individual without reference to the *independent* significance of social and cultural systems. My experience with such problems as that of motivation in occupational roles and in psychotherapy convinced me that psychological help was needed to attack many empirical sociological problems. To be helpful, however, it had to be a psychology which could *fit with* the analysis of social systems. For example, it had to be a psychology able to recognize that the difference between the behavior patterns institutionalized for the role of physician and the role of business man was more than simply a matter of the way different types of personalities happened to behave.

From this attempt to fit psychological theory into the requirements of social system and cultural theory and to give it a place *between* social systems and the organism I have derived the principal points of reference for defining the significance of psychological theory and for specifying the *kind* of theory which could acquire that significance. The conviction that such theory could form an integral part of the more general theory of action, which is the main guiding line of this essay, grew up only gradually and has not become fully crystallized until quite recently. That this should be so, however, seems to me to be a reasonable inference from the results of various previous attempts to codify the relations of sociology and the psychology of personality, and it seems also to fit well with the general conception of the nature of scientific theory which I have put forward.

It seems best, with this discussion, to pass immediately to the outline of the main theoretical system itself. Such general methodological issues as the problem of prediction, of the role of models, of quantification, and of the formal organization of theory can be more profitably discussed when the main outline of the scheme is before the reader.

Part II

THE GENERAL THEORY OF ACTION AND ITS APPLICATION TO PSYCHOLOGICAL SYSTEMS

Structure of the theoretical system. The structure of the theory of action as a system will be initially discussed in two parts: (1) the "frame of reference" or set of postulates involved, and (2) the principal properties of empirical systems and units in systems which are made use of in the theory, including parametric "givens." The classification of

variables as independent, dependent, and intervening will be discussed under (2).

1. The theory deals with systems composed of units (in the sense in which particles or cells are units); there is no system without at least two units. What for a given analytical purpose is treated as a unit of a system may, at the next more microscopic level of analysis, be treated as itself a system. When an entity is treated as a unit, its properties are always imputed to the unit as a whole and their "sources" internal to the unit are not identified; the properties are qualities or performances of the unit as such. When it is treated as a system, the attention is focused on the internal processes by which given qualities and performances of the system become understandable. Every system in turn is potentially a unit in some more macroscopic system.

Units of systems of action are both "actors" and "social objects" according to the point of reference. A unit is an actor when it is conceived as "orienting to" one or more other actors and performing or "overtly" acting in terms of its orientation; it is a social object when conceived as being oriented to and acted toward by one or more other actors. The same concrete unit may, of course, be both actor and object.

From the point of reference of any given actor, all objects which have meaning to it are "situation." The situation as differentiated into concretely discriminable entities is composed of objects (as distinguished from abstract conceptual entities like colors or shapes). Social objects are objects which are also actors, i.e., action systems of persons or collectivities, which therefore can be treated as *interacting* with the actor of reference, ego. Other objects are (a) physical objects, which have physical spatio-temporal existence and various types of meaning to actors, but are not treated as interacting with ego in the technical sense, and (b) cultural objects, namely, patterns of meaning which can be learned and otherwise oriented to (e.g., a proposition can be "believed" or "disbelieved"), but are not treated as interacting with ego (e.g., the *proposition* does not seek to "convince" ego, but only some other actor who believes it).

Any entity which constitutes a meaningful unit in a system of action or in its situation may be treated as an object, or if conceived as "acting" meaningfully, as an actor. Individual human beings of course are actors, though very generally for the purposes of the analysis of many social systems, it is the sector of the personality involved in a role, not the total personality, which is the significant unit. Not only individual personalities and subsystems of them but *collectivities* may be treated as actors. Similarly, in the other direction, units or subsystems of the personality may be treated as actors (e.g., the ego or superego) and also such organic subsystems as, e.g., Olds's cell assemblies [20, pp. 107ff.].

What from the viewpoint of any given actor-unit is situation is divided as follows:

1. The qualities and performances of other actor-units in the same system of reference.

2. Qualities and performances of units of cognate systems with which ego's membership system (or he himself in his membership capacity or role) interacts.

3. Objects belonging on a system level of lower order in action terms than the system of reference. Physical objects belong in this category, for as we use it, the concept is not an ontological one; it is *relative* to system-reference in the theory of action.

4. Objects belonging on a system level higher in the order of system-reference than that of reference.

Some collectivities as objects, and some cultural objects at least, belong in this category; 2, 3, and 4 are situation to the system chosen as a point of reference.

Actors are "oriented" to objects in their situation in so far as the object (or a category of objects) in its relations to him may be said to have acquired a *pattern of meaning* to the actor in question which is relatively stabilized and can therefore serve as a reference point for analysis of his action. Meaning is, in the most elementary terms, resolvable into two components: (a) "cathectic" meaning, as a goal object (or object to be avoided) or source of gratification (or deprivation), (b) "cognitive" meaning, as part of a relatively stable "definition of the situation." Instrumental or means objects have *primarily* (though not exclusively) cognitive meanings to the actor. Cathectic meaning answers the question of degrees and kinds of wanting or not wanting to stand in a given *relation* to the object; cognitive meaning the question of what the object *is* in a sense significant to action, but independent of ego's cathectic relation to it including what it can be "used for." A *value*, or an evaluative meaning, is an organized pattern of both cognitive and cathectic components which can be used to formulate a relatively stable general orientation of an actor or class of actors to an object or class of objects in the light of its relation to partially equivalent alternatives.

Interaction operates in the first instance through *communication*. Communication is a type of act involving the transmission of meanings *common* both to the agent and to the recipient object. On the part of the agent the meaning is "intended" (not necessarily consciously) and on the part of the recipient, is "understood" (again not necessarily consciously). All communication operates through signs or symbols, acts, or situational consequences of antecedent acts (e.g., artifacts), which can have intended meaning to the agent and can be "understood" by the recipient of the communication.

All communication, hence all interaction, implies "normative control" of action. Only by the observance of conventions or rules regarding the "proper" meanings of signs and symbols is effective communication possible. However arbitrary the sign may be, from an intrinsic point of view, having acquired a meaning it cannot be used arbitrarily (i.e., its meaning changed) in an interactive process without disrupting the process.

From the theoretical point of view, action in a completely nonsocial situation, where there is no interaction and no communication, is a special limiting case. It is logically derived from the more general case by suppressing certain ranges of possible variability involved in interaction, i.e., those involved in the responses of alter to the communications of ego, and vice versa. Only meanings originating in ego's own psychological system, and unaffected by feedback on the communicative levels, need be taken into account for this case.

In the most general terms, the frame of reference of action may be regarded as a schema for analyzing mechanisms which *control* behavior. Its focus is not in the first instance on the behavior processes themselves, e.g., muscular-skeletal movements, but on the determination of *when* and in what circumstances they will and will not take place, and in *what states* of the actor in relation to the situation, i.e., of the system. For this purpose, certain properties of behaving organisms must be treated as given data, e.g., their constitutional capacities for certain types of behavior; others, such as learned skills, can be treated as consequences of action which in turn condition further action. There are, however, many levels on which such data are relevant, and the theory is not ontologically tied to any one, but "plugs in" at any one of several. At the "lower" limits, however, it can be said that the basic data or parameters are the "performance capacities" of the organism (i.e., independent of learned content) and the factors or conditions of the nonsocial situation (which are neither artifacts nor signs). These of course include the potentialities of both for modification, the first through "learning," the second through "mastery."

2. Let us now turn to the characteristics of *systems* of action. A system is constituted by the interaction of two or more units, empirically determined at a given level in social or psychological terms and on the microscopic-macroscopic range. We conceive a system of action to be determined by (a) certain properties of its units and of its situation which are given independently of processes in the system, and (b) the processes of the system which in turn can be subdivided into (1) processes internal to the system and (2) processes of interchange over the boundaries of the system with its situation.

Let us start with the processes of the system itself. These (including

both subcategories (1) and (2)) may be described as constrained by four fundamental sets of exigencies or "functional problems" which, taken together, are the dimensions of the space in which action processes operate [cf. 24, chap. 3]. The four exigencies to which a system of action is subject are those of "goal attainment," "adaptation," "integration," and "pattern maintenance." These are dimensions of a space in the sense that a state of the system or of its units' relation to each other may be described, relative to satisfactory points of reference of course, as "farther along" or less far along on each of these dimensions; a change of state may be described in terms of increases or decreases in the values of each of these variables. These four dimensions are conceived to be orthogonal; their values are independently variable in the sense that change of state with respect to any one cannot be interpreted to have an automatically given relation to change of state in any of the others (except so far as this relation comes to be known and formulated as a law of the system). It is also true that maximization of all four, and probably of any two, is not possible in the same state of a given system.

As an essential point of reference for defining the four functional exigencies or dimensions of systems, we assume one law, or postulate, according to the way it is viewed. This, we call a law of *inertia*, on the analogy (or more than that) of the use of the term in classical mechanics. The law may be stated as the proposition that a process of action (as part of a system of action) will tend to continue with its direction and potency (see below) unchanged unless it is deflected or otherwise changed by the impingement of some other process (in the system or in its situation).

Very closely related to the concept of inertia is that of equilibrium. Indeed the latter may be regarded as a special case of the former, where a system, rather than one of its units, is taken as the point of reference. Equilibrium is the principle that a system will tend to remain in a given state (including stability in the operation of processes—it most emphatically does not imply a state where "nothing happens") unless and until it is disturbed by some influence from outside the system. Furthermore, if such a disturbance occurs, tendencies will be set up to bring about the state in which the system *would have been* had the disturbance not occurred (this formulation allows for the state of a system to be defined as conformity with a pattern or trend of orderly change, e.g., the growth curve of weight of a child).

The degree of stability of a state of equilibrium is of course an empirical question. Some equilibria are highly stable, i.e., the forces tending to maintain or restore the initial state are very strong; others may be highly unstable, i.e., a relatively slight disturbance may precipitate fundamental changes which make restoration of the original state altogether

impossible (e.g., detonation of nitroglycerin by an electric spark produces a violent change; in certain respects the nitroglycerin is in a state of unstable equilibrium). Still other equilibria fall between high stability and high instability.

We define the "tendency to seek goals" not in terms of any specific propensity of organism or personality or social system, but in terms of the concepts of inertia and equilibrium as applied to a system. From the concept of cathectic orientation it follows that an actor-unit or system will develop differential evaluations of different objects, and of different relations to the same object (or category of objects), in its situation in different circumstances. Once a pattern of such orientation has become established, there will be an *optimum* relation to a given object, an approximation to which we may call the "consummatory" or maximum-gratification state.¹⁵ If both the state of the system (or systems) of which the actor is a part and of the relevant situation could be assumed to remain stable, the principles of inertia and of equilibrium would tell us that the tendency with respect to any given object-relation would be for it to remain in the optimum consummatory state (this abstracts from the possibility of changing orientation patterns by learning).

For most empirical systems, this is a radically unrealistic assumption. States of the system (i.e., the relations between the unit of reference and other units) and states of the situation are continually changing. Such changes will bring about discrepancies between the actual (and over certain periods expected) states and the optimum consummatory state. From the concepts of inertia and of equilibrium, therefore, we derive the tendency to change the state of the system and its relation to the situation in the direction of a closer approximation to the consummatory state. *This is what is meant by the tendency or drive to attain goal states.* We treat it as a property of all systems of action, physiological, psychological, social, and cultural, resulting from the consequences of disturbance in the optimum relations between system and situational object.¹⁶ The same general theoretical reasoning applies to the other three functional problems of the system.

If we assumed as a limiting case a system of action in a situation consisting only of one undifferentiated significant object, it would be im-

¹⁵ Such a state may, in certain cases, be defined in terms of *rates* of inputs and outputs to the object; hence it is not a "static" state of relation.

¹⁶ In discussing goal attainment, it is particularly important to keep system references clear. It is a category of the *relation* between a given system and its situation. It is particularly dangerous to jump from the goal of a unit of a system to the situation of the system (rather than of the unit) since the relation of the system to its situation is never a *simple* function of the properties or state of *one* of its units in relation to the situation. Situation for the unit consists primarily in other units of the same system.

possible to discriminate between the problem of goal attainment and that of adaptation.¹⁷ But most systems of action function in situations differentiated into a plurality of significant objects which present different conditions of goal attainment. To some degree, courses of action oriented to one goal are incompatible with those oriented to another. Yet some courses of instrumental action, e.g., the acquisition of facilities, can serve the attainment of a plurality of goals, and the decision among the goals need not be made until a relatively late stage in the sequence. A type case is the earning of money in a modern society; activities devoted to the acquisition of money resources need not involve firm and specific advance commitments as to the exact disposal of the proceeds for final consumption.

As distinguished from goal attainment, adaptation is the degree to which a system has developed a *generalized* capacity to meet the exigencies imposed by an unstable and varying situation, without reference to any one particular goal interest. When the system of reference is the total society as a social system, this is its economic level of income or wealth. Since goal attainment and adaptation are independent,¹⁸ on a level where plural goals are involved, it is possible, and to some degree inevitable, for them to conflict. For the *generalization* of facilities relevant to a wide range of goal interests may be bought at the cost of loss of particularized suitability for any given specific goal. Reconciling these two bases of interest is one primary focus of the integrative problem in systems of action.

The two functional exigencies of action systems so far discussed, goal attainment and adaptation, concern relations between the system and situations external to it. The other two concern problems internal to the system, i.e., conditions of its stability which, in the analytical sense, are independent of situational conditions. We have called these two internal exigencies, pattern maintenance and integration respectively.

As we define it, a system of action is a system of *relations* between living organisms and objects in the environment. It is only a system of action in the technical sense so far as the relations are organized through *learned* patterns of orientation.¹⁹ Once thoroughly learned (in a personality sense, internalized) such patterns become the primary focus of the

¹⁷ This case is presumably approximated in the "mother-child identity" of infancy [cf. 25, Chap. 2].

¹⁸ But, of course, also interdependent. We assume them to be orthogonal dimensions.

¹⁹ The case where the environmental event is only a "trigger release" for a constitutionally built-in pattern of behavior is, in action terms, a limiting case [cf. 25, Chap. 4]. The crucial point is the significance of the *consequences* of response as a basis of learning, and hence modification of subsequent behavior. If the consequences have no effect on subsequent behavior, the phenomena are of no theoretical interest from our point of view.

organization of the system as a system of action. It is in this sense of organization on a pattern, i.e., "cultural," level that we have introduced the concept of value.

There are two primary aspects of the functional problem of pattern maintenance. The pattern system (which on a human level is certainly "cultural" whatever term may be used for subhuman analogues) which regulates any particular system of action is always part of a larger system of patterns, related to other elements in terms of "meaning-congruence," consistency, etc. In the limiting case of a total society, the total meaning system may be independent of any others, but it is still subject to the exigencies of its "making sense" to the actors in the system.²⁰ In any case, some part of the relevant pattern may be threatened by actual or apparent incompatibility with other parts of a larger system.²¹

The type of potential disturbance with which we are at present concerned may arise from outside the system in question through posing questions of compatibility in inescapable form. It may also arise from within, through weakening the specific mechanisms ("defenses") through which "intrinsically" incompatible elements are enabled to co-exist in the same system. Indeed, in a slightly different perspective, what we refer to here as the function of pattern maintenance might be called the "strain toward consistency" in the pattern system. This is the active version which is parallel to "goal-striving."

The second primary aspect of the functional problem concerns not the consistency of the pattern system itself, but the level of motivational commitment to implementation of the pattern or of some subpattern component of it. It is for example possible to "believe" a proposition in the sense of assenting to its truth when involuntarily confronted with a situation where it is impossible to evade taking some position; it is quite another thing to believe it as a focal center of primary orientations.

²⁰ This would be strictly true only in a limiting case. Historical tradition and the presence of other societies "frame" the meaning-problem of a particular society in reference terms wider than the psychological needs of its members.

²¹ We assume here the psychological validity of the "principle of contradiction," namely, that it is not possible, without strain and the operation of specific mechanisms, to hold two or more mutually contradictory beliefs at the same time, e.g., that Boston is northeast of New York *and* that Boston is southwest of New York. This constitutes a fundamental reference point for psychological (and sociological) as well as logical analysis. Further, we hold that in the cathectic-expressive field there is a similar principle of congruence according to which it is not possible without strain and the operation of specific mechanisms to be committed or attached to two or more mutually incompatible cathectic commitments at the same time. For example relatively total "love" and "hatred" of the same person is only possible if mechanisms of defense prevent the full juxtaposition of these attitudes. This is why the cruder ambivalences which are not reinforced by other strategic factors cannot survive good psychotherapy.

This problem of motivational commitment is what we call the "tension-management" aspect of the pattern-maintenance function. (Tension is here used as a general psychological term in motivation theory for a state of unstable equilibrium.) So far as there is tension, there is a "disposition" toward change of state. This may potentially take a direction which will lead away from conformity with the cultural pattern in question.

The generalized significance of this problem derives from the interpenetration of psychological, social, and cultural systems. Broadly, we may say no internalized cultural pattern system is ever entirely idiosyncratic to the particular personality. Short of the total personality, any partial psychological system must reckon with the repercussions of the rest of the motivational system on this particular partial system. Conversely, no human cultural system ever "operates" without being borne by a multipersonal social system and the several participating personalities. The system exigencies of cultural consistency and the operative actions of both social and psychological systems always impinge on the tension problem. In other words, cases of relative stability of pattern maintenance must always be accounted for by specific mechanisms which "forestall" the continually arising threats to this stability. It is *never* safe to assume that a cultural pattern is "naturally appropriate" and will be maintained in a system of action just because it "has to be that way."²²

The two primary aspects of the pattern-maintenance problem belong together because their outcomes flow into the same channel, namely, *either* reinforcement of the conformity of action with the values and expectations defined in the cultural pattern system, *or* reinforcement of tendencies to deviance from these expectations. The pattern system is the fundamental point of reference for analyzing the problems of stability and instability of systems of action.²³

The pattern-maintenance function refers to the state of the unit, and the conditions of its stable equilibrium which are relatively independent of its position as a unit in this particular system of action. Essentially we may say that the foci of these changes are (a) the "culture" relatively independent of specific action-system involvements and (b) the "personality," in the sense of the impingement of the motivational system as a

²² In terms of the hierarchy of controls discussed above the consistency aspect of the pattern-maintenance problem looks "upward" to the central source of control, the "tension" aspect looks "downward" to the units subject to control.

²³ We may suggest that stability-instability is the best pair of terms to use for the *system* level of reference. They refer to the concept of equilibrium, and through it, to inertia. On the other hand, the terms conformity-deviance are best used with reference to the *unit* level. A unit conforms to the norms of the system or does not; but a system is stable or unstable.

whole on the particular action system in question. This is indeed the center of the problem area which has been called "culture and personality."

We still have a fourth functional problem of the system of action which, from the point of view we have discussed, must be treated as primarily "internal" to the system but at the same time must be distinguished from the pattern-maintenance function. This is the function we have called the integrative. From the point of view of the unit in a system, a culture pattern has in the first instance cognitive significance; it is *content*, in the sense of information, to be learned in the double sense of comprehension and motivational commitment. After being learned it becomes a property of the unit itself. But the units of a system are also objects to each other in a predominantly cathectic sense. By system integration we mean the mutual cathectic adjustment of these units to each other in the perspective of the internal harmony or, as is often said for social systems, solidarity or cohesion of the system. Every system then has a level of integration which is a function of the "object-relations" of its units to each other, of the adjustment of their mutual cathexes through motivational mechanisms. If the units are persons or their roles, this takes place through what have been called the mechanisms of adjustment [cf. 25]. If the system is intrapersonal, it is through the mechanisms of defense. Mutual antagonism or aggression (intrapersonally, "conflict") is of course *prima facie* a threat to integration. System integration and pattern maintenance are dynamically interdependent, but much analytical and codifying work makes it clear that it is essential to discriminate them as independent variables.

These four are the fundamental variables of our system. Before discussing some of the parametric²⁴ categories which are essential to give the system empirical determinateness (at different levels), one fundamental relation among the four variables must be mentioned. It has already been noted that two of them refer to aspects of the state of the system in relation to the situation external to it, whereas the other two refer to aspects of the internal organization of the system. Let us elucidate some implications of this differentiation.

A system of action is, we have held, a "boundary-maintaining" system. There must be, then, in the relevant respects, a closer order of integration or organization within the system than between the system and other systems. We take this to imply that units operate with a greater scope of freedom or autonomy when they are functioning in intersystem

²⁴ By the terms parameter and parametric I refer to propositions which are *empirically* essential to determinate analysis by use of the theoretical categories of the system but not values of these theoretical variables as such. A parameter states *given data* for an empirical problem. The frame of reference of a parametric statement must, of course, be congruent with that of the theoretical system.

relations than when they function in intrasystem relations. In so far as the hierarchy of systems of action is a hierarchy of levels of control of behavior, there is a difference of level between goal attainment and adaptive references on the one hand and pattern maintenance and integrative references on the other. The former pair look "downward" in the scale toward the remoter situational factors which are relatively independent of the organizational system of reference. The latter pair look "upward" toward the more central foci of the total system of control.²⁵

This distinction is not ontological but entirely relative to the place of the given system in a larger reference framework. Such a larger reference framework is inherent in the general conceptual scheme of action; the relative treatment given on the one hand to goal attainment and adaptive problems, and on the other to integrative and pattern-maintenance problems, is a function of the place of a given system in this reference scheme.

At this point, we should also comment on the relation of this four-dimensional scheme to that of "pattern variables" which has figured prominently in previous publications [cf. 23, part 2, chap. 1; 24, chaps. 3, 5]. The essential point is that the four dimensions incorporate the core of the pattern-variable scheme. The difference is that the latter makes explicit the basic distinction between the "attitudinal" and the "object-categorization" aspects of the general action frame of reference. That is to say the attitudinal and object categorization subpairs of pattern variables can be "matched" so that functional specificity and universalism become the two relevant aspects of the adaptive dimension, affectivity and performance, those of the goal-attainment dimension, functional diffuseness and particularism of the integrative dimension, and affective neutrality and quality of the pattern-maintenance dimension. Of the original five pattern variables this omits self vs. collectivity orientation. This last, it has become clear, is a category referring to the relations *between* systems of action, not to the constitution of any one particular system.

There has been criticism of our formulation of the pattern-variable as dichotomies: affectivity vs. affective neutrality, universalism vs. particularism. It now seems to be clear that they are dichotomous because of the location of their reference to *the integrative problem* within systems of action. Interests in goal attainment stand in an inherent poten-

²⁵ When generalized, this distinction can serve as a principle for relating systems to each other in a hierarchical series. Put otherwise, a hierarchy of control, such as we have discussed, may be conceived as a series of alternating layers of adaptive goal-seeking components and integrative pattern-maintaining components. According to which pair has the functional priority we can then distinguish two types of system—one more situation- and performance-oriented, the other more internally and "expressively" oriented. This distinction will be used later in connection with psychological systems.

tial conflict with those in pattern maintenance because the former can always easily be dominated by questions of "expediency," the practical (i.e., situational) availability of objects of consummatory gratification. Similarly, the opposition between adaptation and integration derives from the dilemma between the unit's orientation to the external "fence-mending" functions and its integrative adjustments to other units in the system. Because adaptation is the *generalized* focus of situational relations, it is the focus of conflict with integrative interests.

Our most central methodological distinction is that between fundamental theoretical variables of our system and parametric categories. The latter can be divided into two classes, those characterizing the units of the system, and those characterizing the situation in which the system functions. In common with other fields of science we distinguish, with respect to both, relatively stable properties of objects, and time relations [cf. 24, chap. 5].²⁶

Time relations present what, for us, is the simpler problem. All the empirical sciences take it for inexorable fact that certain events have occurred at given times and in given time sequence. *Given* certain antecedent time determinations other time determinations can be deduced by theoretical reasoning; this is what we mean by prediction in its temporal aspect. But time is *never* a manipulable variable; time is a frame of reference within which one can state and interpret the assumptions about and the consequences of the operations of manipulable variables. When we say that we decide "when" something will be done, we do not manipulate time but the variables which have consequences *in* time. Like all parametric features of systems, temporal relations have two orders of scientific significance: (a) time is one fundamental aspect of the givenness of the empirical world which provides the empirical base from which any deduction or prediction can be carried out; (b) in the sense of *when* a given future event will occur relative to others, time is a fundamental aspect of the empirical manifold to which any chain of scientific reasoning will lead. If time (possibly period rather than instant is the relevant unit) cannot be specified, empirical determination is incomplete.

For purposes of the theory of action, the properties of objects, both of units of systems and of situational objects other than time order, can be reduced to two classes: their value orientations, and their "potency." Value orientation is *location* in a system of reference formulated in terms of the four dimensions we have discussed. This aspect will be further discussed when we take up the internal differentiation of systems of action. Here, let it be noted only that location can be viewed "statically," in terms of place in a structure, or "dynamically" in terms of processes of change of location; or the two can be combined in the concept of "orbit" [cf. 24,

²⁶ For the view that time relations constitute, for physics, a set of parameters, not of fundamental variables, cf. [3].

chap. 5]. Theoretically these are different derivatives from the same theoretical analysis, and the distinctions do not raise any special problems.

Potency is a new term we have introduced in this essay because we have not heretofore had a single term to cover what we mean for physiological, psychological, social, and cultural systems. By potency we mean relative degree of importance as between units in affecting the outcome of processes in changing states of the system. It is the analogue of mass in classical mechanics. For psychological systems the best formulation available to us seems to be that of "motive force" as used by Olds [cf. 20, pp. 110ff.]. For social systems, with some qualifications, prestige seems to be the best available term. Looked at in performance terms, potency thus is relative *capacity* to influence the outcome of a process. The rank order of units with respect to this capacity is, so far as it is legitimized by values, the *stratification* of the system. It is a function of integration with the value system, internalization or institutionalization, but not only of this.

For purposes of a given analytical procedure, both values and potency, like temporal relations, are given either as among the data of the problem or as empirical resultants of the process being analyzed. This holds so far as strict analytical procedure in terms of a given system reference is concerned. Intrinsicly, of course, all these data are subject to change; only for specific methodological purposes can they be treated as given.

Here the distinction between the properties of the units and the properties of situational objects becomes essential. A change in the major properties of units and/or of their patterns of temporal relationship is to be regarded as a parametric change in the state of the system, not as merely an "equilibrating" process. In psychology, the distinction is that between learning processes and performance processes, in social system terms, between "social change" and "normal functioning."

To analyze processes of learning and of social change theoretically, it is essential to take account of *multiple* system references. The unit cannot be treated as the object of an explanatory problem in other than "positional" terms unless it is itself treated as a system; hence its relations to other units are not intrasystem relations but these others are treated as its *situation*. This distinction between "positional" change (including both direction and rate) and parametric change is fundamental to the theory of action, and we believe to all other comparable theoretical schemes.

We believe that classification of variables as independent, dependent, and intervening cuts directly across the classification built on the distinction between fundamental system variables and parametric categories. The independent-dependent-intervening variable scheme refers to the logical operations involved in an empirical problem-solving sequence.

The system-variable-parameter scheme refers to the logical structure of a theoretical system, which is never relevant only to one problem orientation, or totally involved in the same way in different ones.

In a given case, the values of one or more of what we have called the fundamental system variables or of significant parametric categories may be those which serve as systematic or empirical independent variables. Then the values of one or more other fundamental system variables or of one or more other parametric categories may be the corresponding dependent variables. The intervening variables would this time be the values again of one or more parametric categories which were not directly observed but inferred from the data on the values of independent and dependent variables. This statement implies that the laws connecting the values of the system variables are known. So far as this is not the case, one or more of their values might serve as an intervening variable.

Although there seems to be no very specific rule, it appears that the most general case of system analysis is what we would call input-output analysis. In the present methodological terms, this would involve treatment of a significant situational parameter as the independent variable and usually one or more other situational parameters as the dependent variables. A case would be the introduction of deprivational changes in the situation which would reduce the input of goal gratification into the system. The analytical procedure would trace the repercussions of this input change through the operations of the fundamental system variables and thereby describe a new state of equilibrium of the system with altered outputs from those obtaining in the initial state. Both the output values, which would usually be empirically ascertained through the states of the situational parameters, and the values of the system variables would change, and any of these could be the empirically determined dependent variables. (See under Construction of function forms below.)

Before we can deal with psychological systems as such, we should discuss two further general points about systems of action. The first of these concerns the ways in which systems are differentiated and integrated relative to their patterns of value orientation; the second concerns the implications of the conception of boundary-maintaining equilibrium for the problem of "normality" and "pathology."

As we have presented it, the theory provides three foci of the internal differentiation of a system of action: (1) The units may and generally will be differentiated in a rank order of relative potency. (2) They may be differentiated in terms of relative position (treated statically or as an "orbit," a range of successively occupied positions) in action space, i.e., of functional significance in the system. (3) They may be differentiated with respect to rates of input-absorption and output-production in their relations to other units.

The differentiation of the system may follow both temporal and, in the action sense, spatial patterns. The first we speak of as the differentiation of *phases* of system process, the second as differentiation of the *structure* of the system. The two are different aspects of the same basic phenomena and are capable of being analyzed in terms of the same frame of reference [cf. 24, chaps. 4, 5]. There is, however, a sense in which phase differentiation is a more "elementary" phenomenon than structural differentiation.

The fundamental basis of the phenomenon of phase differentiation lies in the fact that, as discussed above, the variability of its situation does not permit a system to remain stably in a consummatory state. Adaptive-instrumental modifications of the system, and through these of the situation, are necessary to optimize the possibilities of gratification (goal attainment), and these activities have internal repercussions in the integrative and pattern-maintenance aspects of the system. The primary basis of phase differentiation, then, lies in the limitations on the perpetuation of consummatory states and on the system-situation conditions which lead to repetitive return of approximation to such states. Within limits, phase differentiation is possible without structural differentiation; i.e., the system can be treated as a single unit.

We know of no class of empirical systems for which it seems useful to treat the system as one of plural units which are absolutely undifferentiated except for the phases of the units; such a system is theoretically conceivable though probably its equilibrium would be highly unstable. The *primary* basis of structural differentiation is functional, i.e., in terms of the primary "contribution" of the unit to the functioning of the system. This primary contribution is defined as the output at the goal-attainment boundary of the unit in question. Thus a unit differentiated from others (i.e., specialized) in terms of adaptive function for the system will contribute not directly to the system's goal attainment but to the adaptive level which facilitates attainment of an indefinite number of specific system goals.²⁷

The goal attainment of any system (and in discussing differentiation we must consider the unit as a subsystem) is at the same time the production of an output to *its* situation (the rest of the system) and the source of a category of inputs to itself, in this case "gratification" or reward in some sense. Units thus will tend to be specialized with respect both to their types of output and to their types of input, e.g., reward or gratification source.

But, since on the requisite levels units themselves are systems, they will tend to have not only distinctive goal outputs and rewards (as com-

²⁷ For an analysis of the boundary interchanges between subsystems of a society see [27, Chap. 2].

pared with other units) but also distinctive adaptive patterns and functions, integrative patterns and functions, and distinctive subvalue systems and pattern-maintenance functions. Not only must the goals of units be integrated in the system, but all their other functions must also be integrated.

It is clear, too, that the structural differentiation of units must be coordinated with the phase differentiation of the system. This occurs as different units have their goal-attainment and other phases at different points in the phase cycle of the system as a whole. The consummatory phase in the phase cycle of a unit comes at the phase in the system cycle where its primary function in the system has primacy for the time being. Thus an adaptively specialized unit will come to its consummatory phase during the adaptive (instrumental) phase of the system cycle. The consummatory phases of the other units then fit with other phases of the adaptive unit, etc.

The concrete structure of systems of action cannot be derived from this functional paradigm alone. For each unit is subjected to exigencies other than those defined by its primary goal interest. These exigencies, as traced through hierarchies of system-subsystem relationships, will "deflect" the structural patterning of the system in certain respects from the "pure type" of a functionally differentiated system. We hypothesize that of the three functional needs other than the goal interest in general the value pattern of the unit will be least deflected by varying exigencies (if the unit arises by differentiation; where, as in social systems is often the case, it has recently "joined" the system the deflection may of course be considerable). The second order of deflecting exigency will be the integrative, and the most prominent the adaptive. Hence, next to major alteration in a unit's opportunities for gratification, its adaptive position is the most prolific source of change in its structure.

One further point: we have emphasized that for the strictest purposes of equilibrium analysis the properties of the unit of a system of action must be assumed to be given. This means that what we are calling the structure of the system is given. Structural change then must be treated as raising a different order of problem from that of analyzing equilibrating processes in a system with given structure and situation. In one major aspect, the difference lies in the fact that in the former problem at least two levels of system relation must be taken into account. It is no longer possible to treat the unit only as possessing stably given properties and not as itself a system. Both the system in which the unit is a unit, and the unit as itself a system must be treated "dynamically." For this reason, analysis of structural change in systems of action presents a more difficult theoretical problem than does analysis of equilibrating processes of a single system of reference.

Finally, a word about the problem of normality and pathology. We have stressed that there is *always* a normative aspect in the analysis of process in systems of action (we believe this to be the case with all physiological processes as well). This was brought out most clearly in the case of the concept of communication; if the conventions of the sign-meaning system are violated, communication is disturbed. We connect this normative reference with the property of boundary maintenance which we impute to systems of action. Boundary maintenance means maintenance of a distinctive intrasystem pattern which is not assimilated to the patterning of the extrasystem situation. In this theoretical setting, the question of "how well" or "successfully" the system is maintaining its pattern is unavoidable on any comprehensive level of theoretical analysis, though for special purposes it can be avoided. Furthermore, the question cannot be localized in any specific aspect of system functioning; it applies to the system as a whole in relation to the situation as a whole. In a *relative* sense, the functioning of systems must be evaluated: they are more or less well adapted to their situations, more or less well integrated, etc. A conceptual scheme which makes values a central category cannot evade this consequence, and should not attempt to. But evaluation of the functioning of a particular system in a particular situation is a very different thing from a judgment of the value of that system and its results in some wider frame of reference. Our theory makes no particular assumptions on the latter level.

It may prove useful to introduce here a diagrammatic representation of a system of action. [Adapted from 24, p. 182.]

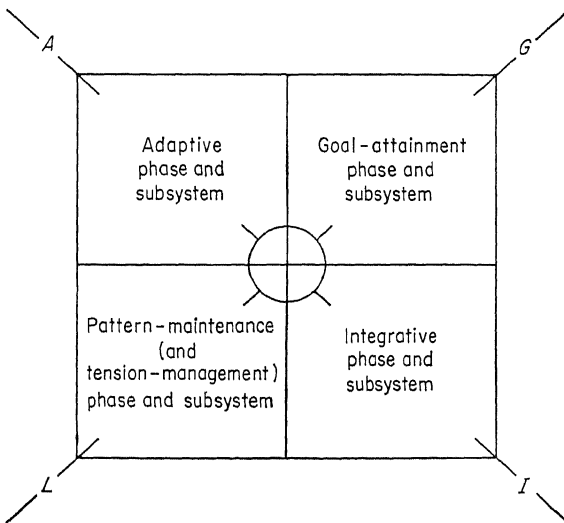


DIAGRAM 1

The letters represent the four fundamental functional system problems or dimensions of action space as follows: *A* adaptation, *G* goal attainment, *I* integration, *L* latent pattern maintenance. Representation of these four on a two-dimensional plane is simply a matter of convenience. The diagram *does not* represent the cross tabulation of *two* variables, but *each* of the four sectors of the diagram represents an *independent* dimension of variation. The clockwise order of juxtaposition is that involved in the phase movements of ordinary system process (in psychological terms, performance processes); the counterclockwise order is that involved in the phase movements of processes of structural change in systems (psychologically speaking, learning). Hence the order of juxtaposition is not arbitrary but has theoretical meaning.

We have pointed out that these four dimensions also constitute the primary frame of reference for processes of differentiation of systems of action. Hence the same diagram may be used to represent the primary functional subsystems of a larger system of action and certain of their relations to each other. Each of the subsystems will then be conceived as engaged in exchanges of inputs and outputs with each of the others. Seen in these terms the diagram takes the following form:

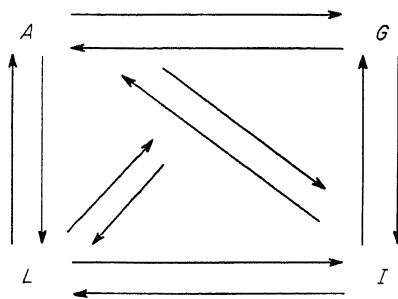


DIAGRAM 2

We will attempt to explain below the rationale of the different types of input and output at each boundary of each of the functional subsystems.

Psychological systems. Having outlined the main structure of the theory of action, the next task is to show how it can be adapted to psychological subject matter.²⁸ The reader should recall that in the form in which we and our associates worked it out, the general theory of action was not originally applied to psychological systems as such but to social systems and then to certain of the latter's points of articulation with cultural and with psychological systems. The application to psycho-

²⁸ On one very important level this has already been done by Olds [20, Chap. 4]. His treatment should be compared with the following throughout.

logical systems thus represents an extension through codification procedures. It does not purport to discover or state *new* psychological knowledge, but to show that certain main lines of known psychological analysis can be stated and systematized in terms of the theory of action.

It is also important to keep in mind Olds's caution concerning the level on which this attempt is made. It does not select a single fundamental empirical unit of all action systems and attempt to show how different levels of system can be analyzed with it. Rather, it selects an *abstract* unit, located in a particular type of space and having other general properties, and shows that a variety of empirically *different* units, e.g., Olds's "concepts," need-dispositions, role expectations, collectivities, etc., can all be defined as belonging to this class of unit in an action system. Hence the propositions general to systems of action can be applied to systems involving this type of action unit.

Let us now discuss psychological systems in general terms and then attempt to spell out the general statement with reference to some selected cases. We have defined a psychological system with reference to *behavior*, i.e., a set of *relations* between a living organism and objects in its environment. A psychological system is a system of behavior pertaining to a *particular* organism. The total system of behavior of one organism is its personality, but a personality, even of a subhuman animal, is divided into a complex set of subsystems of different types. The following discussion applies both to personalities and to their subsystems.

Thus psychological systems stand *between* the organism and the object-system presented by its environment. They are the systems generated by the *relations* between these two entities. Finally it must be noted again that a particularly crucial class of objects for behaving organisms is the behavior, i.e., personalities, of *other* organisms, particularly though not exclusively of the same species.

Like any system, certainly any system of action, a psychological system must be analyzed in terms of two sets of processes: "boundary processes," which involve the relations between the system and its environment, and internal processes, which involve the units of the system in their relations to each other. Empirically these two sets of processes are not always completely separated, but the conception of boundary maintenance, which is fundamental to our scheme, makes their discrimination a primary theoretical distinction.

Not all the boundary processes in psychological systems are directly observable. This is because one essential set of boundary relations will be those between the psychological system and the organism; a certain proportion of these will not be directly observable as behavior, but must be inferred as resultants of intervening variables. The same is true of the internal processes of the psychological system. The directly observable

processes then include some, but not all, the boundary relations between psychological system and organism, and all the boundary relations to objects external to the organism-personality system.²⁹ Further, it must be noted that where the psychological system in question is not a total personality but a subsystem of a personality, *its* important boundary relations will not be the same as those of the personality but will include relations to other units of the personality system. Hence it is dangerous to presume that the same classes of processes will be directly observable for all classes of psychological systems.

The first substantive question we have to raise is, what are the units of psychological systems as systems of action? In a general sense they are components in the *organization* of behavior processes which have come into existence through learning. What is learned is the meanings of objects in the situation of the organism-personality unit, and of parts and processes of the person's own body, treated by him as objects. The units embody what we have called elsewhere cathectic and cognitive components organized in relation to each other in evaluative terms; the units thus consist in organized modes of *orientation* to the objects in the situation of action.

Looked at in a slightly different way, the units of a psychological system constitute, in one respect, what is ordinarily called the *needs* of the system, in another its *dispositions* to act, i.e., to control the capacities of the organism and of external objects in the interest of goal-directed behavior. Hence we have adopted the composite term need-disposition to refer to these units [cf. 22, part 2]. It is essential to note that in our view *all* of the units of a psychological system have both cathectic and cognitive components; all of them are organized with reference to values; all of them have both need aspects and dispositional aspects. They come to be differentiated from each other in a variety of respects, which will effect the relative primacies of these aspects for the different units of the same system, but this does not invalidate the central proposition that all aspects are present for every unit of a psychological system.

Depending on the degree of differentiation assumed with reference to organic life in a particular frame of reference, the psychological system may or may not be treated as "part" of the organism. If the less differentiated frame of reference which discriminates only "organism" and "culture (I)" is used, then it clearly is part of the organism. If, on the other hand, one uses the more highly differentiated frame of reference which discriminates four system types in the action field, the psychological has to be treated as an independent system level. It is on the latter level that the present discussion will be conducted.

²⁹ This, of course, disregards "conventional" restrictions in such observations, e.g., where rights of privacy as between husband and wife are involved.

In this case, a psychological system will have three principal types of boundary relationships with other action systems (other, that is, than other subsystems of the same personality). These will be relations with the organism, with the social system (i.e., social objects in the situation of the personality), and with the cultural system. In addition there will be relations with systems outside the action frame of reference, notably physical (including chemical) systems both "internal" to the organism (in its "vegetative" aspect) and in the environment. Of these last no attempt will be made here to give an account, except to note that action systems in the organic world in general operate to *control* (and adapt themselves to) the functioning of physical systems.

Within our range the relation between a psychological system and the other three types of action system which constitute its environment or situation may be analyzed in terms of two fundamental concepts, namely input-output interchange, and interpenetration. Let us discuss each of these for each of the three main boundary types.

Psychological System and Organism

It is not possible here to give a complete account of the very complicated interchanges between personality and organism; only a few highlights can be touched upon. First, on general theoretical grounds we suggest that the inputs the personality (psychological) system receives from the organism are in the first instance to be regarded as *facilities* for its functioning; this is not exclusively the case but the facility aspect has primacy.³⁰

In what do these facilities consist? In accord with the paradigm which we use to classify each aspect of the operation of a system in terms of four functional categories, we can propose four categories of inputs. The first, and in a sense most basic, is what is usually referred to as motivational energy in the *organic* sense (which should not be identified with motivation in a psychological sense). In other words the organism is the source of the energy which underlies all processes of action. This is the phenomenon underlying "tension" and should, in our opinion, be treated in terms of the concept of inertia, i.e., as a flow which tends to remain constant unless increased or decreased by special factors impinging on it.

³⁰ The general theoretical grounds derive from the fact that, as noted earlier, in the general system of action the organism has primarily adaptive functions, whereas the personality system has primarily goal-attaining functions. Then, on grounds which cannot be developed here [cf. 27, Chap. 2 for the fullest exposition yet attempted], the interchange between an *A* subsystem and a *G* subsystem is primarily mutual interchange of facilities whereas that between a *G* subsystem and an *I* subsystem is primarily a mutual interchange of rewards.

The second and third types of facility provided by the organism are also familiar in psychological thinking. The second is perceptual (or cognitive) capacity, i.e., the capacity to assimilate and organize "information" coming from the environment of the psychological system, not only from "external" objects but also from the organism through proprioceptive processes. The third is "performance" or "response" capacity, the capacity to utilize the structures of the organism, notably the skeletal-muscular structures (and through them external means-objects) for physical manipulations of the environment.

Fourth, there are the facilities which aid in integrating these other facilities with each other and with the needs of the psychological system. Though much in this area is obscure, it seems correct to speak of pleasure as the primary integrative facility. Pleasure is particularly closely associated with the capacity to learn, constituting a proprioceptive reward mechanism which can, by learning processes, be associated with the attainment of the goals of the *psychological* system.

What outputs of the psychological system to the organism correspond to these classes of inputs? Two guiding lines may be suggested for identifying them. First, they should appear as mechanisms of *control* of the organic processes most closely involved in behavior; second, they should be identifiable as facilities from the point of view of the behavioral organism.

In one context the most fundamental of these psychological outputs seems to be what Olds calls "motive force." [Cf. 20, p. 110, *et passim*.] This may be thought of as that part of the energy received by the psychological system from the organism which is "fed back" to motivate instrumental processes and which can increase the performance potential of the organism when it is controlled by the psychological system. A second type of psychological output to the organism can be called the "directional" component. This means that in relatively specific situations the facilities of the organism come into direct control of specific motivational structures of the psychological system. It is the process of immediate determination of the directions of "interest" in the perception process, of the directions of goal-seeking in the performance process, and forms of "acting out" for which pleasure can be a direct reward.

The third component of output may be said to be the "expectation" component. This determines the "attitudinal set" of the organism with reference to its integration with the psychological control system. Basically it is the "expectation" that organic interests will be well served by "going along" with the psychological system, i.e., satisfying psychological needs. Disturbance in this relationship seems to be involved in the deeper types of psychosomatic problem.

Underlying all of these is a pervasive problem of the "organic secu-

ity” which is dependent on the stability of the whole relationship between organic and psychological systems. It is presumably on this security above all that the stability of the organic energy flow to the psychological system is dependent.

Sketchy as this account of the input-output relations between psychological system and organism has been, perhaps it has been carried far enough to suggest that a variety of problems which have played an important part in the psychological literature can be approached in terms of the theory of action.³¹

Before taking up the concept of interpenetration in more general terms, a few preliminary remarks are necessary. At many points in constructing the theory of action it has become evident that analytical distinctions between *types of system* do not correspond to concrete systems. A business firm, for example, may be spoken of as a collectivity with economic primacy, as both “participating in” the economy and more determined by its role in the economy than in any other analytically defined subsystem of the society. But “the economy” cannot be defined as the aggregate of business firms and their relations, if firms are interpreted to be concrete collectivities. For these units have political and other “aspects” and many collectivities other than firms, e.g., households, have economic aspects.

Where it is necessary to speak of two or more analytically distinguishable relational systems as *both* constituting partial determinants of process in a concrete empirical system, we speak of the systems as *interpenetrating*. The same concrete phenomena must be interpreted as “participating in” both analytical systems. Clearly, in the conception set forth here, behavioral organism and psychological system are interpenetrating in above sense although treated as analytically distinct systems. On the more concrete level, behavior is always behavior of the (concrete) organism. If the organism as an anatomical-physiological system be reified (which, as we learn from Whitehead, is illegitimate) then, of course, a psychological system as a distinct system cannot exist; the only recourse is to reductionism. But from the point of view adopted here this is not a necessary inference.

In the present context interpenetration implies that there will be

³¹ It is of interest in the general context of this essay to note that the analytical model for this relation has been derived from the boundary of the economy as a social system, the boundary which involves the input of capital as a generalized facility for economic production, and which Smelser and I have interpreted to be a boundary vis-à-vis what we have called the “polity,” which is not to be confused with government. [Cf. 27, Chap. 2, pp. 56-59, 70-72.] Though far less fully developed in the psychological case, the correspondences appear sufficiently striking so that more than a mere analogy in the derogatory sense of the term seems to exist.

identifiable physiological mechanisms of all processes operant in psychological and psycho-physiological systems. But *processes* with respect to these mechanisms will be conceived as analyzable in terms of the *interaction* of the two system levels we have distinguished, not in terms of either one of them taken alone. The cell assembly which Olds [20, p. 107ff.], following Hebb, postulates is a model of such a mechanism.

We believe that the stimulus-response-stimulus paradigm of the most elementary psychological processes refers to a system in which the interpenetration of psychological and organic systems is treated as the salient feature. It is significant that the cases which have occupied the greatest attention have been those of animal behavior in highly restricted, short-term conditions, with a specifically set goal and a severely limited range of instrumental possibilities. These conditions altogether eliminate the higher-order level of social object-relations, and the more complex patterns of culture.

The S-R-S paradigm deals with the behavior of the organism on a level where its mechanisms of control are relatively closely bound to specificities of skeletal-muscular movements, so that the categories of stimulus and response can be given quite specific physical perception and physical movement meanings. In other words, it is truly an elementary action system in which some components of more developed ones are rudimentary at best, others are not differentiated from each other to a readily analyzable degree at all.

We would expect that the closeness of fit of organic and psychological processes would be most marked at this level. But there is no reason to believe that the interpenetration which is so conspicuous here ceases to exist at higher levels of differentiation and organization in systems of action. What happens is rather, we suggest, that the interpenetration comes to utilize higher-order mechanisms of the physiological control of behavior which are much less readily identifiable directly as mechanisms of "overt" behavior. It seems likely that these are most centrally located in the higher centers of the central nervous system and involve "field" phenomena of dynamic equilibria rather than gross physiological movements.

The phenomenon of interpenetration, which we wish to treat not only in the present context but more generally, is closely related to that of *internalization*. In a sense parallel to that we will employ later in speaking of objects internalized in the personality, we can, I think, here speak of psychological structures as coming to be internalized in the organism. By this we mean that through processes of learning the structure of the organism must be modified in ways such that ordered responses to stimuli (both in perception and in response in the narrower sense) can be produced without repetition of the learning experiences.

With respect to perceptual content we speak of this altered structure as the physical basis of memory, with respect to performance patterns, as skill. It may be suspected also that the learned potentialities of pleasure production (including, perhaps especially, erotic pleasure) constitute another focus of this organic modification.

There is a sense in which this conception of the boundary interchange between psychological and organic systems and their interpenetration with each other constitutes an approach to the old mind-body problem. We do not wish to stress the philosophical aspects of the question. We do stress that once one has learned to avoid reifying analytical systems and has understood that plural analytical systems are involved in the same concrete phenomena, there need be nothing mystical about what is meant by mind (i.e., a psychological system) as *analytically* distinguished from organism or body. When we add the conception of organization and its relation to processes of control, and the conception of emergence in an evolutionary perspective, we have a scientifically intelligible frame of reference for analyzing mind, body, and their relations to each other.

The Object-relations of Psychological Systems

Let us turn now to the second main set of boundary processes of the psychological system, namely, the interchanges with objects external to the organism-personality system. On general theoretical grounds I shall maintain that, as the psychological-organic boundary relation involves primarily an interchange of facilities, so the psychological-object interchange involves primarily an exchange of rewards. This, of course, can be strictly true only if the object in question is itself an action system. Hence for present theoretical purposes, we must regard the physical object as a special case, because the relationship established between it and the psychological system is one-sided; what for the psychological system is a reward is, for the physical object, simply a "state of affairs." In other words, the goal-attainment boundary of a psychological system—so long as it is independent, not a subsystem of a larger psychological system—defines its relations to a social system in the technical sense of the theory of action.

What, then, can we say about the relations between output and input in this interchange? Both inputs and outputs at this boundary are, we have suggested, maximized in what has been defined as the goal-attainment state of the system, the establishment of an optimum relation to the significant object in the situation. To use a term which has both social and psychological connotations, we may say that the system gains "support." As long as an optimal relationship with the object is maintained—whatever the respective shares of system and object in its

establishment or maintenance—from the point of view of the “needs” of the psychological system, events in the environment are supportive of processes in the system. Put in slightly different terms the gratification level is at a maximum—for *this* system in *this* situation.

In terms of its meaning for the system, however, the input of support must be evaluated. This in turn involves a possible breakdown of the input into components—a breakdown which of course need not be explicitly performed by the actor. The principal components seem to be as follows:

1. The input of immediate goal gratification independently of any conditions on which its continuance, repetition, etc., may rest.

2. A component of predictable (and possibly controllable) conditions in the situation, independent of any features of the relational tie between ego (the psychological system of reference) and alter (the object) on which the continuance and/or repetition of the gratification may depend.

3. An integrative tie between ego and alter by virtue of which they may be treated as belonging together in the same solidary collectivity in the sociological sense.

4. A shared system of cultural values which define *legitimate* expectations in the relationship.

It is as a resultant of these four factors that the attainment of a goal is not merely “enjoyed” but positively valued. The relative weights of these components will vary for different psychological systems and on different situations.

Psychology calls the principal category of relevant output, goal attainment, achievement, etc. Perhaps the best term is achievement. Here the primary emphasis is on the *agency* of the system, on its “decisions” or “commitment” to the goal state as a factor in bringing about that state. The balance may vary from a situation where ego merely “enjoys” a goal state freely “presented” to him without effort or foresight on his part to a goal state which he “succeeds” in attaining only in the face of the most formidable obstacles, but this element is always present. Achievement in this sense can also be broken down into the following components: (1) simple “acceptance” of the optimal situation as gratifying; (2) manipulative control of the conditions of the optimal situation which are independent of the relational tie to alter and of the common values they share; (3) maintenance of the integrative tie by virtue of which ego and alter are bound together; (4) conformity with their shared values.

In so far as the object relationship is one to *social* objects, the attainment and/or maintenance of the gratificatory state is subject to a *double* contingency [cf. 22, chap. 1]. The first two sets of inputs and

outputs are relevant to relations to a nonsocial object; the last two are always involved in a social relationship. The double contingency consists in the fact that ego's attainment of a goal is contingent not only on his own actions in relation to a nonaction situation, but also on the reactions of alter to ego's actions and their consequences for ego's goal attainment.

It is also important to recognize that, in the general terms we are using, the seeking of pleasure, so far as it serves as a motive, is not the same thing as goal gratification. Pleasure is a state of the *organism* in relation to the psychological system; goal gratification is a state of the personality in relation to the *external* object system. Of course once a psychological system has become firmly established it can learn to treat the arousal of pleasure sensations from its own organism as a goal, but only by treating the body as an "external" object.

For reasons which should be evident, the main tradition of experimental psychology has been concerned only with the first two components of "support" and goal attainment, namely, the gratificatory relation to the object (primary reinforcement) and the "conditional" factors most closely related, such as timing, periodicity and aperiodicity, and the like. Where, however, gratificatory behavior has been studied directly in relation to social objects, the other two factors have emerged into prominence, most conspicuously in the case of Freud and his intellectual descendants. Here factor *c*, the relational tie, appears mainly in the form of object cathexis; the sharing of common value is prominently involved in Freud's concept of identification.³²

Because of these considerations, one must infer that the more *general* case (in a theoretical sense) of the relation of a psychological system to external objects is that in which the most significant objects are social rather than physical. Freud was doing more than following his own special interest in human socialization when he put relations with social objects in the forefront of his thinking. But not only is this true, it is necessary to go one step further. To simplify the exposition, I have used as an illustration the situation where the social object is an individual

³² Cf. "Social Structure and the Development of Personality" (*Psychiatry*, Nov. 1958). For reasons we will take up later, it can be inferred that either where object-relations are virtually confined to nonsocial objects or where such relations to social objects as exist are highly stereotyped by instinctive patterns, the potentiality of development of psychological systems is severely limited. The main path to the development of the human level of personality is the introduction of elaborate processes of *socialization*, i.e., of learning through interaction with social objects who are bearers of a highly differentiated culture. This is a principal reason why the empirical generalizations derived from the study of animal behavior in nonsocial situations apparently are less fruitful for the psychology of the human personality than it has often been claimed they should be.

person, an alter to the ego of reference. Human beings, however, are not generally oriented to and integrated in object-relations only with discrete individual alters. Their relations are to *systems* of social objects. Ego's integration in such a relational system is integration into, the acquisition of membership in, a *social system*, in the type case a collectivity. The objects significant to him include not only the other members of the collectivity as individuals but the collectivity itself. Freud's concept of identification, for instance, must be taken to include reference to the collectivity. For example, the main identification which occurs in the oedipal period is primarily an identification with the family of orientation as a collectivity, not merely with the father or mother as individuals.³³

These considerations bring us again to the meaning of *interpenetration* in reference to psychological and social systems and the crucial concept of internalization. It seems clear that physical objects become internalized, that there is on both the psychological and physiological levels some kind of enduring structure which corresponds to every category of objects of which the individual has had experience—without such a postulate the phenomena of memory and the continuities of behavior could not be accounted for. In so far as object-relations are learned, these structures must be built up and changed through processes at least partly psychological.³⁴

The same general principles apply to the internalization of social objects, with the modifications which result from the double contingency of social relationships. Double contingency is a critical source of instability which makes the psychological certainties attainable in relations to physical objects difficult, if not impossible, to reach for social objects. Yet at the same time, the human socialization process seems to depend on the double contingency of social interaction.

Apparently, the main reason for this is an "artificial" stabilization of the environment by social interaction. This creates for the child a specially stabilized environment between the physical world and the nascent personality system. This artificial environment is more closely adapted

³³ There is another sense in which S-R-S theory may constitute the more general case, namely, that all psychological systems involve elementary units on this level. Hence in some sense, all higher organizations can be said to be "composed" of such units. But we treat organization as an independent variable in our system, so it is out of the question to *derive* the properties of the higher-level systems simply from the properties of elementary units. On the other hand to say it is the units *plus* their relations begs the question since it is in the category of relations that the factor of organization is found, and its value must be specified. For an elementarist theory these *must* be treated as parametric.

³⁴ On the general nature of internalized object systems with special reference to physical objects [cf. 20, Chap. 5].

to the child's psychological need structure than the physical world; hence it sharply modifies the conditions of learning.

The conditions of human learning are certainly complex; no one factor alone is sufficient to explain them.³⁵ Nevertheless, we may list four important conditions. First is a stable identification, or set of identifications, with one or more social objects, which entails a cathexis or attachment to the social object and the expectation of support (in some contexts "love") in return. A second condition is sufficiently severe frustration of previous expectations to disorganize previously established behavioral patterns. A third is the selective rewarding of trials in accordance with the expectations of proper behavior for the "higher" level of socialization. A fourth is the application, relative to this level, of a systematically organized *pattern* of sanctions over a long enough period to furnish reinforcement adequate to bring about internalization.

Through socialization processes of this sort the *social* object system comes to be internalized, *not* as one such system once and for all, but as *series* of progressively increasing complexity succeeding each other over a long period. What is internalized is a complex system of "expectations." These include more than definition of and motivation to expected behavior for ego alone. In view of double contingency, this could not be sufficiently specific, since how ego is expected to behave is always contingent on how alter has just behaved and vice versa. It cannot then be just a pattern of expectations for ego which is internalized but, as G. H. Mead clearly saw, a *reciprocal role-relationship* pattern, including the *general* norms governing the behavior of both ego and alter, or of a still more complex system. In other words, there must be organized in ego's psychological system a structure which corresponds to a continuing system of ego's learned complementary role relationships in social collectivities in interaction with a plurality of alters. That structure is most definitely organized on a variety of axes through generalization of patterning.

The *main* structure of the human personality may well be organized about the internalized social object systems as residues of the socialization process.³⁶ Probably this is not, in the same sense, true of animal personalities. The difference is linked to the far higher degree of control of human behavior by generalized, and hence abstract, cultural patterns.

Furthermore, not merely the *cognitive* side but also the *motivational* side of the personality is arranged around internalized social objects.

³⁵ One attempt to analyze them has been made by Parsons and Olds [see 25, Chap. 4].

³⁶ The authority of Freud, in his later phase, can be claimed for this proposition. Cf. especially *The Ego and the Id* [8], where he speaks of the ego as *consisting in* the precipitates of "lost objects."

Not instrumental skills alone, but the *goals* of the mature personality are organized through identifications and the consequent internalization. For the goal structure of the human adult cannot be derived from the *structure* of the "instinctive," i.e., genetically inborn, needs of the organism.³⁷

The discovery of the internalization of social objects must be regarded as one of the most crucial in modern psychology. On the psychological side certainly the main credit must go to Freud. It seems, however, that Piaget arrived at it independently, especially in his early work in the distinction between "moral realism" and "cooperation" [cf. 30]. Moreover, it is significant that, in somewhat different versions, it was also clearly set forth by G. H. Mead, a philosopher who has also been thought of as a social psychologist, and by a sociologist, Durkheim. From the standpoint of systematic analysis, internalization constitutes the principal link between psychological and social systems.

A particularly important consequence of the discovery of the concepts of internalization and identification is the insight that the role-expectation pattern must be understood not merely as a component of the structure of social systems, but also *at the same time* as an establishment of the personality, a part of *its* structure; i.e., they interpenetrate. This will be discussed further, after an outline of the internal structure of psychological systems has been presented.

Psychological and Cultural Systems

Input-output relations, interpenetration, and internalization can be outlined and partially analyzed in relation to a third major boundary of the psychological system, namely, the boundary with the cultural, as distinguished from the social, system. Just as for many purposes of "behavior psychology" organism and psychological system have not been distinguished, so for many purposes in the present area social and "cultural (II)" systems have not been distinguished. For more refined analytical use, for the analysis of more complex systems, however, this distinction becomes essential.

On the theoretical grounds referred to in the two preceding sections I shall state, but not attempt to justify, that the main significance of the interchanges between psychological and cultural systems is mutual integration. Thus the inputs and outputs are neither facilities nor rewards but rather, regulatory "cues" which have a primary bearing,

³⁷ These themes are much more fully developed in [25] and in my paper "Social Structure and the Development of Personality," *op. cit.*

not directly on the functioning of the system in relation to its external situation but on its internal integration. They facilitate (or obstruct) the adjustment of the units within the systems to each other.

The primary role which culture plays in the psychological system is that of *legitimation*. As a result of this stabilizing factor, the system's functioning is made subject to *normative patterns*. Thus, the culture defines the conditions of stable equilibrium in accordance with "expected" conditions. In other words, cultural values are parameters which establish certain perceptual and action thresholds and other forms of selectivity.

Legitimation can also be seen as the aspect of the organization of psychological systems most closely associated with the concept of "rationality." Rationality may be conceived as *organization* of a psychological system in accord with a system of norms so that, in specific situations, it can perform in accordance with those norms. The legitimation input (into the psychological system) may be divided into subcategories of norms: (1) adaptive-cognitive rationality, where the correctness and generality of the adaptive orientation provide the normative criteria (i.e., knowledge); (2) instrumental goal-directed rationality, where effectiveness in the attaining of specific goals is the criterion (principally skill); (3) integrative rationality, where the internal harmony of the psychological system itself is the criterion, giving each of its units and subsystems an "acceptable" place (thus minimizing "defensiveness"); (4) "moral" rationality, where conformity with a set of norms transcending reference to the psychological system in question is the criterion. Ordinarily these moral norms will be the cultural values institutionalized in the social system(s) of which ego is a member and which are shared with other members.

Turning to the output side, we may say that the general output category from the psychological to the cultural system is "motivational commitment." This goes beyond "understanding" of the relevant norms to "acceptance" of them as guides to action in particular situations. The capacity for such commitment is attained only through full internalization of the norms; i.e., the norm is not simply an aspect of a pattern of sanctions imposed by external objects but becomes a part of an internal regulatory mechanism of the personality system itself. Motivational commitments can be classified on essentially the same basis as types or components of legitimation.

Particularly in the earlier stages of socialization the internalization of norms proceeds empirically as part of the same process as the internalization of social objects; indeed since the social object system is culturally organized and controlled, it is impossible to internalize one without the

other. In the later phases, however, it seems possible to differentiate the two functions in social interaction, and there is evidence that they also become differentiated in internal function in the personality.

Probably after what Piaget calls the stage of "moral realism," the individual comes to be capable of discriminating a rule from an aspect of a concrete solidary or antagonistic social relationship. He becomes capable, as we say, of treating the individual "impersonally" as a "case," or in more technical terms, by *universalistic* standards. This we may regard as the differentiation of what is primarily a cultural reference from the matrix of a social system reference.

Freud's distinction between the ego and the superego is the best point of reference for an attempt to distinguish internalized cultural patterns from internalized social objects in the personality. Freud conceives the ego as composed of the precipitates of lost social objects. It is also that sector of the personality most directly governed by the "reality principle." This is quite consistent, for Freud considered the *social object* system as the crucial component of reality.

The superego, Freud says, centers on the "parental function." [Cf. 8, 9.] It originates when the child's family of orientation is internalized as a collectivity, but precisely in its *governing* aspect. We may then surmise that once this familial object has been "lost" in Freud's sense, it is the system of *norms* symbolized and implemented by the parents which becomes the focus of organization of the superego. It is noteworthy that for Freud the superego does not begin to function fully until the latency period, when the child is emancipated from his family. This coincides broadly in timing with Piaget's formulation. It is our view that this differentiation and its maintenance require a special mode of social interaction mediated by highly generalized cultural symbols.

Finally, we may note one further aspect of the relation of a psychological system to its situation or environment. This is the residual category associated with the pattern-maintenance function, not as it operates through the integrative channels we have just discussed, but directly through congruence or incongruence of values. Any concrete behavioral sanction would, as we see it, be resolvable into the three components already reviewed as facilities, rewards, and legitimation sanctions. The significant sanction form here is purely attitudinal—as to whether the system in question "fits" into the larger value complex of which it is a part. There is thus an absence of expectations on either side, for performance or for sanctions specifically contingent on one another. What is involved is rather an over-all "judgment of worth" which we may call an attitude of *esteem* (or *disesteem*). This is the level of regard or respect in which the system (most clearly in the case of a total personality) is held by other social objects. Change of status in this respect calls for

more than specific types of acts; it calls for a change in the structure of the personality system, including its value content.

The Internal Structure and Processes of Psychological Systems

This brief review of the principal boundary processes of a psychological system vis-à-vis nonpsychological systems can serve as the starting point for systematic treatment both of the internal structure of the system and of the mechanisms involved in its functioning. We will take up these problems on two levels, first that which abstracts from internal differentiation and second that which attempts to deal with it explicitly.

In the outline of the general structure of the theoretical system we are using here, it was pointed out that the value system served as the general point of reference for analysis of structure and processes in the system. This has been the case in the discussion of boundary processes between a psychological system and the nonpsychological systems of its environment with which it interacts. That discussion did not take account of variations in the *content* of psychological value systems. This will be the task of the present section.

A psychological value system is the main point of interpenetration between culture and the psychological system, in the most interesting case, personality; it is defined as internalized cultural pattern. On the cultural level a psychological value system must be placed in a frame of reference which defines the category or population of higher-order systems within which the particular range of variation falls. The terms we will use, referring as they do to relative primacy of function in the system of reference, are always relative, not absolute. We may thus say that, within the given reference system, personality or culture A is *more* adaptively oriented than personality or culture B. But we should never state or imply that there is, in the present theoretical system, an absolute standard which defines "the" adaptive value system, or a cognate standard for any other type.

Differences between values in psychological systems can be stated as differences in the relative degree to which adequate or satisfactory solutions of the four functional problems have been emphasized. This is because, relative to a given system situation definition standards of adequacy *for each type* of solution are built into our theoretical analysis of the types of input-output balancing. Thus, given ego's paramount goal, estimation of the degree to which this is attained in a given state of the system is not arbitrary but in principle can be precisely defined (whatever the empirical difficulties).

A somewhat related statement is that the value system is the focus of a set of cybernetic control mechanisms which regulate the relations

between the given parts, essentially by assigning priorities whenever a conflict situation arises or becomes a serious possibility. Hence, the most fundamental feature of a value system is the order of priority it gives to the solution of each of the four system problems. In interpreting this statement, however, it is necessary to be clear about two things. First, there are always limits to the extent to which any of the four problems can be subordinated or neglected; we have defined all of them as functional exigencies. Thus the denial of *all* goal gratification would be as realistically impossible in the long run as would total neglect of adaptive imperatives; it is always *relative* importance which is in question, never whether or not an interest shall be respected to some degree. Second, the realistic possibilities for rank-ordering values are always a function not only of the kind of system involved but also of the situations in which it is placed. Since situations may be presumed to vary over a considerable range, the value system never can fully prescribe what the priority relation must be within any *short* time period unless it is to develop a rigidity in the total system which is incompatible with capacity to cope with variability in the situational conditions. During longer periods there may be structural change in the system.

Rank-ordering of functional problems presumably entails giving relative "weights" in the determination of action of the system to the principal types of different performance disposition and of sanction need. By definition the largest weight is assigned to the paramount functional need-disposition, and so on down. Therefore a typology of psychological systems³⁸ resting on types of values must give first attention to the need-disposition which occupies top place in the rank order; the range of variability will become progressively smaller as a function of variations in each of the others down the line until the fourth is reached. At the same time, it is most important to note that the relative weight of the lower-order functions is never *completely* determined by their position in the rank order. We can say only that this position sets certain limits to the variation; it cannot exceed an upper limit without changing places with the unit above, conversely it cannot exceed a lower limit without exchanging places with that below. This consideration introduces an important element of flexibility into our classification. This flexibility does not, however, destroy its validity, since shift in rank order presumes certain qualitative changes in the characteristics of the system.

By adopting this approach, certain combinatorial possibilities in the theoretical universe we are dealing with provide a framework of analysis.

³⁸ This analysis in principle applies to *all levels* of psychological system. It is, however, easiest to illustrate it at the personality level and we will hence couch our discussions in those terms.

If attention is paid to the first-order function then there are four "primary" types of value system; each of the four system-problem solutions holds the position of first priority in one of the four types. Each of these types can then be subdivided according to which of the remaining three holds second place in the priority scale, yielding twelve possible "secondary types." Finally, each secondary type can be divided into two according to the relative priority of the remaining two possibilities, yielding twenty-four "tertiary" types. Since we assume a universe where each type is characterized by the rank order of four components, this exhausts the combinatorial possibilities, since last place is fully determined when the other three are given. However, it should be remembered that there is a further range of flexibility open in the area of range of "weights" within a given place in the rank order.

We have held all along that a primary feature of a value system as the focus of internalized culture is its property of generalization. In the present context, we interpret this to mean that the type of solution arrived at for any one of the four system problems will be generalized to the other three; the orientations toward all four will tend to constitute a *meaningfully* coherent system. We would hold that this system provides the central structure of a personal or other psychological value system. It is relatively easy to spell out these implications for first-order values and hence the broad characterization of primary-value types. Because of the refinement and subtlety of the distinctions which have to be made, spelling out implications becomes increasingly difficult as we proceed to the levels of secondary and tertiary typology. In all this, the problem of the range left open for "variation" must also be kept continually in mind.

Given that the locus of the primary value pattern is in the latency or pattern-maintenance aspect or "part" of the system, our problem is to spell out the implications of a given type of content of this "cell" (in the formal paradigm) for the other three functional contexts and input-output balances. Throughout this discussion it will be obvious that we assume that the primary pattern tends to be maintained; the question is how.

Let us start with a system which tends to maximize goal-attainment values. Remember that we assume that the goals of the mature personality are for the most part learned goals. Our present analysis is not concerned with the content of these goals; this depends on the kind of socialization process the personality in question has been through. Our present concern is with the implications of primacy of attainment or gratification of *whatever* goals have been learned over other system functions. In the goal-attainment function, this will give a primacy of functional performance and/or gratification; in this particular sense, we

may speak of a primacy of interest in gratification or in power.³⁹ But it should be kept clearly in mind that given the goal content, this is true of *any* goal-oriented activity. What is peculiar to this value or structural type is not this, but the *primacy* of goal-attainment values, hence of interest in power including both the performance aspects and the gratification aspects, over the values of other system functions.

Psychological discussion of cognate problems has tended to emphasize the interest in goal gratification (and often its organic counterpart, pleasure). But if attention be shifted to the relational aspect and the concern with alter as the source of gratifying sanctions, this primacy leads, at least in one sense, to an interest in *power*, in capacity through effective performance to control alter's behavior in the interest of ego's goals. The balance between the gratification and the power emphases then will be a function of relative activity or passivity in handling situations, i.e., the degree to which ego attempts to *control* the situations in which he seeks goal consummation as against the degree to which he accepts them as they come.

Second, with respect to the adaptive function the goal-oriented personality will tend to give primacy to "practical know-how" for the attainment of his given goals. He will then tend toward a strong pragmatic-adaptive interest with a complementary limitation on the interest in *generalization* of facilities. Ego will tend to feel that adaptiveness beyond the limits of his own goal interests is not of "any use." He will not maximize the adaptive function, but will tend to subordinate it to goal attainment.

Since this psychological type is primarily concerned with external situationally oriented goal attainment, his handling of the integrative problem of his personality will tend to be subordinated to this interest. The most important consideration here is that, however important the conception of a paramount goal for the system as a whole may be, and it certainly is high, concretely there will always be a plurality of particularized goal interests which compete for gratification and which cannot all be satisfied at once or all of the time. The primary integrative focus then will be the rationing of gratification opportunities among competing goal interests. Many such interests will be inhibited at any one time, and all of them at some times, in the interest of optimizing the gratification balance of the personality as a whole. Put otherwise, this involves hierarchization of goals, i.e., a rank-ordering of the rights of different subgoals to gratification. There is, it may be noted, an important

³⁹ A special case of the potency referred to above. This presumes that power is generalized capacity to mobilize the resources of a system for the attainment of a goal. Personality and social system references of the concept should not be confused.

relation to the adaptive problem in that gratification opportunity is a function of control of the relevant objects. Gratification can be jeopardized either by the competition of another goal interest or by the refusal of the object to "cooperate." Optimization requires some balance in coping with two sources of danger which are largely independent of each other.

Finally, what the pattern-maintenance function itself? The principal "drive" of a goal-oriented system is to attain and retain the consummatory state. But because of the inevitable factors of instability in the situation and perhaps within the system, for the sake of long-run maximization or optimization of gratification, consummatory interests must suffer renunciation or inhibition. Inhibitions or renunciations may be called for in the face of adaptive and integrative exigencies; the pattern-maintenance function regulates the relationship between the other three and determines which shall take precedence on a given occasion.

But there is a further pattern-maintenance problem, that of maintaining commitment to particular goals. Probably the most important threat to pattern maintenance within the system lies in certain consequences of personality development; namely, that both values and goal structures have had to be built up by a process of socialization. Hence the residues of earlier value- and goal-commitments are still to some degree operative; under strain there is always the possibility of *regression*, i.e., of abandonment of more mature goals in favor of reactivation of earlier ones. Control of tendencies toward regression is a primary pattern-maintenance function in the personality.

We have suggested that the rank order of the other three value-pattern components below the one enjoying primacy is intrinsically open to the whole range of combinatorial variation. Yet as a function of the intensity of commitment to one primary value type there may be a "strain" in the direction of favoring one subsidiary rank order over another. In the present case, the central consideration appears to be the high significance of situational relations. This would tend to give second place to the adaptive function, subject to the limitations we have indicated. This tendency could be counteracted, for instance, in the case where maintenance of commitment to the goal pattern in question involved sufficiently serious internal strains, which would tend to give second place either to the pattern-maintenance or the integrative function.

As a second major value type, let us consider that based on the primacy of adaptive values. This, it will be remembered, will involve interest in control of the situation of action, and in the generalization of the means of control relatively independently of particular goals. With respect to goal type, primacy of adaptive values will tend to a kind of

goal we may call that of achievement or *success*. This means essentially that subsidiary goals will be evaluated in terms of their contribution to the larger goal of putting ego in a position to achieve *any* goal he may come to be interested in, with the specification of particular goals left in abeyance. We may speak of this position as that of generalized *mastery* extending over the whole range of actual and possible situations. In the interest of this generality of mastery, it is necessary to avoid too many and too deep commitments to particular goals and particular solidarities.

On the positive side, as generalized means to success, two categories seem to be of primary significance. Internally the generalized means of control of situations is *knowledge*, particularized in the form of skills. But knowledge is essentially the capacity to understand and prepare for anything which may happen in the external world. Externally, the central category is wealth which, in a highly differentiated society, is the most generalized means of influencing others' behavior in the desired direction.

On the negative side, with reference to goals and otherwise, the personality with primacy of adaptive values will tend to be characterized by high levels of discipline. He will be wary of expending his resources on gratification of particular goal interests if this tends to impair the maneuverability of his position with reference to alternatives. He will also tend to be wary of commitments to solidarity since this also impairs his freedom of action.

From this, it follows that ego's pattern of adaptive action will be characterized by a relatively active disposition to exploit opportunities for improving his adaptive position; it is in this direction that his primary goals will lie. His integrative pattern, on the other hand, will be characterized by a kind of utilitarian self-discipline, a strict control not in the interest of harmonization as such but of adaptive efficiency.

The pattern-maintenance problem will have a similar but in some respects different meaning in this case from that of goal-attainment primacy. It is similar in that the maintenance of pattern is secondary to other interests, and the focus of these other interests is on relations to the situation external to the system. It is different with respect to the significance of particular consummatory goal states. The essential point is that generalization in the situational reference tends to be maximized. Once such a value system has become established there is less pressure to attain particularized consummatory states, and the question of consistency with an internalized pattern is more important. Perhaps we may say that, for the goal-oriented type, the primary focus of integrative strain is the problem of "expediency," namely, whether all-out consummatory commitment is justified in the particular instance. In the adaptive case, on the other hand, it is the problem of the *organization* of the

adaptive system as a system, since the essential focus is on the total *level* of adaptive success, not on any one component contributory to it.

Can we say anything about the strains toward a preferred rank order of the remaining three value components in the case where adaptive interests are primary? The same basic consideration which was noted in the goal-attainment case seems to apply here also, namely, the high importance of the relation to and control of the external situation. This would indicate the probability that other things equal, second place would go to goal-attainment values. Probably, however, pattern maintenance would be a closer competitor than in the case of goal-attainment primacy, because of the great importance of cognitive orientation and of organized consistency in the adaptive system. Too strong consummatory interests, because of the particularistic relations to external objects that they imply, would tend to threaten this consistency.

In both pattern maintenance and integrative primacy, the primary orientation of the system is inward rather than outward to situational objects. Hence, objects are more likely to appear as potential sources of disturbance and less likely to be sources of opportunity than in the other two cases.

In the pattern-maintenance case, the primary concern is with maintenance of the integrity of the internalized value system itself. According to its content there will be more or less need for active "realization." Where goals are specified, as they must be, the measure of effective performance will not be so much the fact of attainment of a consummatory relation to the goal object as the *meaning* of this attainment in terms of the value pattern. There is, therefore, a tendency to dichotomize consummatory situations, in realization and in expectation, either as expressive realizations of the value pattern or as in conflict with it. The latter can be justified only in terms of expediency and this is the primary integrative problem for such a value system. The tendency, therefore, is to accept certain goals, and pursue them with great energy, and to reject others as totally unworthy. The person with high primacy of pattern-maintenance values is likely to be an uncompromising "idealist" who defines situational objects in black and white terms as either proper goal objects or totally inadmissible.

When we come to the adaptive problem, the distinguishing feature of the pattern-primacy personality is the minimization of the importance of successful adaptation. The rigid pattern of selection which he imposes on possible goals precludes a strong interest in maximizing the generalized potential for goal attainment. He wants to know what specific goal you are committed to. He will go all out in acquiring means for approved goals, but is very suspicious of any command of facilities where the uses

to which they might be put are not carefully specified. If he is a religious person he wholeheartedly approves a wealthy church, but is very suspicious of wealth as such and its secular uses. Again, therefore, he tends to vacillate between commitment to adaptive values where the goal is approved, and rejection of them where the goal is not. The sheer fact that certain facilities are inherently generalized presents a major dilemma for him. This applies both to knowledge and to wealth.

The integrative problem, finally, will tend to be met by a pattern of hierarchization. But this is of a different character from that involved in the goal-attainment primacy type; its primary function is not to ration gratification opportunities, but to ensure the ascendancy of the primary values and to ward off threats to this ascendancy. The internal discipline then is likely to be more repressive, indeed, to take actual advantage of repression, than in the other cases. The essential principle is to minimize the necessary concessions to need-dispositions other than those directly concerned with the primary values.

It seems probable that in this case integration is likely to come next to pattern maintenance in order of importance, because the first line of defense of the value system is internal. However, if the value system calls for a strongly active orientation to external objects it can be that goal attainment will take the second place.

Lastly, let us sketch briefly the implications of the primacy of integrative values. The plurality of need-dispositions will be frankly acknowledged, and the primary effort will be to weld them into a harmonious system so that they can "live peaceably together." There will be a strong tendency to take the underlying pluralism for granted as grounded in "human nature." The hierarchical aspect of rank order will be recognized, but as a "natural" thing rather than as something to be enforced for other reasons.

In the goal-attainment context the primary goal type will be what we have called "satisfaction"; it will be the attainment of diffuse solidary relationships to persons and collectivities so that their patterns of integration can be reflected in and reinforce that of ego's own personality. In the adaptive sphere there would tend to be the least motivation to positively active adaptive improvement and the greatest tendency to "come to terms" with situational objects in such a way as to protect the internal integrative balance. The primary exception to this would be the interest in particular types of skills, primarily in the field of human relations, which would facilitate the attainment of satisfaction goals.

The pattern-maintenance function would also tend to be relatively subordinated, and above all, too rigid idealism would be avoided. The value system would have to provide internal sanction to the pluralism of motivational interests and give positive value to their variety and to the

task of harmonizing them. There would be a premium on self-control, not in the interest of a single motive but in that of moderation and balance.⁴⁰

It seems probable that second place will go to the pattern-maintenance function, since only relatively "strong character" can provide protection of the internal harmony ideal against the inevitable situational pressures with their wide variations. In particular situations, however, the

TABLE 1. PSYCHOLOGICAL ORIENTATION TYPES

Mode of meeting exigency	Type of value system			
	Goal-attainment	Adaptation	Integration	Pattern-maintenance
Goal-attainment	Commitment to implementation of paramount goal	Generalized success or mastery	Pluralism of acceptable goals	Commitment to <i>meaningful</i> goals and avoidance of expediency
Adaptation	Maximization of power (practical know-how) for paramount goal	Interest in knowledge and/or wealth	Tendency to compromise with external object interests	Restriction of generality to bearing on values
Integration	Rationing of gratification opportunities	Discipline in the interest of active mastery	Internal harmonization, avoidance of conflict	Hierarchization: repression of conflicting needs
Pattern-maintenance	Control of expediency and regression	Maintenance of organized self-control	Control in interest of moderation and balance Avoidance of rigid idealism	Maintenance of integrity of the value position

appropriate type of goal attainment can become extremely important where external reinforcement of the internal pattern becomes crucial or is threatened.

Table 1 gives a schematic view of the relations between the first-order value-pattern component and each of the other three just reviewed. Each column represents a schematic description of one of the four primary psychological value types; since there are four principal ways of meeting each of the four system problems, sixteen combinations are possible. The diagonal from left top to right bottom shows the points

⁴⁰ The Confucian Chinese ideal of the "superior man" may be regarded as one of the classical expressions of the value pattern for the case of personality.

in each case where the strongest motivation to fulfill a value requirement will come, because this is the functional problem which is given first place in the rank order for the type in question.

No attempt is made in the table to represent the more refined bases of variation. It is, however, important to our general argument that each of these primary types can, by the logic of rank-order analysis alone, be divided into three subtypes according to which of the three remaining value components takes the second place, and the resulting twelve can again each be divided into two according to which of the remaining two comes next. Furthermore, it should be remembered that, still further, concrete variability is possible because a rank-order position of a given component is interpreted to involve a range rather than a point. Since we restrict the components discriminated to four, for most empirical purposes it would seem that these ranges should be relatively wide. Finally, it will be remembered that we emphasized the relativity of the parametric points of reference which would locate the total population of personal value systems our scheme can deal with relatively to others.

These points we emphasize to forestall criticism that we are imposing a classificatory strait jacket on what many feel to be the infinite variety of psychological types, a classification which is necessarily unrealistic because it cannot take account of the finer nuances of difference. We have distinguished 24 possible types (by no means all of which are of equal empirical probability), and each of these in turn may vary as a function of differences in the population parameters and in position of the variables within the range consistent with maintaining the rank order. In putting forward this classification, we have still not taken into account a further very important source of variability; namely, that concerned with pathological factors.

If psychological theory is to deal with the problem of variability in any theoretically determinate way, it cannot rest content with simply asserting the fact of infinite variety. It must impose some order in terms of a manageably small number of categories. We feel that the scheme just outlined offers quite sufficient possibilities of complexity to occupy theorists for a long time. Whatever other faults it may turn out to have, gross oversimplification of the problems is not likely to be one of them except in the sense that all scientific analysis oversimplifies. What we have attempted to do is to *derive* a high order of complexity from varying combinations of a small number of elementary components.

The same pattern of analysis which we have used to develop a classification for normal psychological system types can also be used to approach the problem of characterization and classification of the *mechanisms* of personality functioning. For our purposes the first essential consideration is the distinction between those mechanisms which mediate

the relations of the total personality system to the situation external to it and those which mediate internal processes between units of the system. The former we will call the mechanisms of adjustment, the latter the mechanisms of defense. It is clear that so far we have laid the groundwork only for approaching the mechanisms of adjustment since we have not yet dealt with the internal differentiation of the personality system.

The scheme we have been using provides a very simple basis for a classification of patterns of adjustment. The essential point is that the equilibrium of the system depends on the *balance* of inputs and outputs both in each category and in all of them taken together.

In the most direct sense, each of the balances can be affected by ego's either increasing or decreasing his own rate of output. Thus in the consummatory phase, ego may increase the intensity of his effort to maintain the consummatory relation, presumably by performances thought to be gratifying to alter, or he may decrease the rate of consummatory effort and thereby also decrease the input from alter by lessening alter's "incentive" to continue the input at the same rate (which from alter's point of view is output). Similarly, in the adaptive context, ego may increase the rate of adaptive output by adding to the effort or energy deployed in this direction, and by withdrawing energy or effort, he may decrease the relevant output and, presumably through the effect on alter, the corresponding input. Finally, the same would apply at the integrative boundary in terms of increasing or withdrawing motivational commitments.

Thus with respect to each of the boundary relations of the system we have, basically, two possible directions of change, a positive active, in one sense, "aggressive" direction and a negative passive "withdrawing" direction. In any given case, the effect on ego's personality of such a change of course depends on the shape of the function, namely, the relation of the rate of addition or subtraction of output to valued input rate. But also it depends on the shape of the function in terms of which alter reacts to ego's changes of behavior. We are not in a position to specify these functions on either side except to say that ego's curve of "demand" for any category of input from alter will tend to slope downward to the right, and the curve of his supply of any category of performance in response to alter's actions will tend to slope upward to the right.⁴¹

⁴¹The assumption is that output is a quantitative function of rate of input received for each level of output; equilibrium is defined as the point where certain quantities coincide. The theoretical model which is most highly developed and most appropriate to the present problem is that of supply and demand functions as used in economic theory. The statement just made about the slopes of input and output functions may be regarded as a statement of the law of effect. [Cf. 27, Chap. 1.]

These possibilities of increase and decrease of output respectively apply to the three "open" boundaries of a psychological system. These may be said to constitute the elementary mechanisms of adjustment. But there are two orders of possibility of organization of these elementary components into higher-order mechanisms. These have essentially to do with the higher-order and the lower-order references respectively in the hierarchy of normative control and of cultural generalization.

The higher-order reference concerns the importance of the sanction of esteem, as we have called it. For ego, the *meaning* of a given alteration in any one type of performance output may be not to secure a specific change in the rate of the corresponding category of input from one or more specific alters, directly, but to enhance the level of esteem in which ego as total personality (or other relevant system) is held by the significant alters (and also by mechanisms involving internal processes, perhaps his level of self-esteem). This enhancement of esteem will depend on the same types of acts involved in the more specific performance-sanction interchanges, but directed to a different and more generalized (with respect to ego as a personality) meaning-level. The meaning of esteem must be defined in terms of a value system *common* to ego and alter, which is internalized in both their personalities and institutionalized in the social system in which their interaction takes place.

The lower-order reference, on the other hand, concerns the importance and the possibility of *control* of the situation, i.e., of alter's behavior. There is a sense in which what we have just called the elementary mechanisms of adjustment are to be regarded as instruments of control because, in each input-output context in social interaction, alter's behavior is always in some degree, contingent on ego's, as well as the converse. But by control in a broader sense, we mean *combinations* of the three output types (and also possibly of the manipulation of esteem) in such a way that a desired *pattern* of alter's behavior involving several of the input categories to ego's psychological system can be brought about or made more probable. The most familiar case is adaptive activity which is oriented to increasing the probability of consummatory situations occurring, through "inducing" alter to enter into the consummatory relationship, or "coercing" him into it.⁴²

Control thus, as we conceive it, is the process of enhancing the input

⁴²By inducement, we mean the offering of rewards for alter's "cooperation," i.e., using control of the situation to make it more favorable to alter's interests than it otherwise would be, contingent on his doing what ego wants. By coercion, we mean the obverse case of threatening to use control of the situation to alter it to alter's disadvantage if he does not do what ego wants. "Seduction," as the term is used by psychoanalysis, is a special case of inducement.

from alter's action (or reducing it if this is "desired") by *indirect* measures, i.e., by operating on *other* elements of alter's input-output balance than the one directly of interest to ego with a view to the effect on one particular performance-sanction interchange.

Before turning to the internal differentiation of the personality system, a word about the relation of the mechanisms we have outlined to processes in the psychological system over time. We have suggested earlier that if the situation of a system were perfectly stable and if its internal state were stable (including the flow of energy from the organism) the rates of input-output and their ratios would also be stable. But this patently is never the case for long. The situation and certain internal factors are continually changing and there must be "responses" to these changes. But neither changes nor responses are random. They tend to become structured in the form of differentiation of types of response (in terms of relative primacy) at different periods in time, e.g., in some kind of cyclical periodicity. The diurnal cycle of wakeful activity and sleep is probably one of the most fundamental of these; sleep certainly is partly a physiological phenomenon, but it seems to us most unlikely that psychological factors are of negligible importance. In other words, the differentiation of behavior into temporal phases which has been clearly identified in small groups and in large-scale social phenomena such as the business cycle, certainly applies to psychological systems as well.

Determinate temporal phases result from the combination of two fundamental considerations. First, the external factors affecting a psychological system are continually changing. If to some extent the system did not change its state in response to them, the effect of the range of change in its inputs would be intolerably great and would be incompatible with the maintenance of a stable organization as a system. Second, a stable organization cannot change its state in nearly random fashion without dissolution. If the effect of situational fluctuations on the system were not partially neutralized by "active" mechanisms which partly counteract these effects, it equally could not persist as an organized system. The emergence of a *pattern* of changes of state over time may be said to be the consequence of the fact that neither of these two "radical" solutions is realistically possible; the actual process is a "compromise." There must be some "give" in response to the instability of the situation, but also there must be some resistance. Different types of psychological systems will presumably have patterns characterized by different "amplitudes" of situation-determined fluctuation and duration of phases. Thus, a "psychopathic personality" tends to be *too* responsive to situational changes, a "compulsive" personality too unresponsive.

The Internal Differentiation of Psychological Systems

Included in our definition of a system for purposes of the theory of action was the proviso that it must consist in at least two interacting units; otherwise it cannot be treated as a system but only as a unit in a larger system. We also stated that systems of action tend to differentiate, and though by no means exclusively, in the *first* instance, this differentiation tends to take place on functional lines. Such functional differentiation tends to define the goal orientations of the differentiated units or subsystems, the rule being that the goal of the unit is a "contribution" to the functioning of the system, tending to be specialized in one principal function in the primacy of this function over others.

This functional basis of specialization with the system as a reference point cannot alone determine the structural patterning of any system without modification. For the units themselves are systems subject each to its own functional exigencies in categories other than goal attainment and the system as a whole is subject to situational exigencies which may modify the "ideal" structure. Above all, the same order of input-output relations and of interpenetrations obtains between units in their capacities as subsystems as obtains between the larger system and the situation in which it is placed.

To deal with all the ramified possibilities of modifications of psychological systems at a functional basis of differentiation is beyond our limits of space or competence. Furthermore, it must be noted that many psychological systems do not attain a degree of differentiation where even the four basic functions can be clearly discerned as foci of differentiation. The best we can do here is to take a hypothetical four-unit psychological system where the primary basis of differentiation has been the four system problems as outlined in our general discussion of systems of action. It is hoped that in this analysis of the psychological output and input types involved we can identify some familiar mechanisms of psychological functioning.

What we have called the functional basis of differentiation serves to "locate" the unit in question relative to others in our action space. Besides the coordinates of location, however, we specified that a unit has a property of "potency," of relative weight in the system. We suggested, too, that in the case of psychological systems what we mean by potency can be identified as what Olds has called "motive force." Hence in addition to the location of the units, an essential parameter of a system is always the *distribution* of motive force between its units. This will in turn consist in two components: what Olds's calls "intrinsic" motive force, which is relatively stable and can be redistributed only gradually, and his "added" motive force [see 20, chaps. 3, 4], which

is more variable and continually redistributed in the course of system processes.

It may be assumed that, in any system capable of integrated behavior, the distribution of intrinsic motive force is unequal and that the highest level of intrinsic motive force probably will be found in the unit specialized in the function of goal attainment. This is specifically true vis-à-vis the adaptive unit, as Olds has convincingly shown, but I think it is more generally true; it is a condition of capacity to "act." Indeed a change in relative position, as in revolutions in social systems, simply makes what was previously a "means" into an "end" and endows it with the qualities of a goal-specialized unit.⁴³

We may suggest that the distribution of the intrinsic component of motive force, which is a stable property of the unit in question, belongs to its "pattern" features (Olds refers to the "pattern force" of the system) and in general is not involved in the input-output interchanges between the units of the system. Hence, we can ignore it for the rest of the present analysis (i.e., we treat its distribution as given and do not attempt to analyze changes in it).

It should be clear that the boundary interchanges between the units of a system—i.e., the internal processes of the system—should be subject to analysis in terms cognate with those employed in analyzing the interchanges between the system itself and the situation external to it. However, there are two differences. First, we are dealing with a different level of system, the personality as a whole composed of intrapsychic units—hence, the parametric references must be different from those appropriate to the external boundaries vis-à-vis nonpsychological systems. Second, we are dealing with all of the internal boundaries of a system, not only the three external boundaries considered before. There should be six interchanges between each pair formed by the possible combinations of the four functionally differentiated units.⁴⁴

First, let us take up the interchange between the primarily adaptive subsystem and the primarily instrumental goal-oriented subsystem. This is the relation in which, to use Olds's terms, "stimulation" is transmitted "forward" from the stimulus-processing unit to the goal unit, and "motive force" is transmitted "backward" from the goal unit to the adaptive unit.

Stimulation (sometimes also called "expectancy"), on the intra-

⁴³ In general Olds's concept of "added" motive force in the psychological sense just defined is cognate with power (in the political sense) for macroscopic social systems. It is interesting that the properties both of motive force and of power are perhaps in about the same stage of imperfect theoretical clarification. At any rate the present discussion of motive force must be considered extremely tentative.

⁴⁴ In this we follow the paradigm set forth earlier. [Cf. Fig. 2.]

psychic level, is cognate with organic energy on the level of input into the psychological system. Generally in the psychological phase cycle, it is a set of conditioned stimuli which are the external activators of this system. What it does in relation to the goal-attainment system is to "put at its disposal" control of the relevant cognitive mapping or planning facilities which have been built up in the system.

The reverse flow is that of added motive force. By this in psychological terms is meant the motivational capacity to perform the *additional* information-processing job which is needed for the implications of this particular stimulus to be evaluated in terms of the motivational interests of the system. We may say that an *expectancy* for gratification—indicated by the internal reaction to a conditioned stimulus—stimulates internally the mobilization of the resources for the job of planning *how* to get to the goal.

This interchange is a crucial one because of its relevance to a historic psychological problem, namely, what mechanisms can account for the "teleological" aspects of behavior? Some schools, of course, have held that any "backflow" process was impossible since only antecedents influenced later events.

We feel that this view neglects the clear implications of a conception that processes occur in a *system* of interdependent units; it fails to see the possibility of treating the psychological system as a system of plural units with mutual feedback relations to each other. It also confuses antecedence of particular events in time with function in the system.

Once we see information processing, on the one hand, and commitment to instrumental action, on the other, as the functions of two different units in the system, there is no difficulty in conceiving of a shifting balance of internal interchanges between these units over time in which the output of each is instrumental in facilitating the other. It is essentially a feedback system. The "backflow" of motive force may be treated as a feedback consequence of the activation of the adaptive unit by stimulation and its communication of that stimulus "forward" to the goal unit. Vice versa, the activation of the goal unit, e.g., as part of an internally determined "need" cycle, may produce an output of motive force which leads to "seeking" the appropriate external stimuli, and when they have been found, passing expectations "forward" to the goal unit.⁴⁵

The interchange of stimulation for motive force between adaptive and goal units is not sufficient to account for the determination of actual courses of action of the system as a whole. A second vital set of processes

⁴⁵ On the level of the macroscopic social system this interchange is cognate with the investment of capital in the economy and the corresponding output of "control of productivity." Capital, we hold, is a form of political power and as such cognate with motive force. [Cf. 27, Chap. 2.]

implies an interchange between the goal unit and the integrative unit. Here the problem is as follows: the goal unit is activated, we shall say, by an expectancy of a particular gratification opportunity in relation to an external object. But this is only *one* of many possible gratification opportunities; there must be an ordering of the acceptance of such opportunities in time and as between the different "needs" of the system. Essentially then, there is needed an adjustment between the unit which has responsibility for commitments and the integrative unit which must give "consent" or "support" for the commitment in terms of its bearing on the internal balance of the system. In this interchange, we suggest, the output of the goal unit is commitment or "decision to act," whereas the corresponding integrative output is "support." Apparently, this means conflict arising from the commitment to accept or to renounce a particular gratification opportunity is minimized through readjustment of internal rewards within the system. We have reviewed several types of such integrative process such as the rationing of gratification opportunities. Rigid scheduling is another possible device.

Since relative to the organism, a psychological system is essentially a set of mechanisms of control, we suggest that the main control of behavioral responses in the organically manipulative sense rests in the pattern-maintenance unit of the psychological system. Using a sociological analogy this is a "technical" function, and technical functions are located in this subsystem. Hence what Olds calls the "response-control" unit may be located here. The same basic psychological structure can, of course, be the controller of the use of facilities outside the individual's own organism, provided they are controllable. The basic *psychological* function is not operation of facilities but implementation of control; it is specification and authorization of use, not the technical processes by which a goal state is brought about.⁴⁶

The psychological question then concerns the factors involved in the determination of the *utilization* of the lower-order facilities available to

⁴⁶ To take a particularly dramatic example, I happen to be writing this section aboard a ship traveling from the United States to Europe. One of my goals recently has been to "get to Europe." But since boarding the ship in New York, and until I disembark on the other side of the ocean, I will not have "done" a single thing which contributed to the physical transportation of my body across the Atlantic. That is all done by the ship controlled by its officers and crew. Indeed, I am strictly forbidden even to try to influence the process. Psychologically, what I have "done" is not the technical action of transportation, but to *decide* to go, to make a reservation, to *pay* for it, and to appear at the proper pier in New York at the proper time. Then, once aboard, *all* I can do toward this goal is to wait until the ship gets me there. I have *controlled* the relation between the ship's operation and my own change of physical location, but in no technological sense have I "transported myself" across the Atlantic.

(i.e., controlled by) the psychological system, notably, but not exclusively, the responses of the organism. The first condition of the activation of response facilities is adequate *information* about the situation. This is given in the interchange between the pattern-maintenance unit and the adaptive unit. Organized, processed information is received—and its output by the adaptive unit is a function of the motive force which its expectancy-output has elicited from the goal unit. This information tells (to some approximation) *what* response or other technical procedure would be most effective in attaining the goal state.

What does the pattern-maintenance unit “produce” to balance against its input of processed information? Essentially it puts the facilities of the organism at the disposal of the adaptive unit for achieving *its* functions; i.e., it controls the perception of external events and the processing of information (this includes relating it to the stored and catalogued information we call memory). I would thus locate memory in the pattern-maintenance subsystem and in the pattern-maintenance “cells” of the other subsystems.⁴⁷

A second fundamental condition of the commitment of response facilities to action is what may be called “integrative authorization.” Any commitment raises an integrative problem within the system since there are competing claims for use of the inherently limited facilities controlled by the pattern-maintenance unit. Adjudication of these claims is essentially an integrative function; a social system analogue is the legal system. On the personality level, I think it reasonable to suggest that reference of proposed actions to the ego ideal is the kind of process involved.

The reverse flow, from pattern-maintenance to integrative units, is easier to characterize; it may be called the conditional commitment to implementation. Essentially, this means that integrative authorization has no practical significance unless there is a sufficient probability that the authorized actions will in fact be carried out on the response level. In other words, the control authorized must be genuine, not spurious. Various psychosomatic phenomena, most obviously hysterical paralysis, illustrate the failure of such control.

On grounds of general theory we may state, but again will not be able to demonstrate here [cf. 24, chap. 5], that the pattern-maintenance system has a dual set of functions. (Cf. under Psychological systems, earlier.) The first set, which includes selection of response and integrative authorization, falls broadly under the heading of “tension-manage-

⁴⁷ On the social system level, this interchange is cognate with the input of consumers' goods from the economy to consuming units—preeminently the household—and the corresponding input of labor services to the economy. [Cf. 27, Chap. 2.]

ment." These functions control tendencies to "act out" directly through the command of response facilities within and outside the organism. But the other set, which is relative to the pattern-maintenance function in the narrower sense, has a special relation to maintaining the internalized value system, and hence to legitimating the functions carried out by the other subsystems of the personality.

This second function is paramount in the integrative interchange between the pattern-maintenance unit and the goal unit. The essential function of the pattern-maintenance unit here is *legitimizing* the selection of a particular *goal* at a particular time by referring it to the internalized values of the psychological system. It gives the go-ahead or authorization signal to the goal-commitment unit. But legitimation is not given "for nothing"; the essential condition, i.e., the input from the goal unit, is what we may call motive-force commitment, i.e., a commitment for the transmission of sufficient motive force to the other units of the system so that *they* can be motivated to perform their functions in the implementation of the goal. The goal unit is, we suggest, the main locus of the input of motivation from outside the system in the form of goal gratification. But it is not merely an input channel, but also an agency of distribution; in fact, it does distribute from its "reservoir" but recoups the loss when the goal state has been reached [cf. 20, chap. 4]. The commitment of motive force by the pattern-maintenance unit is a "token"; it is not the main flow which goes to the integrative and the adaptive units. But it is "assurance" that there will be enough motive force distributed to produce sufficient "effort" to attain the goal.

There is, finally, one further integrative interchange, that between the adaptive and the integrative units of the system.⁴⁸ It gives the impetus of adaptive, i.e., primarily cognitive, innovation as an input into the adaptive unit and in exchange produces new "ideas." This interchange may be the focus of "creativity" so far as it is internal to the psychological system.

To conclude this section, there should be brief comment on the dynamics of process through time. Only in very short time perspectives or at very high levels of abstraction do many important psychological processes appear to be phase movements, namely, stages through which a process goes in the course of completion of a sector of behavioral process.

We have emphasized above our interpretation of goal-striving as the result of a disturbance in the relation between a system of action and its situation. With respect to any particular goal state there may be a typical phase cycle beginning with state of latency (when, for internal or external reasons or both, the motive in question is quiescent), moving to-

⁴⁸This is cognate with the interchange of "entrepreneurial service" as an input into the economy with new combinations of goods and services as its output.

ward an adaptive-preparatory phase, then from an instrumental phase (of active striving) to a consummatory phase, then to an integrative one, and finally back to latency. The determination of the number of phases and placing of dividing points may be arbitrary, but the main outline is clear.⁴⁹

These phase movements for whole systems can in turn be broken down into interlocking phases for subsystems. Since Olds's analysis is the most illuminating I know for psychological systems, I summarize some main points. A unit, or as he says on the S-R-S level, a "concept," has four phases: "latent," "conceptual" (adaptive), "perceptual" (neurological, "firing"), (instrumental goal-attaining), and "refractory" (integrative). The first step in bringing a motivational system out of latency is possibly through external stimulation (CS) of the adaptive unit of the system. The adaptive unit transmits stimulation to its successor, the goal unit, as the process continues and the adaptive unit comes into the perceptual phase; this is sufficient to conceptualize the goal unit. During this phase which, for the system, is the adaptive phase there is active interchange between these two units. As the system passes over into the instrumental goal-attainment phase, the adaptive unit tends to become refractory and the goal unit to pass over into the perceptual stage with actual presentation of the goal object (US). After consummation, the goal unit in turn becomes refractory and the system as a whole goes into an integrative phase, etc.⁵⁰

The essential point is that a system phase cycle is in one respect the resultant of several different but *synchronized* phase patterns of its units. In general, a unit reaches its consummatory goal-attainment phase when its special functional problem has highest strategic importance for

⁴⁹This was first worked out for small groups by Bales and Strodtbeck. It is fully reported in [24, especially Chaps. 4 and 5]. Olds has applied it to the S-R-S psychological system [20, Chap. 4].

⁵⁰Two types of factor may account for what Olds calls refractoriness, i.e., spontaneous cessation of consummatory activity on the part of the system or a unit of it. Some kind of intrinsic "satiation" process may set a limit. Stomach distention as a consequence of eating would be an example. The negative stimulation from this state will surely eventually outweigh the enjoyment from continued eating. The second factor, however, is the demand of *other* motives in the psychological system for gratification opportunities. We presume that giving the first motive its opportunity has necessitated holding back others which displayed some "tension." The longer this inhibition lasts presumably the greater the tension, and this forces cessation of one type of gratification to make way for others, including the instrumental activities necessary to attain them. The importance on theoretical grounds of the principle of inertia should predispose us in favor of the second line of explanation. It is always easy to relieve theoretical embarrassments by making *ad hoc* assumptions about intrinsic satiation factors—but these assumptions are generally to be regarded with considerable suspicion.

the system as a whole, e.g., the adaptive unit is in the "perceptual" stage during the adaptive phase of the system, preparatory to commitments.

The preceding review of the internal boundary interchanges between functionally differentiated units of a psychological system is even more tentative than other parts of this essay. It represents a *first* attempt to adapt a paradigm which has been worked out in the analysis of social system materials to psychological systems. As such it is certainly exceedingly crude. Yet it does seem to strike at a number of the critical problems in psychological theory and to illuminate them in certain respects. It seems to me at least to "make psychological sense." If this first attempt accomplishes only this, experience suggests that sufficient further careful work will produce far better results. Unfortunately this takes time and no results of such work can be presented at present.⁵¹

A Summary of Psychological Problems

As we have noted, our analysis of the structure and functioning of psychological systems is necessarily crude at this stage. Careful attention to our paradigm for action systems will show that the analysis made here is systematic. But since it is couched in terms unfamiliar to most psychologists (particularly in regard to the pattern of analysis in which those terms are used), it may be useful to offer a brief summary of its relevance to a few of the most familiar problems of psychology.

First, we may note that we make a clear distinction between a psychological system and the systems external to it. We provide a framework in which such concepts as stimulus and response can be interpreted in terms of processes at the boundary of the psychological system: stimuli are sources of input into the psychological system, responses, consequences of a process of output from it. There is, further, a basis for the discrimination of "conditioned" stimuli, or cues, and "unconditioned" stimuli, or the presentation of goal objects.⁵² Similarly there is a basis for the distinction between instrumental responses and goal responses.

⁵¹ It might be appropriate here to adapt the famous remark of Dr. Johnson comparing the woman preacher and the dog who walked on his hind legs: the remarkable thing was not how *well* the thing was done but the fact that it was done *at all*. That a paradigm worked out in connection with sociological problems could apply *at all* and make any sort of sense in the psychological field is the remarkable thing. If it can be done, in time it probably can be done much better. In my own experience, in another connection, at first it seemed remarkable that a general sociological analysis could apply to the boundaries and internal processes of the economy at all, but a good deal of work has produced a relatively satisfactory fit in considerable detail, as documented in [27].

⁵² This must, of course, be treated as a *relative* distinction if we mean by an unconditioned stimulus the presentation of a goal object, since it is to us a cardinal tenet that most action goals have been learned.

Our treatment of the boundaries of the psychological system presents three features which may be called somewhat unconventional from the standpoint of most current psychological theory. First is the analytical distinction between psychological system and organism. From this point of view, stimuli (as inputs into the psychological system) are not events in the environment of the organism, but the neural and other processes *in* the organism which provide information to the psychological system (it seems improbable that any stimulus influence can reach the psychological system except through organic sensory mechanisms). Similarly, what psychologists ordinarily classify as responses are not strictly speaking psychological processes but physiological *consequences* of psychological processes, controlled by the latter. We like Olds's reference to a "response-control unit" as part of the psychological system—it is *control* which is distinctively psychological. On the organic side, the location of the closest interpenetration of psychological and organic systems should be not the skeletal-muscular system but the brain.

The second unconventional feature is the emphasis on the importance of the *social* object system in goal orientation. We recognize that truly psychological processes operate where the essential objects are purely physical, e.g., food objects. But we hold that for our general theoretical purposes this is a special case; the higher levels of organization of motivational systems involve the cathexis of social objects, not only as "individuals" but in social systems. Without the conception of identification and of the internalization of social object systems, we do not see how it is possible to account for the higher-level human organization of personality. We think we are authentically following Freud in this respect.

The third unconventional feature, if it can be called such, is the analytical isolation of the relations of the psychological to the *cultural* system and its discrimination from the social object system. We felt, for instance, that this could help account for the importance of the distinction between ego and superego in Freud's later theory. We realize, of course, that these last two problems, of social object system and culture, do not arise at the simpler levels of S-R-S theory. But again we maintain that, seen in a larger perspective of psychological theory, the latter deals with a group of special cases. Our scheme is quite capable of dealing with the S-R-S case by suppressing certain ranges of variation with respect to the organization of object systems and correspondingly with reference to levels of generality of meaning.

Turning now to the internal aspects of psychological systems, by the combination of a postulated (though avowedly schematic) differentiation of the units of a system, and analysis of the input-output relations between subsystems, we think we have found a place for dealing with a number of other traditional psychological problems. First, we have discriminated between the processes of consummatory goal-response com-

mitment and those governing adaptive and instrumental behavior preparatory to goal gratification. Here we follow Olds in the conception that the goal unit of a system has a higher level of motive force, whereas the adaptive unit has a lower level. It is in certain ways justifiable to refer to the former as a "motive" and the latter as an "idea," but we agree that this is a question of relative primacy; motives also have cognitive elements, and ideas are also motivated.

Above all we think we can account for the apparent teleological or "purposive" property of behavior in terms of the feedback relations between stimulation (expectancy) and motive force as outputs of these two units of the system to each other. The phases of an action process may be treated as coordinated but differentiated phases in the input-output relations between these different parts of the system.

Although considerations such as these can account for many of the mechanisms involved in a single stimulus-response sequence, they cannot account for selective and integrative processes in more complex systems which have a multiplicity of such subsystems in their repertoire. Here the integrative and pattern-maintenance units, in their interaction with the other two and with each other, are useful. The selection of cue meanings in terms of their integrative significance in the system as a whole, the storage and organization of information, and the selective legitimation of responses all involve the functions of these units; in a very broad way we have tried to show how the internal input-output processes can account for these features of more complex behavioral systems.

Most generally we can say that the integrative unit consists in a system of internalized object systems, in the more complex cases of a social character. Their cathectic or motivational significance is primary from this standpoint. Through the process of internalization their previous need-gratifying power as external objects has also become internalized, so that there is an organized internal reward system which operates as the primary regulating mechanism for the adjustment of internal strains and conflicts.

The pattern-maintenance unit, on the other hand, consists mainly of internalized cultural pattern components in which values play the central part, but it is also the primary repository of organized and codified knowledge. Thus, on the crude level of this discussion, we account for the essential functions of memory and of internal normative controls.

Some Levels of Organization of Psychological Systems

Until this point, it has seemed advisable to speak of psychological systems in general terms rather than attempt to discriminate different types of systems on any basis other than that of the relative primacies of functional components in the value systems and in input-output interchanges. Effort to carry through our whole analysis for each of several

levels is not possible in the allotted space, yet confining it to one such level, the personality, for example, would unduly restrict generality, the demonstration of which is so important an aspect of our theoretical intention.

Let us, therefore, attempt at least to put the problem into perspective by briefly discriminating a few levels of organization. It should be clear that this basis of classifying systems cuts across the functional types defined in terms of value-component primacies which were outlined earlier.

It is possible to deal with a range which extends from the adult human personality, treated as a total psychological system, to the simplest S-R-S unit system involving a single simple behavioral goal, a simple response pattern, and a very simple conditioned stimulus and drive situation.

Let us start with an S-R-S unit. Most important for present purposes is that, as Olds has demonstrated, it can fruitfully be treated as a psychological *system* in the fullest sense of the theory of action. Olds treats it as a system of three units (the number may or may not be arbitrary). With a plurality of units (whatever their number) their *interaction* (on the intervening variable level) is an essential focus of analysis for process in the system. Secondly, in this case there is a particularly close interpenetration between psychological system and organism in that there is a psychological unit for each discriminable component of observable physical behavior. This definitely cannot be said of higher-order psychological systems, where the interpenetration must center on the central control mechanisms of the organism, many of which probably do not have, in the psychologically relevant processes, important physiological functions.⁵³

Even in animal behavior, it seems certain that the personality is not just an aggregate of elementary S-R-S units but that higher-level organizations of the kinds Olds refers to as object systems and temporal systems come into play. To take one example, we speak of a "hunger" motive. But most animals will accept as food not just one class of physical objects but a variety of objects which resemble each other very little physically. When the animal is hungry, it does not seek only an object of a particular physical class but "something to eat." Hence, there must be a "food complex" which organizes several different classes by their functional similarity, rendering them substitutable for each other within limits. Therefore, the animal cannot react only to cues which indicate the availability of particular physical goal objects, but must be able to react to the availability of food as such. This category entails a fairly

⁵³ Thus, it seems probable that highly abstract thinking, such as is involved in mathematical reasoning, does not have any palpable physiological function for the particular organism, e.g., cessation of it would not interfere with "health." But it certainly involves complex neurological processes. Here the brain may be functional to the personality rather than vice versa.

high level of generalization since it has many diverse instances. For this reason, we may suggest that the food complex constitutes an object system organized about a physiologically given need but containing a complex network of learned properties about food objects which has become stored in organized form in the animal's "memory."

Similar considerations apply to what Olds calls "temporal systems,"⁵⁴ namely, complex chains of instrumental and goal-oriented relationships between which there is often a relatively high degree of contingency, so that with the failure of one set of expectations (for example the prey gets away and the food quest must begin anew) another instrumental sequence can be set into motion. This sensitivity to a range of alternatives is one of the paramount characteristics of "intelligent" behavior.

We suggest that what Olds calls (internalized) object systems in the primarily cognitive sense are located mainly in the pattern-maintenance subsystem of the psychological system. The primary criterion of their integration is the *consistency* of pattern elements with each other. Behaviorally, this consistency is observable in a low degree of contingency in moving from one "aspect" of the system to another, and by the reversibility of relations. Temporal systems, on the other hand, are, as internalized, located primarily in the integrative subsystem. They are the internalized counterpart of the system's adaptability to changing circumstances, with a pluralism of instrumental alternatives and of particularized goals integrated into a system.

As noted, it is a paramount feature of social objects that because of the property of double contingency, they tend to share the characteristics *both* of object systems in the narrower physical sense and of temporal systems. This feature certainly is associated with the fact that their internalization constitutes a primary feature of the attainment of the higher social cultural level of psychological organization. In other words, the basis on which the motives for these rewards can be brought under control within a single organized system is the diffuse cathexis of a social object, for the child ordinarily first the mother, which can include a multiplicity of more specific rewards. Hence the very young, the oral, child does not yet differentiate in its internalized organization between these systems. We have suggested that only after the oedipal period when the superego is formed is there a clear distinction between an object system, the cultural norms of the superego, and a temporal system, the internalized social objects of the ego.

We have said repeatedly that all systems of action seem to be organized in hierarchies of control. The above considerations suggest that in psychological systems—and probably in others as well—there is a pattern of alternating "layers" of system types in this hierarchy. One type is exemplified by the elementary S-R-S system. Its primary characteristic

⁵⁴These seem to be very similar to Murray's "proceedings" as Olds recognizes.

is the functional predominance of adjustment of the system to the *external* situation through adaptive and goal-oriented activity. This type may, following Olds, be called a "motivational system."⁵⁵

The second type of system is organized primarily about the pattern-maintenance and integrative units. Its focus is the system of internalized objects in the widest sense, which includes both high- and low-contingency types; we may refer to it as the internalized "object pattern" system.

Next it seems clear that these two types of system constitute alternating layers within a larger system, e.g., a personality, which are not concretely distinct, but *interpenetrate*. The principle of control hierarchies makes clear the nature of their interpenetration: the *A* and *G* components of a motivational system are articulated with the next *lower* pair of *L* and *I* components in the series to form a system. This is to say that at any given point in time the internalized objects of previous phases of learning underlie and regulate the operation of motivational systems. For object-pattern systems on the other hand, the relevant *A* and *G* components are the ones "below" the relevant *L* and *I* components in the series; i.e., the necessary situational adjustments to *maintain* an *L-I* system are carried out through the next lower-order motivational-behavioral mechanism system. An example of such a subsystem would be concern with relatively secondary "operational" reference within a personality primarily concerned with abstract thinking (as in the case of mathematical work).

Given this mode of interpenetration, it is possible to arrange the subsystems of a more complex psychological system in a hierarchical order. In our technical notation they appear as follows:

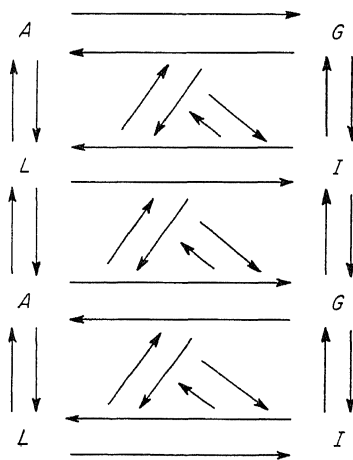


DIAGRAM 3

⁵⁵ In the unpublished working paper referred to in note ¹, above.

So far as we are able to ascertain, all the interchanges between functional units thus discriminated are empirically significant.⁵⁶

It should be noted that there are also "lateral" relations of interpenetration and boundary interchange between subsystems. The most obvious of these is the S-R-S or the "means-end chain," in terms of which a proximate goal state is an instrumentality for a further and more remote goal, etc. The selective feature of the organization of such chains, which crisscross each other in complicated ways, must, we hypothesize, be accounted for by their regulation in terms of the *L-I* subsystems which lie above and below them in the hierarchical order. Clearly also the *L-I* components of subsystems are integrated together in larger integrates.

Finally, it may be noted that the principle on which the hierarchical aspect of the structure of psychological systems is organized has much to do with *levels of generality* in the cultural pattern content of the internalized objects. The most familiar case is cognitive "control" of diverse empirical data by bringing them under a generalized theory from which particulars can be "derived." With proper consideration of the motivational component, we believe that this is a prototype of the organization of systems of action.

Now let us take up very briefly another trend of differentiation of the more macroscopic subsystems of psychological systems which is found only in the human personality. We have already discussed what we believe to be the "tendency" for psychological systems to differentiate on functional lines. We suggest that this occurs in the human personality in a rather special sense because of the conditions of early socialization.

According to our model the socialization of the human child ideally results first in the establishment of a personality system primarily organized about an internalized social object (the oral mother) and then differentiated (on *this* level of organization, not the S-R-S or even nonsocial object-system levels) into what may be called four *primary* need-dispositions. The focus of this differentiation is the internalization of the four primary role categories of the nuclear family, differentiated from each other on the two axes of generation and sex. The child's own role, of course, is ascribed in terms of his belonging to the filial generation, and to one or the other sex, but in his own personality all four basic roles, i.e., son, daughter, father, mother, are internalized as social objects and constitute the main scaffolding of the personality system at this stage.

As applying to the oedipal personality we have referred to these need-dispositions as (1) adequacy (internalized son-brother), (2) security

⁵⁶ This statement has been tested through a good deal of substantive work dealing with the "stratification" of the personality as resulting from the phases of the socialization process. It has not, however, as yet proceeded far enough to justify reporting on here.

(internalized daughter-sister), (3) nurturance (internalized mother), and (4) conformity (internalized father) [cf. 26, chap. 2]. Ego's self is categorized of course in terms of either the son-brother or the daughter-sister role. But the difference is a matter of primacy, and all four of the objects are internalized in personalities of both sexes.

These four primary need-dispositions *originate* in role behavior within the nuclear family. With the child's post-oedipal "emancipation" from the nuclear family the requisite objects are "lost," but as the scaffolding of personality structure, they persist throughout life. This suggests that raising the child, through oral dependency, first to the level of an internalized *social* object system, and then differentiating this object system along primary functional lines, provides the indispensable basis for a personality which can give primacy to *cultural-level* organization of behavior. Whatever the limitations in the neurological structure of sub-human species, it seems clear that the absence (or rudimentary character) of socialization experience gives such primacy to the adaptive-gratificatory exigencies determined by the nonsocial environment that it is never possible to attain a human level of organization. The animal must give too heavy primacy to "motivational" systems; he has no chance to develop sufficiently complex and highly organized object-pattern systems.⁵⁷

After the establishment of the primary need-disposition structure, which persists only in modified form, the child must develop a new and crosscutting type of psychological system; this may be called a role-orientation system. Of course, the need-dispositions originate as role-orientation systems, but these roles are superseded. What I am discussing now is the role structure of the adult personality.

The first point to be made about adult role-orientation subsystems, e.g., the occupational role, is that they involve *all* the primary need-dispositions. The primary need-dispositions are learned subsystems of the personality, the goals of which are not constitutionally given—except in the sense of some sort of "potentiality." The role-orientation systems are learned at a later time on a crisscrossing basis; hence they are removed from a constitutional base by at least *two* major steps not merely of elementary learning but of profound systemic reorganization of the whole personality system. Any attempt to treat the "motivation" of activity in such roles as a simple manifestation of a "propensity of human nature," meaning by that a constitutionally given "drive" or "instinct," is grossly unsatisfactory. For not one but several layers of internalized objects operate between the constitution of the organism and the control of

⁵⁷ Because of limitation of space this sketch is extremely schematic. Much fuller discussion will be found in [25, Chaps. 2, 3, 7] and in "Social Structure and the Development of Personality," *op. cit.*

overt behavior. Motives themselves, and the sanctions to which they are sensitive both externally, from alters in the situation, and internally, from other subsystems of the personality, possess a degree of generality in meaning which cannot in any sense be found in instincts. Neither can such motivation complexes be interpreted as mere aggregates of conditioned reflexes or even of "habits," in the atomistic S-R-S sense. They are *highly organized* systems.

Let us illustrate briefly with one example, the motivation of adult occupational activity. This is simultaneously a role in a nesting series of several different social systems. In the typical modern case, the focal one is the *organization* in which the individual is employed, e.g., a business firm, a government agency, or a university, which in turn serves functions in larger social systems. By virtue of his contract of employment, ego is obligated to perform a whole complex of services to the organization, often breaking down into very diverse subcategories with, however, some kind of primacy relationship. Thus, for a university faculty member teaching and research—themselves very complex activities—usually have first place, but he is also a member of the faculty and the department and has some responsibilities for their conduct as organized bodies.

A crucial characteristic of most occupations is that the "product" is not typically utilized directly by the producer: the teacher only secondarily teaches himself; the shoe worker wears very few of the shoes he helps to produce. Hence there must be some other motivational mechanism than in simpler cases is provided by the immediate goal value of the outcome of his activities. To account for this we must distinguish external rewards dependent on the situation—typically sanctions—from internal rewards. In the first class there are typically two essential subcategories. One is "remuneration," e.g., money as a generalized medium of exchange with which particular goods and services can be acquired. Remuneration establishes a relation to "intrinsically" significant goal objects, but through a highly generalized and peculiar mechanism very different from what is found in most rat-running experiments. Other "tangible advantages" may also be gained, such as prospects of promotion to more remunerative jobs in the future, various kinds of power or influence, etc. The second class of sanction rewards may be called symbolic and attitudinal; these are valued not for what they "do for" the actor as goal objects but for "what they mean." Money remuneration itself has a symbolic aspect as a measure of the regard in which a person is held, of his "success." But beyond this, various kinds of approval, recognition, honor, etc., must also be taken into account.

Turning to the internal rewards, the most essential point is the relevance of internalized values to the motivation of such activity. It seems

to be well established that the motivation to types of occupational achievement which involve highly intangible, not in the cruder senses "consumable," results cannot be understood without reference to the internalization of values of work and of achievement through the formation of ego ideals and superegos.⁵⁸

The reward system for sophisticated human achievements must quite clearly be organized on highly generalized cultural and symbolic levels. We do not yet have an adequate technical analysis of these things, or a terminology, to use in discussing them. But to revert to common-sense terminology, terms like achievement, success, wealth, power, fame are indispensable, as are, on the internal side, concepts like self-respect. A psychological theory which cannot interpret the common-sense meanings of concepts like these except to "debunk" them cannot hope to approach the analysis of the mature human personality and its linkages with the social and cultural systems.

Structural Change in Psychological Systems

Whether inevitably or no, the type of theoretical analysis set forth in this essay seems naturally to *start* with the presentation of the structure of a system and the processes by which a system with that kind of structure "functions" in relation to its environment. But "structures" do not remain unchanged. They come into being through some kind of developmental process, and they pass away and are replaced by other structures. Our approach in no way implies that structure or equilibration is empirically more important than change. But it will help give perspective to discuss the approach of a theory of action to problems of structural change.

First, on the most general level of theory there is no difference between a theory of the equilibrating processes of systems and a theory of their processes of change. A theoretical system in the analytical sense is not a set of empirical generalizations; it is a system of concepts and their logical interrelations in generalized propositions at various levels. "Theories" of equilibrium and theories of change are *applications* of generalized analytical theory. The differences between them are parametric, not theoretical.

The crucial parametric differences concern the stability of the "structure of the system," which we have associated with the internalized value

⁵⁸ On the psychological aspects of this compare [19] especially. On the sociological aspect a particularly famous case study is that of Max Weber, *The Protestant Ethic and the Spirit of Capitalism*. I have concerned myself with these problems from time to time over a long period. For a relatively early discussion see "The Motivation of Economic Activities" [23]. The most recent discussion will be found in [27, Chap. 3].

system. This stability is a function of various other features of the internal character of the system, e.g., its level of integration. It is also very particularly a function of the relation between the system and the ranges of variation of its significant situational conditions. Every empirical conception of equilibrium must include some conception of the limits with respect to the essential inputs which can be tolerated by the system. For example, an organism has definite limits with respect to nutritional intake, oxygen, etc.

Three things can happen to a system, including a psychological system, when the input-output factors approach "dangerous" limits in relation to its environment. First is the manifestation of "strain," of "abnormal" functioning of some sort. This is always the initial consequence, and may become consolidated into a "pathological structure," i.e., one which is relatively stabilized in such a way as to cope with the specific sources of strain, but at some "cost" elsewhere.

The two other possibilities are more fundamental. One is dissolution of the system. For the organism this is death, but social systems can dissolve without either the physical or the psychological death of their members. Another is structural change which makes possible successful adaptation to environmental situations outside the previous limits of toleration. The three possibilities are dynamically related. In the process of change there is always "pathology"; there is always "a little" death in the sense that some old structures are eliminated. Short of complete dissolution there is always some structural change; it is difficult to imagine "pure" pathology over a long period without any structural change.

To us, learning and structural change in psychological systems are identical. In interpreting this statement, it is essential to be clear about the level of system definition and organization to which it applies. The human personality is continually learning new things, but this does not necessarily constitute in a relevant sense structural change in the personality as a system, but only in very subsidiary subsystems at a level removed by a good many steps from the personality as a whole. We can speak of someone as having "a very stable character" through a great deal of such change. At the opposite extreme, the basic stages in the psychosexual development of the individual, e.g., the oral, oedipal, and adolescent stages, mark fundamental structural changes in the personality as a system. These, too, are in the most general sense "learning processes," although it does not follow that what is usually called "learning theory" is necessarily adequate for their understanding.

In a very tentative way, we have sketched a model of the process of adaptive structural change in a psychological system; we believe that this model is of very general significance in the theory of action, at several psychological levels and in social and cultural systems as well. It will

be remembered that the model involves frustration of important goal interests, expression of the consequences of disturbance, diffuse supportive input from the environment, provision of new facilities, selective rewarding of new performances, and reinforcement by a systematically structured pattern of sanctions.⁵⁹

Work with this paradigm has not yet proceeded far enough to permit a competent judgment of its accuracy or of its generality over any wide range. It was to be expected, however, that in the general theory of action there should emerge a paradigm of structural change of a level of generality comparable to the phase pattern of performance processes worked out by Bales and others for the small group. Olds has shown how that paradigm applies broadly to elementary psychological systems. It also broadly fits the pattern of the trade cycle in economics. On theoretical grounds we argue that in a structural change cycle the order of phases should, from a certain point of view, be the *reverse* of that characteristic of a performance or equilibrating cycle. On the level on which we have worked, this turns out to be so.

Part III

SOME QUESTIONS OF METHODOLOGY AND OF THE SCIENTIFIC SIGNIFICANCE OF THE SYSTEM

In this concluding section, I shall try to bring together very briefly discussions of the topics formulated under rubrics {3} to {12} of the outline. Some of them have been dealt with earlier and in these cases I shall avoid repetition as much as possible.

Initial evidential grounds for assumptions of the system. Part of the answer to this question has been anticipated under Background Factors and Orienting Attitudes. In the particular case of this scheme it is necessary to distinguish evidential grounds for the general theory of action from those for its treatment of psychological systems. As has been noted, the most salient evidence for me was, in the first instance, sociological. Above all it came from the field of comparative study of institutional

⁵⁹ Cf. above. The model was presented by Parsons and Olds in [25, Chap. 4] and generalized by Parsons and Bales in chap. 7. In that context it dealt primarily with a single cycle in the process of socialization, but with the appropriate parametric modifications, we believe it would fit other levels of psychological change. One social system case has been worked out, in application to a structural change in the economy, illustrated by the separation of ownership and control in the American economy during the past fifty years. This is published in [27, Chap. 5] and also in *Explorations in Entrepreneurial History* [28] in a paper by Parsons and Smelser. Smelser is now engaged in testing it in a much more complex empirical case, the development of the British cotton textile industry, 1780-1840.

structures as undertaken by Max Weber, whose particular focus was on the relativity of the institutions and behavior patterns associated with the modern Western economic order. This concern led into psychological variables, since the economic doctrine of "self-interest," the point of departure of my technical concern with psychological theory, is partly based on psychological assumptions. In the theoretical categorization of the evidence concerning institutional structures, my starting point was the problem of classification of social roles, as they differ both among societies and among role-types in the same society. Above all, it was in defining the contrast between business and professional roles that the elements of the scheme of "pattern variables" took shape. (See above, under the discussion of Structure of the Theoretical System.) This scheme is relevant to a vast amount of empirical data in sociology.

On the psychological level, the main evidence comes from data on the motivation of role behavior and the psychological bases of behavior patterns manifested in different roles. Thus the facts mobilized from several directions by Freud, with relation to the superego, by G. H. Mead, with relation to "taking the role of the other" and by Durkheim with special reference to *anomie* constitute reference points of primary importance. Data concerning the internalization of values and of social objects as psychological phenomena first gave strong impetus to my theoretical thinking on psychological matters. Several other bodies of evidence, such as Weber's account of how religious ideas could influence behavior, seem to be congruent with these.

From this point my interest ramified into problems concerning the structure of personality, with reference to levels such as those dealt with by Murray in his analysis of needs, and into the genesis of personality in the process of socialization, where the facts mobilized by Freud and other psychoanalytically oriented writers, particularly perhaps Erikson, were salient. Only much later did specific empirical evidence from the more microscopic levels of psychological analysis, as distinguished from general interpretive views, become particularly important for my own theoretical development. Here my principal interest has been in codification, namely, looking for a fit between available evidence and the requirements of the general theory of action.

It is perhaps understandable that, for a sociologist in the initial stages of psychological interest, particularly concerned with relatively macroscopic sociological problems, the "data," i.e., the assertions of fact from psychology which are most relevant, have been of a relatively general character. To me, it is a central methodological point that a fact is, as noted earlier under Orienting Attitudes, a *statement* or proposition which has been empirically verified. It is stated in terms of a conceptual scheme and may be couched at any level of generality; of course, the

more general the statement the more difficult it may be to verify, though this is not always the case. Hence a statement of Freud that the super-ego "represents," i.e., internalizes, the parental function in the family of orientation I consider a statement of fact. The important point is that the "immediate data" level, and the appropriate "language" for describing such data, is *relative* to the scientific problem in hand. To the sociologist interested in certain psychological problems, a generalization of Freud's may be an immediate datum, though to Freud himself only materials derived from the observation of particular cases would constitute such data.

Construction of function forms. Three principal topics seem to belong under this heading: the problem of intervening variables, that of prediction, and that of models. The logic of the independent-intervening-dependent variable scheme is, of course, central to the structure of the outline. In this regard, the most important single point I wish to make is that it lies on a different, more empirical level from that of the main structure of the theory of action and its psychological subsystem which has been set forth earlier. The essential difference is that the independent-intervening-dependent variable scheme is concerned with the interpretation or prediction of a particular empirical event or class of such events. The general theory, on the other hand, is not restricted in this way, but is applicable to any empirical phenomena falling within its range. When, however, a specific empirical problem is tackled by the general theory, it can be cast in terms of the other scheme.

Ideally, the best procedure would be to deal with all the fundamental variables of the system simultaneously for the solution of any given empirical problem. Even in such a case, however, many empirically relevant considerations would have to be treated as given parameters of the problem. In other words, only one subsystem of the theory of action (or one boundary-interchange process between two subsystems) could probably be formally and explicitly handled in a single analytical procedure. If, therefore, the problem in hand were psychological, the organic, social, and cultural factors would have to be treated as given. Presumably the same is true of the main facts about psychological subsystems of the total personality other than the one directly under analysis. This, however, is by no means to say that the empirical systems appropriate to analysis in each of these theoretical terms must be treated as unrelated, a set of watertight compartments of the empirical world. On the contrary, they are most closely interdependent, and we have shown that specific analyses of the relations between them (e.g., between personality and social system in the area of "object-relations") are of the first importance and are theoretically quite feasible. The question is how such intersystem relations can be *technically* analyzed. The answer

seems to be that the processes of a given boundary interchange can *themselves* be treated as a system of action, using the same basic paradigm which has been used for intrasystem processes.⁶⁰

The kinds of restrictions on the generality of theoretical analysis which have just been noted unfortunately do not give the whole story. We are not yet in a position to treat all the variables of a system of action in terms of a set of simultaneous equations. Hence for the empirical solutions which we can reach in the present state of knowledge, it is necessary to select certain of these variables as independent and to hold the others constant for purposes of the particular problem, or at best, to introduce *ad hoc* qualifications of the implications of the assumption of constancy which we can derive from our incomplete knowledge of the interrelations of the selected variables with the others. Thus we are forced to use the independent-intervening-dependent variable scheme, but this is not the logical ideal *except* for handling particular empirical problems.

A second question is what variables are "intervening." The essential point here is that very likely most, if not all, of the variables employed in the theory of action are intervening variables for some types of systems, but these are not the same for all cases. To take one example, for many types of personality analysis, such as that attempted above, it seems that most of the processes of boundary interchange or communication between units internal to the system are not directly observable and hence must be treated as intervening variables. As independent and dependent variables, the observables must be found in behavior, which may be symbolic, including verbal. But the case is different for the social group. In the type of experimental study of small groups carried on by Bales and his associates (and others) the unit of the system is the member-in-role. Processes of communication between these member units constitute the principal empirical subject matter of direct observation.

This kind of difference provides an important source of strength for such a scheme as the theory of action. To be sure interpersonal communication in group behavior is not the same thing, empirically, as intrapsychic communication between units of the personality system. But if we are right that the *same variables* are involved, relations between the variables which are established from the study of interpersonal communication should, if sufficiently generalized, prove applicable to the processes of intrapsychic communication. It is necessary to be extremely careful to determine the parametric differences correctly, but this is not by any means inherently impossible. And I think it is very unlikely that

⁶⁰ This has been attempted, within the general social system framework, in [27, Chap. 3] for different subsystems of the society. Its extension to the interchanges of social and psychological systems seems altogether feasible.

any of the essential variables of the general theory of action must be treated as intervening variables for *all* types of system to which the theory is relevant.

The problem of prediction is very closely related to that of the use of the paradigm of independent-intervening-dependent variables. This is, to my mind, essentially a paradigm for formulating either a prediction (a hypothesis of course is an application of the logic of prediction) or an interpretation of a past event, which is logically speaking a retrospective prediction. Thus I take for granted that *one* of the primary tests of a theoretical scheme is its usefulness in prediction. But the generality of this usefulness is rooted in the levels of theoretical systematization which have been the main subject of this essay. If *theoretically* based, successful prediction is an application of general theory. But it is not the only such application; codification of factual knowledge is another conspicuous one, an important example of which is the systematization of morphological classifications, so important in the biological sciences.

Predictability is, furthermore, a valid test only under conditions which must, methodologically, be very carefully defined. To take one common example, the difficulties of predicting day after tomorrow's specific weather do not constitute an adequate test of the validity of the systematic theory of physics in the field of the behavior of gases.

It is an implication of the Whiteheadian view I have stated earlier that there is a presumption that, in any concrete set of phenomena, a plurality of theoretically defined systems may intersect. Any *one* such system can then serve as a basis of accurate and detailed prediction only when the data conform to certain specific methodological requirements. Whether or not these are controllable in practice, they must be the "experimental conditions" for observing the generalized uniformities formulated in the theory. When empirically such conditions are not given, then two recourses are open. First, it may be possible to attempt to achieve relatively precise predictability by treating specific phenomena as resultants of the operation of two or more theoretical principles, perhaps deriving from different theoretical systems. Second, one may use various techniques to discriminate between the effect of the variables relative to one level of theoretical analysis and some order of residual variance which can be more or less randomized for purposes of the particular analysis and tested for by statistical techniques.

One particular caution is necessary when the adjective "psychological" is applied to prediction. We have noted that any theoretically based prediction must be hedged by the possibility that variables or factors other than those included in the system being used for prediction may influence the result. As systems of behavior, psychological systems may be influenced by the independent operation of processes in physio-

logical and biochemical systems, and the usual methodological precautions would have to be taken to isolate the psychological system from these.

But this is not all. If we are correct in our view that psychological systems constitute one special class of systems defined in terms of the broader theory of action and that in general the other classes are involved in *all* concrete behavior, especially on human levels, then there is *never* a presumption that concrete behavioral processes can be adequately explained or predicted from purely psychological analysis unless the factors appropriate to social and cultural systems, as well as physiological factors, are adequately controlled. Furthermore the personality is a complex organized system with many subsystems differentiated from each other on various levels. Hence careful discrimination of type of system and of system level *within* the category of psychological systems, as well as discrimination of psychological from other systems, is necessary for valid prediction based on psychological theory.

I have no particularly clear set of views about the utility and role of models. The most general sense of the term model seems to be that of an "ideal type" of structure or process, arrived at by hypothetical reasoning from theoretical premises, which is then used, through comparison with empirical data, to analyze such data. In this meaning, model seems to be almost identical with theoretical scheme. And, if it is theory, and of course, good theory, I am in favor of it.

Apparently, the term model is used for conceptual schemes on many different levels of generality. For example, Tolman's "A Psychological Model" [22] comes very close to being a general statement of his theoretical position in psychology. Models seem to vary all the way from this level to formulations of very specific processes.

I wish to call attention to one danger in the use of models which has come into focus in connection with some work on the border line between economics and sociology [cf. 27]. This is the tendency to isolate a specific set of independent variables within a parametric setting, and then refuse to say anything about anything else except that the parameters must be treated as "given data." This is justified for certain limited purposes, but it often tends to engender a habit of mind by which an artificially sharp line is drawn between the problem area on which attention is focused, and related areas which both empirically and theoretically impinge upon it. It favors a special case of the fallacy of misplaced concreteness, in that it is easy to slip from the position of holding the factors involved in these data constant for *particular* methodological purposes, to the implicit assumption that they vary, if at all, only in ways which do not affect the internal structure of the model; i.e., their variations are assumed, usually implicitly, to be random.

Two striking examples of this tendency may be noted. First, although the more sophisticated modern economists have been careful to hold to the given data formula about many borderline problems of economics, often implicitly and sometimes explicitly (cf. the work of Lionel Robbins) they have tended to say that it is a condition of economic analysis that it should operate in a setting where there is *no* theoretically intelligible order on the border line of economic processes; i.e., the data are not merely given, but random. A second example, this time from psychology, lies in the fact that in much learning theory work, particularly that stemming from Hull, no specific analytical attention is paid to the structure of the stimulus situation; it is "given," usually set up by the experimenter in a form unalterable by the rat. The paradigm has been that of an animal pursuing a given goal, e.g., hunger-drive reduction in a given situation. It is never explicitly denied that, for example, human children are placed in complex situations of *interaction* with others (children and adults) where the structure of the situation is anything but given. Yet when Hullians have started to generalize beyond their favorite experimental situations, they have argued as though the variant structure of the situation did not matter.

I am quite aware that these dangers are not inevitably connected with "model-theorizing" and that neither all economists nor all Yale psychologists have, in this sense, misused their models. But this has happened often enough to make a call for caution appropriate. The important point is that every model must be conceived to have a *theoretically* ordered context; it is a product of the special abstraction of *part* of a range of theoretical relationships for specific purposes and must never be "reified."

By and large, the idea of model is on the periphery rather than in the center of my thinking about theory, though I have occasionally used it [e.g., 27, chap. 5]. For me, the idea of systematic theory is the central one and models are as likely to be harmful as useful if they are not carefully related to theoretical systems.

Mensurational and quantificational problems. On this point I take an essentially pragmatic position. I do not depreciate the importance of measurement or of the quantitative treatment of data secured by measurement. Their importance stands out as overwhelmingly great in almost all branches of science. At the same time, I definitely refuse to concede that knowledge which is not put in these terms is always "both meager and unsatisfactory."⁶¹ Such a doctrine rules out a very large part of biological science and in our discipline much of the use of clini-

⁶¹ The well-known statement of Lord Kelvin used as a motto for the Social Science Research Building at the University of Chicago.

cal observation and anthropological field work. I regard the common dogma that quantitative techniques alone have scientific value as similar to the old behavioristic dogma that data of verbal behavior were scientifically inadmissible because they were "subjective"; the quantification dogma seems destined for a similar fate.

A prime theoretical consideration here is that quantification makes sense only when concepts and their logical relations are well defined. For reasons inherent in the structure of logical systems, these definitions can never be exclusively quantitative; if they were, the state of any empirical system, however complex, could for every purpose be represented by a single figure. There is a "qualitative" component in any theoretical structure, which must be reflected in the categories in which data are classified and classes are related to each other. Similarly I would radically deny the dogma, popular in the early phase of the vogue of operationalism, that operations of measurement can, in their scientific significance, be reduced exclusively to the physical manipulations from which numbers emerge, with the implication that all theory is purely inductive generalization from the results the operations produce.

The first essential point is that quantification works from a qualitative base in the logical structure of theory; the latter can never be eliminated, though of course it changes. Second, this qualitative component of the theoretical base plays an essential part in determining observational (including measurement) operations; questions of the significance of the results of measurement, if not the concrete data themselves, never can shake free of this logical dependence on qualitative considerations.

Within this methodological framework, the degree of specificity of measurement and quantification is not a question of methodological principle but of their relative fruitfulness for particular purposes. This in turn will depend on a number of factors, such as the kind of data actually and prospectively available, the techniques of measurement available to secure them, and the relations of these techniques to other observational procedures, the kinds of analytical and processing procedures available for handling data, and above all, the state of the theoretical scheme and its relation to these other problems, which will determine what order of theoretical significance can be attributed to whatever quantitative generalizations can be formulated and empirically validated. All this makes it a very complex problem area.

I believe that there is, in a sound development of science, a general trend toward greater quantification, but that this is likely to be maximally fruitful, i.e., theoretically significant, after the requisite spadework on qualitative levels when there has evolved a well-articulated analytical scheme in terms of which operational problems of measure-

ment, notably defining *what* should be measured, can be stated. However, this is a matter of relative predominance and not of exclusive either-or. Both types of methodology will be found at all stages but with increasing prominence of the quantitative.

Furthermore, it has often been pointed out that there are circumstances where too great empirical precision (quantification is a form of precision) may be a positive hindrance to scientific advance. This is because such precision may distract attention from broader relationships, concentrating it on detail. Thus, had Newton had the data available to Einstein, but the mathematical-theoretical equipment he actually had, he well might never have reached any major synthesis, because the "Newtonian" theory would have been empirically inadequate, and he could not have attained the Einsteinian. I believe that this is a really serious problem in the action field. Over considerable ranges of it there is a sense in which we know too much; a very great problem is how to *select* among the available data in terms of their theoretical significance, as opposed to exploring the "fine" quantitative relationships which are still unknown in any sense [cf. 20].

I am also inclined to think there are prejudices to the effect that any theory which is at all general should be capable of being expressed in terms of some specific type of mathematics. The great model of course has been the differential calculus as the form used in Newtonian mechanics. This has played an enormous part in the history of science, but there may well have been an element of chance in its magnificent scientific utility. The logical tools now available are far richer than in Newton's time, and many different emphases are possible.

On the whole, I believe that the behavioral sciences are following a course of development not unlike that of the biological sciences but are now still in an earlier stage. Only relatively recently has quantification (and certainly higher mathematical analysis) begun to play an extremely prominent part in biology. Yet it is most emphatically false that all the preceding qualitative work on the classification of species, on anatomy and histology, on "either-or" experiments, etc., has been in vain. Even in physiology and biochemistry an immense amount of the most significant work has been qualitative, e.g., simply *identifying* by qualitative chemical analysis the specific chemical compounds present in different physiological processes, and then perhaps resorting to the crudest possible quantification, e.g., in terms of the consequences of presence vs. absence.

I believe that the biologists' task has been easier than ours because of the immense range of organic species available for observation and comparison, and the clear empirical differences of their crudely ob-

servable properties, as well as because of the greater opportunity for controlled experiment. There is a sense in which the basic classificatory schemes of biology have emerged from relatively "obvious" groupings of data, in a sense in which this has not been possible for the behavioral sciences.

Thus very broadly, I think that the theory of action is in a state not very far from but in several respects still behind that of biological theory, at about the stage when really "modern" physiology appeared. The primary systematization we have so far achieved deals with "analytically descriptive" classifications of structures—structures of roles, collectivities, and institutions, or of need systems, which are often complex hierarchies of structures. Beyond this, some "structural-functional" analysis of processes within and between such structures in terms of the "mechanisms" by which certain functional "needs" of such systems are met has proved possible. The importance of this level of systematization should not be underestimated. Among other things, it has provided a framework within which the theoretical significance of more detailed empirical problems could be formulated and a framework for codification whereby structures and processes of different types of systems could be rendered comparable.

There is, of course, an immense amount of quantitative information available in our fields, but it is not yet very highly codified or adequately related to larger theoretical analyses. This as much as anything delays the much-desired cumulative development of knowledge, since cumulation in the scientific sense is by no means a simple matter of increase of *numbers* of facts known; it involves *organization* of the facts, which can only take place through generalized theory.

The methodological ideal is that the specific data obtained by observational procedures should turn out to be the values of the variables of a generalized theoretical system; this was the case for part of the astronomical phase of classical mechanics and has been true in a few other cases. As yet, this ideal cannot be said to have been attained anywhere in the behavioral sciences. Economics alone has, for any length of time, possessed a theoretical system of the requisite logical integration and refinement, but repeated attempts to gather statistical data which provided the *direct* measures of the fundamental variables have failed. The basic reason seems to be clear; the "experimental conditions" necessary for success do not occur in "nature" any more than do certain of the chemical elements.

On the other hand, where generalizations based on quantitative data have been achieved and validated, so far no one has been able to fit them directly into any generalized theory. Probably this is because what

has been measured is some order of resultant phenomenon, involving usually not only several variables of the same system, but even more likely the intersections of plural theoretical systems. A demographic generalization such as Pearl's logistic curve or Stouffer's treatment of intervening opportunities would be cases in point [cf. 29, 33].

The version of the general theory of action presented here has taken a turn which may prove to be the harbinger of quantifications closer to the level of generalized theoretical significance than before. This is the statement, as attempted above, of the equilibrium conditions of systems in terms of the balances of inputs and outputs in relatively clearly defined categories over the boundaries of the system. In a logical sense, this is an inherently quantitative approach, since it involves for empirical application the distinction between an optimum input or output and a too much or too little which deviates from the optimum in one direction or the other.

On very general levels, this input-output analysis has already been applied to the problems of small-group equilibrium, to the genesis of deviant behavior in social systems, to phases of the socialization process, to the boundary processes of the economy, and in a highly tentative way above to the processes of psychological systems.

Formal organization of the system. Again, my attitude on this question is relatively pragmatic. One should distinguish between methods of actual substantive theoretical work and certain ways of stating results and subjecting them to certain types of test.

Methods of substantive theoretical work are complex and difficult to describe. I and my associates have generally focused on empirical-theoretical problems and problem areas, that is, problems which have both empirical and theoretical aspects. The empirical elements have appeared at several different levels both on the microscopic-macroscopic range and with respect to the character of the relevant data. Thus, to take the critical example discussed before, the problem of the nature and limitations of the economic conception of "self-interest" was studied in relation to the contrasting broad patterns of institutionalized role behavior in the business world and in that of the professions, with a range of comparative institutional structures outside our own society in the background. Theoretically, the study dealt with the relation between psychological and sociological problem statements, but on the whole it worked from the sociological into the psychological rather than vice versa. Another much more recent case would be the consideration of the learned and institutionalized elements of sex role, with facts drawn both from cross-cultural material on kinship and sex role outside the kinship realm and from the socialization process in the family. Again, the theoretical interest has been both psychological and sociological [cf. 22, 25].

Perhaps the most important generalization about our methods of work is emphasis upon the immense importance of theoretical work as such. A prejudice widely current in the culture of our branches of science asserts that all the "hard work" of science lies in the empirical fact-finding and validating process; once the facts are known their theoretical significance and implications will automatically spring to mind. This, I feel, is a very serious error. After all Galileo knew *all* the crucial facts needed to explain the limitations of operation of a suction pump in terms of atmospheric pressure, and he missed the explanation [cf. 6]. Obviously in matters of scientific theory, Galileo was not a stupid man. To see the significance of facts it may be necessary to have attained a theoretical perspective whereby this significance acquires saliency; and this is not likely to occur without effort.

Our theoretical work consists essentially in a laborious process of trying out possibilities of combinations of the available logical components of a theoretical scheme. It also investigates possibilities of modifying both these components and their relations, in the form, for instance, of the redefinition of fundamental concepts and the introduction of new components. The case of the concept of value cited in the introduction and used later is a conspicuous example.

For this purpose, various kinds of devices for formalization become very useful tools. On the level on which I have been working, "equations" are still too specific, and I have used what, logically, are cruder and more elementary procedures, especially relatively formalized classifications. There is always the danger of hypostatizing such tentative formalized statements, and some critics contend that the habit of using them necessarily introduces fatal rigidities into one's thinking. Yet, whatever the dangers, I do not see how the levels of theoretical specificity and generality we are aiming at and working with can be handled if in principle we confine ourselves to discursive exposition.

For these classificatory purposes, the fundamental starting point is the "fourfold table," namely, the simple cross classification of what in some sense are the "polar" or widely separated values of two variables. The general justification of using the logic of this procedure is related to the extremely wide usefulness of *binary* discriminations: the binomial theorem in mathematics, the "bit" theory in communication theory, various trends in symbolic logic, and substantively, empirical evidence such as the predominance of division into two in the biological processes of cell division.

Turning from the field of workaday procedures of theory construction, formalized deductive procedure acquires great significance as a test of validity and as a revealer of hidden premises and problems, at certain junctures in the development of a theoretical scheme. In my

opinion, any working theorist who *confined* himself to this type of procedure would soon cease to be a theorist and become either a logician or a philosopher. Nevertheless, as a completed structure, a theoretical system should be capable of statement in logico-deductive form, starting with axioms or postulates, and proceeding to definitions of variables, theorems, parametric constants, deductions from theorems, etc.

For the theory of action, an attempt on this level was made in "Values, Motives and Systems of Action" [22]; in retrospect, this seems to have been successful. It was far from a "definitive" statement; indeed, it revealed difficulties which have already led to substantial revisions. The attempt was distinctly useful, however, and showed, for the first time on a comparable level, the possibilities of strictly deductive development of such a general scheme. Many of its elements, notably the relational character of all action and the different system-formation reference points, I continue to adhere to; other features have been modified and are expected to be modified further.

In sum, the logico-deductive form is ideal for presenting a theoretical system when it is relatively complete. Attempts to achieve this form constitute one fundamentally important type of test of the state of a theory, along with the empirical test, the most important one. From time to time, it should, therefore, be seriously attempted. Yet deductive procedure in this sense is not the primary everyday method of the working theorist; he is likely to be spending most of his time on relatively specific problem-oriented levels, and thus like the empirical researcher, not worrying about the ultimate validity of what he is doing.

Finally, I do not think *any* logico-deductive statement of any theory is *ever* definitive in an absolute sense. As Whitehead made crystal clear with respect to the classical mechanics, when it came to be stated in sharply deductive form, it bristled with difficulties of which philosophers were well aware long before scientific advance began to deal with them effectively (e.g., the problem of "action at a distance" and the unresolved conflict between wave and corpuscular theories of light). The theory of action certainly involves many philosophical difficulties. It stands or falls, however, not on its meeting these difficulties, but on the contribution it makes to empirical science, relative and approximate as that always is.

Scope or range of application of the system. In view of the nature of the system the problem of scope should be put in two different ways: the scope of the general theory of action; and the scope of the theory of psychological systems which can be considered to be a subsystem of that general theory. I see no answer to the first question other than that the theory must be held to cover the whole range of the "sciences of behavior" as ordinarily defined or what have been a little more narrowly

called the "social sciences," so far as these sciences are fields of the application of systematic analytical theory.⁶²

As the sciences of behavior mature as sciences, they will not continue to be the province of a plurality of competing "schools" of theoretical interpretation, but will tend to converge on a logically integrated, but also highly differentiated, conceptual scheme. The perspectives from which the many different parts of such a scheme have been approached are and will continue to be many, so that the process of codification can be expected to be long and difficult. But the history of the work of codification with which I am familiar, leads me to believe that, again and again, what at one time have been considered competing and incompatible schools of theory in a special field will be shown to be special cases of a more general theory, each of which is fruitfully applicable within the range of its own limitations. Perhaps we are not yet ready over the range of behavior as a whole to promulgate the dictum of Schumpeter for economics, "There are no schools of economic theory; there is only good theory and bad theory" [31], but in my opinion this expresses the general trend of scientific development in the behavior area as in others. In interpreting this view it should be remembered as just noted, that no theory is ever definitive but is always destined to be superseded by a better theory; this does not usually mean that the older theory was "wrong," it means it was limited.

Anything like the general theory of action in its present state is clearly destined to be superseded in this sense. Furthermore, the process of codification between it and materials within its range which are not now explicitly stated in its terms, will not merely have to proceed much further than heretofore, but as codification proceeds, the statements both of the theory of action itself and of the other theories will in the nature of the case have to be modified. Granting all this as nearly obvious, I am not aware of any drastically different conceptual scheme, i.e., one fundamentally incompatible, which is on a comparable level of generality, seriously competitive in the current situation, and likely to supersede it in a sense other than that in which, by the general process of the evolution of theory, any scheme current at a given time always comes to be superseded.

A second point about range is very simply answered. Within any given system type, the theory covers the whole range of microscopic-macroscopic levels, for example, in the case of psychological systems

⁶² The main problem in this last connection is the status of history, which traditionally, for the most part, has not considered the development of systematic theory as its province though historians have, I think increasingly, begun to make use of analytical theory developed in the neighboring disciplines and many historians have made important contributions to theory.

from the S-R-S- system level to the total personality, in that of social systems from the experimental small group to the total society. This claim is fundamental to the whole status of the scheme; disproof of it would be extremely damaging.

The next question concerns the boundaries of the general theory of action. In terms of what are, at the same time, evolutionary "stages" and levels of the organization of behavior I think the extremes of the range of applicability can be defined with fair precision. At the "upper" limit, the important consideration touches the boundary between cultural systems "culture (II)" as *systems of action* and two types of "nonempirical" discipline. One such type has "existential" references but these are non-empirical. The obvious disciplines to include here are philosophy, at least in certain of its branches, and theology. The essential dividing line is given by the fact that the theory of action is a theory of empirical science. That it is not altogether independent of philosophical assumptions goes without saying, but this does not make it a philosophical theory. *All* theories of empirical science are dependent on philosophical assumptions, by virtue of the fact that all human knowledge is at some level a single organon.

The other class of disciplines is central to an aspect of "culture" which I think should be clearly distinguished from that of concern with action, namely, what are usually called the "formal" disciplines. These include among others, logic, mathematics, and the formal aspects of law. Here the essential concern is with the structure of *systems of meaning as such*, not their reference to the empirical phenomena of action nor to the "motivational" aspects of the circumstances of their use. Though, of course, linked with the theory of action in various important ways, they do not form part of its "province."

Psychologists have more direct interest in the "lower" limit of applicability of the theory of action; this concerns the relations between what I have called the "behavioral" organism and the rest of the concrete organism, summed up in the heading "vegetative organism," and certain related questions about the physical environment and its impact on processes of life.

As noted in the Introduction, the organic processes most closely associated with behavior are *organized* in relation to the structure of the object system of the external world; the more so the more "actively" the organism is engaged in "coping with" its environment rather than passively "adjusting to" it. This applies particularly to processes of distal perception, and to locomotor processes including grasping with jaws or limbs, etc. Proportionately to increasing dominance of such processes, what I have called the "meaning" of stimuli and of objects of re-

sponse becomes relatively more important than the physical and chemical properties of the objects with which the organism comes into contact.

This analysis has required distinguishing the two levels in the organization of the organism which I have called behavioral and "vegetative." I have also spoken of these two levels as interpenetrating in the technical sense of that term as used here. The theory of action must be conceived as including one aspect of what is usually called the biology of the organism, but it is not to be interpreted as including anything like general physiology, to say nothing of biochemistry. This view complements the view that psychology cannot be a mere branch of biology unless biology itself be conceived as the science of all life processes; in this event, sociology, anthropology, economics, and political science must also be treated as branches of biology.

The more general view of the relation of the evolutionary scale to levels of selection and organization put forward in the Introduction implies a view of the logical relations between the sciences focusing at these various levels. Physics and chemistry, sciences which are not, in their main subject matter, specific to life processes at all, are the most "general" in the sense that they analyze conditions which underlie *all* empirical phenomena on whatever level of organization. The higher levels of organization do not "suspend," to say nothing of "repeal," the laws of these extremely general sciences. The relation is rather that, through selection and organization, *special conditions* of the operation of these laws have been created which lead to "emergent" phenomena which are not general to the whole area covered by these sciences.⁶³

The cluster of sciences traditionally called "biological," then, seems to deal with a somewhat less general set of phenomena, namely, those aspects of life which are not clearly dominated by the salience of the processes of organized, especially socially interactive, behavior. In the sense in which physics and chemistry are general, I think it can be said that general biology is more general than the sciences dealing primarily with behavior. Therefore, it seems to me that a conceptual scheme which can include both general biology and the theory of action must be on a higher level of generality in this sense than is the theory of action. Action then becomes a special case of the phenomena of life, subsumed under such a more general category but having sufficiently distinctive properties so that a general science of life, as a

⁶³ Although my competence in the field is very limited, I have the impression that, from certain points of view, physics and chemistry are now treated as two broad types of "special case" of the same general theoretical system; their relation is comparable to that between the psychological and sociological levels of the theory of action.

theoretical system, is not adequate to such analysis, just as physics and chemistry, as such, are not adequate to the analysis of the phenomena of living organisms, which constitute a special case of the organization of "matter."

The limit of applicability of the theory of action may be defined, in the first instance, as concern with the aspects of life processes which are not mainly associated with behavior in relation to a structured situation of external objects. In the second instance, that limit may be defined as concern with the aspects of the environment which do not impinge on the organism primarily as a system of structured objects but rather as a set of, in one sense more diffuse, physicochemical "influences." Temperature would be a prototypical case; for physiological theory it is not an "object" in the action sense but an "influence."⁶⁴

In the light of the foregoing, the question of the limits of the psychological subtheory of the theory of action does not require extended discussion. In terms of the general hierarchy of organization and control the upper limit stands at the boundary between psychological systems on the one hand, social and cultural systems on the other. At the lower limit, it is the boundary vis-à-vis the organism.

There is something like a consensus both among psychologists and among their scientific neighbors that the central focus of the subject matter of *psychology is the behavior* of the individual organism. Those who are less analytically minded tend to include a good deal of "social behavior" and hence raise questions of the border line vis-à-vis sociology and anthropology. Here I would prefer to speak of "social psychology" as, at least in part, an interstitial discipline which deals with the boundary interchanges and the areas of interpenetration of psychological and social systems. Historically, social psychology derives from both sides, and I see no better reason for psychology or sociology to press an exclusive claim to it any more than for chemistry or biology to claim biochemistry. Similar considerations would apply at the other boundary, where I would speak of physiological psychology as a discipline inter-

⁶⁴ A further important problem of scope of relevance concerns the social system level of the theory of action. Here it is necessary only to reiterate what has been stated before, that the social system aspect of the theory of action must, on the present premises, be interpreted to include not only "sociology" but the traditional theoretically oriented social sciences, namely economics and political science. On this basis it is necessary to distinguish between a general theory of social systems and, on a lower level of generalization, the theory of specialized subsystems of societies, such as the economy, the polity, etc. The conception of sociology has been ambiguous in this respect. One tendency has been to define it as the general theory of the social system, the other as the theory of a special type of social system, that functionally concerned with integration of societies. This ambiguity unfortunately cannot be said to have been resolved.

stitial between psychological and organic systems and belonging exclusively neither to psychology nor to biology. In all these questions, of course, it should be clear that the focus is on the analytical system; empirically there is necessarily much overlap and interpenetration.

Finally, I may merely remark that I see no reason why the version of psychological theory which can be stated in terms of the theory of action should not cover the whole range of theoretical interests which can be called psychological in any specific sense. This is implicit in the statements made above about the general trend away from "schools" and toward unification of theory. This is not to say that all important psychological theory is at present stated in such terms. Rather, it is a prediction about the outcome of the process of codification which is under way. The prediction is that however present statements of the sort attempted in this essay may be changed in the course of codification, it is likely to produce a general scheme which, in its psychological aspects, will be recognizable as an intellectually "legitimate" descendant of what I have here called the theory of action.

Evidential status; prospective considerations. It seems to me that enough has already been said about the questions involved in rubrics {8} to {12}; further discursive treatment would be repetitious. Both the history of the system in mediating research and the state of currently available evidence have been touched upon, though of course in a fragmentary way; anything like full coverage would require a very extensive treatise. The point which is particularly important for this system as distinguished from others is the evidential value of the *interrelations* between work in the different fields to which the general theory is applicable, particularly psychological and nonpsychological. Perhaps I may illustrate with two examples of such generalization, one going each way, which have been mentioned earlier. The first example was the paradigm or "model" of a process of structural change in a system of action which Olds and I first worked out for personality change in a phase of the process of socialization and then applied (with the help of Smelser) in the analysis of a process of change in the structure of the American economy.⁶⁵ The second was the model for analysis of the boundary interchanges between the subsystems of a larger system of action. Smelser and I first worked out this paradigm in connection with the inputs into the economy from the other subsystems of the society and the corresponding outputs from the economy. Earlier in this essay I attempted to use this same paradigm to analyze both the inputs and outputs of psychological systems vis-à-vis other systems of action and the inputs and outputs of

⁶⁵ The model was worked out in *Family Socialization and Interaction Process* [25, Chap. 4] and generalized by Bales and me in Chap. 7. The economic version is presented in [27, Chap. 5].

the subsystems of a personality vis-à-vis each other. In both these cases, benefits seem to have been gained which would not have appeared possible without the conception of a general theory applying *both* to psychological and to nonpsychological systems. Thus, I think a very important category of evidence for the psychological version of the approach put forward here is its *fit* with the nonpsychological parts of the theory of action.

Within the range of the theory of action, the answer to the question raised concerning the value of methods, concepts, and principles of the system outside the context of the system itself, seems obvious, if outside the system is taken to mean outside its psychological aspect. A major focus of the system is, of course, the fundamental unity of the theory of action as a whole; hence the last thing which could be suggested in the present context is the isolation of its psychological version.

Programmaticity and strategy are very closely linked. Both can perhaps best be stated in personal terms. In this sense my own career has had a good deal of consistency of aim from an early stage although it has undergone a very marked development. The conception of a theoretical system as my major focus of interest first crystallized in the series of studies leading up to *The Structure of Social Action* [21]. At this stage, it was confined to the social system case; only in the course of the work in 1949–1950 which eventuated in *Toward A General Theory of Action* [26] did the idea of a *general* theory embracing not only social systems, but also psychological and cultural systems fully crystallize.

From that time on there has been a rather high level of programmaticity in systematic procedures of codification. My program of work has led successively into detailed explorations of the border line between social and psychological systems, then of the status of economic theory within the general theory of social systems, and more recently of political theory. There has also been less thorough work (as yet unpublished), particularly in collaboration with Dr. Clifford Geertz and others, on some aspects of cultural symbol systems in relation to the social system, with concentration on the symbolism of political ideologies.

My program attempts, piece by piece, to cover systematically all the main subareas relevant to the general theory of action. In so doing I have tried to work in terms of three major types of reference points. The first is the logical structure of the theoretical system itself and the strategically important bodies of empirical data which can already be considered to have been codified in relation to it—on a certain level of only relative satisfactoriness, of course. The second is the establishment of logical relations to other bodies of theoretical analysis which have grown up independently of at least what to me has been the central sociological core. In psychological areas, this has involved psychoanalytic

theory above all but also, to a lesser extent, some of experimental learning theory. It has very conspicuously involved economic theory and the far less fully codified theory available in the political field. The third major reference is to relatively well-established bodies of empirical data which careful analysis can place in a context of larger theoretical relevance by bringing them systematically within the purview of the theory of action.⁶⁶

I have given this type of exploratory codification procedure precedence over new empirical research. As I have noted, since immense labor is still needed in the way of selective ordering and interpretation of knowledge we already have, there is room for legitimate specialization in such work. This does not in the least derogate the importance of new empirical fact-finding, with the hope, of course, that this will fall in fields of strategic theoretical importance.

Let me close within this framework. I have deliberately called this essay an *approach* to psychological theory. It certainly cannot pretend to formulate anything like a *mature* theoretical system. But in the psychological as in the other branches of the theory of action, and with respect to the theory as a whole, it seems to me clear that we are working in and with a genuine theoretical system, crude and incomplete as the present stage of its development undoubtedly is. I underestimate neither the enormous difficulties which lie ahead in the effort to improve it nor the immense amount of work on the part of many people which it will require to overcome them. But in the sciences of human behavior, attainment of what I think the most essential of all the ingredients of a mature science, adequate systematic theory, is a goal close enough to be carefully and deliberately worked for, not in a fumbling but in a systematic way.

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APPENDIX: SUGGESTED DISCUSSION TOPICS FOR CONTRIBUTORS OF SYSTEMATIC ANALYSES¹

INTRODUCTION

We will use the term "systematic formulation" as any set of sentences formulated as a tool for ordering empirical knowledge with respect to some specifiable domain of events, or furthering the discovery of such knowledge. As is evident in science in general and psychology in particular, such formulations may vary in their characteristics over a very wide range. These variations may reflect differences in the intentions of the systematist, limits imposed by the nature of the subject matter, by the status of knowledge about it and related domains, by the availability of techniques for ordering the events in the domain, etc.

Defined in this sense, a "systematic formulation" may vary from one or a few orienting ideas towards the conduct of research, or towards the organization of extant knowledge within a given empirical domain (of any scope), to an explicit, elegant, and quantified systematization. Such highly diverse expressions as "viewpoint," "research philosophy," "Weltanschauung," "exploratory hypothesis" or set of such, "frame of reference," "dimensional system," "systematic (or "theoretical") framework," "explanatory (or "descriptive") system," "hypothetico-deductive system," "theory," "explanatory mechanism" (or set of such), "model," etc., may all be subsumed under "systematic formulation," as we wish to use this phrase.

This study is interested in the "systematic formulations" of present-day psychological science. Comparative analyses of "theory" and discussions of systematic methodology have considered far too narrow a range of formulations during the past few decades. We seek an inventory of current systematic resources which will adequately reflect the diversity and richness of conceptual experimentation of recent and present psychology. Only by the widest possible representation of formulations, with respect both to methodological type and em-

¹This is a copy of the document concerning the discussion themes and their significance, sent to all Study I contributors at the time of their invitation to participate.

pirical domain, can clear light be shed on problems that cut across various classes of "system." Only in this way can problems which are unique to given classes of "theory" be isolated, and interrelationship issues be treated justly and comprehensively.

This study begins with no value judgments with respect to some preferred mode of systematization, or even with respect to some preferred set of systematic aims or ideals. On the contrary, the only value judgment it makes is that issues of this order have tended, in recent decades, to be prejudged. Nor is it the intention of this study to end with such a set of value judgments. Our intentions are explicative, not evaluative, and our belief is that explication of the current systematic situation on a broadened and less stereotype-bound basis is as valuable to a rational determination of next steps on the part of systematists and research workers as it is to more effective pedagogy.

In this era of second and n -order self-study questionnaires and professional nose-counting, investigators whose mode of work is as essentially individualistic and inspirational as that of the systematist may understandably feel that there is a suggestion of the Philistine in any project which requires the answering of questions about their work. To this, we can only reply that among the intentions of this study are not eavesdropping on the creative process, the determination of excellence by ballot, or even the charting of "directions" by consensus. We believe merely that where we can go—no matter in how many different directions—is some function of where we are, and that the assessment of where we are can proceed perhaps a little more efficiently in the light of the information for which this study calls. The type of reflective re-analysis of one's position from a common incidence which this study seeks finds its precedent in such institutionalized channels as symposia, anthologies, handbooks, and the occasional journal issues which are devoted to a common theme.

RATIONALE OF THE DISCUSSION TOPICS

Explicit knowledge about the characteristics of the many and varied systematic formulations put forward in the history of science is in its infancy, but a reasonable amount of information exists about a few of the formulations in natural science (e.g., Newtonian mechanics, relativity theory) particularly distinguished for their generality, explicitness, "elegance," and success in mediating the organization of knowledge. It is highly unlikely that all "successful" systematic formulations in all fields of science exhibit all of the known properties—even in some degree—of the criterion formulations which have so

far been studied by methodologists. But it is probable that all formulations which realize in some measure (whether actually or potentially) such scientific objectives as "prediction," "understanding," or "control" exhibit at least some of these properties.

The discussion topics in the following outline have perforce been derived from the specifiable characteristics of the class of scientific systems which has so far received attention from methodologists of science. Nevertheless, we have no great confidence in the adequacy to psychology (and the biological and social sciences) of the generalizations about problems of empirical systematization made by methodologists of science. Whether systematizations of psychological data can be expected to conform to any large number of such characteristics is, of course, an entirely open question. Unfortunately, we do not as yet have a vocabulary, and a set of corresponding distinctions, which permits us to talk with precision about the widely varying characteristics of non-natural science systematic formulations. Given writers will therefore find that not all items will be equally relevant to their own systematic formulations, and some items will probably be entirely irrelevant. Depending on the nature of his system, the systematist must necessarily give differential attention and emphasis to certain of the items. He may also find it necessary to discuss the formulation with respect to characteristics not included in the outline.

Clearly, we are aiming for commensurability of treatment, but not blindly or rigidly so. Not only may individual writers find it necessary to omit certain of the items, but they may wish, in some cases, to re-interpret items in order to bring them to bear more precisely on the nature of the formulation under analysis, and they may wish to alter the order in which the various discussion topics are arranged. Despite such necessary variations of treatment, the procedure should result in a more commensurable airing of issues connected with systematic formulations than has hitherto been the case.

It would be meaningless to suggest any standard length for the manuscripts. Obviously, we should like to have sufficiently sustained consideration of the discussion topics to ensure clarity for a heterogeneous audience, and to derive maximum explicit benefit from the systematist's wisdom with respect to the problems at issue. On the other hand, we do not wish to burden the systematist with an overly laborious or time-consuming task. The purposes of the study will be adequately served by manuscripts which are as brief as is compatible with meaningful discussion of the outline rubrics.

We have tried to formulate the following list of discussion topics explicitly enough to ensure univocality of interpretation, yet at the

same time to avoid unconscionable discursiveness in our presentation. For reasons indicated above, we have used certain of the "standard" distinctions and terminological counters of the general methodology of science with a reluctance which has only given way because of the unavailability of any alternate vocabulary for talking, with general intelligibility, about systematic problems. If the authors of such distinctions have, in the past, applied them in such a way as to imply value judgments based on the degree of correspondence between the material under analysis and the analytic distinctions at hand, we can only regard this as a regrettable historical circumstance to which the results of the present study may conceivably supply the proper corrective. Indeed, a useful outcome of the present study might well be the aid it can give towards the development of a more meaningful way of talking about problems of psychological systematization.

THE THEMES OF DISCUSSION

1} **Background factors and orienting attitudes**

(a) Background factors which have influenced objectives, methods, and content of system.

(b) Orienting attitudes which have determined systematic objectives, methods, and conceptual content.

Explanation

"Background factors" would include, of course, such matters as education, influence of other theorists, general currents of thought within the field or the culture at large, previous research history, or any other genetic circumstance which the systematist deems noteworthy.

"Orienting attitudes" register those presystematic judgments, values, and beliefs which, in a relatively general and stable way, have determined the aims, inductive basis, conceptual content, or formal organization of the system. Examples might be the systematist's general commitments towards such issues as:

- a. the nature and limits of psychological prediction
- b. "level of analysis" at which it is fruitful to constitute explanatory constructs, with respect both to "ontological reference" (e.g., "purely behavioral," "physiological," "sociological?"), and "coarseness-fineness" of the "causal" or explanatory units
- c. utility and role of "models"
- d. comprehensiveness of empirical reference (in terms of some such continuum as "unrestricted generality of scope—extreme delimitation") towards which it is fruitful for a system to aim, in the present phase

- e. degree and mode of quantitative and mensurational specificity towards which it is desirable and/or feasible to aim
- f. type of formal organization (on some such continuum as "explicit, hypothetico-deductive axiomatization—informal exposition") considered best suited to requirements for systematization, at the present phase, in the area selected by the systematist.

In order to promote adequate understanding of the systematist's goals and working methods, it would be desirable to make the itemization of "orienting attitudes" reasonably complete.

{2} Structure of the system as thus far developed

- (a) Exhaustive itemization of *systematic* independent, intervening, and dependent variables.
- (b) Mode of definition of representative variables of each category.
- (c) Major interrelations among constructs.
- (d) Discussion of order of determinacy and other characteristics of construct linkages.

Explanation

What is sought here is not a discursive summary of the system, so much as a reconstruction of its conceptual structure via the isolation of the chief systematic constructs of all categories, and the exhibition of how they are interrelated within the system. The presentation need not be particularly lengthy, since, for the purpose of the analysis, the systematist need not summarize contents of prior expository publications, to any marked extent.

In order to promote commensurability, we are suggesting that the systematists adhere to the independent-intervening-dependent variable schema which has become more or less conventional in recent methodological discussion. Since many systematic formulations have not been explicitly patterned on such a schema, the recasting of the systematic structure in this way may present difficulties, but, we suspect, not very formidable ones, in most cases.

In cases where a systematist feels that an attempt to recast his material into the independent-intervening-dependent variable schema does violence to his formulation, he may, of course, recapitulate the structure of his system in any way that he considers appropriate.

In certain cases (e.g., "positivistic" systematizations), a system may not contain conceptual components which correspond in functional significance to "intervening variables." In such cases, the systematist's task will obviously reduce to the isolation of systematic independent and dependent variables, and their interrelations.

For purposes of this study, we stipulate the following rather informal definitions of the three classes of systematic variables.

1. The "independent variables" of a system are the terms referring to the factors available for identification, "measurement," and, when

possible, manipulation, which are discriminated within the system as the antecedent conditions of the events that the system is designed to predict.

2. The "dependent variables" of a system are the terms designating the classes of events that the system is designed to predict.
3. "Intervening variables" are terms interpolated between the independent and dependent variables, having properties such that a class of empirical relationships describable by a given number of statements which directly relate independent and dependent variables can be derived from a substantially smaller number of statements which relate independent to intervening variables and these, in turn, to dependent variables.

Note that the item {2} discussion topics call for the isolation of "systematic" independent and dependent variables. In explanation of this, it may be well to note that the expressions "independent variable" and "dependent variable" have become highly ambiguous in discussions of psychological methodology. The independent-intervening-dependent variable schema established (in the first instance) by Tolman for the analysis of theory implies a sense of the expressions "independent variable" and "dependent variable" which overlaps only partly with these expressions as they are used in mathematics and in general scientific methodology. In order to be entirely clear for the purpose of the present study, we present three senses of the expression "independent variable" (analogous definitions may immediately be derived for the expression "dependent variable").

SENSE I. SYSTEMATIC INDEPENDENT VARIABLES

Terms in the *construct language* of a theory denoting the chief classes of empirical events which serve as the operationally identifiable or "measurable," and, wherever possible, manipulable antecedent conditions of the events that the theory is designed to predict. This is precisely the sense in which the present discussion topic calls for the isolation of the "independent variables" of the system under analysis. We may refer to "independent variables," in this sense, as "systematic independent variables."

SENSE II. EMPIRICAL INDEPENDENT VARIABLES

A term or expression denoting any factor in an experimental situation which is systematically varied, or operated upon in some way, with the intent to observe and record a correlated change in another part of the system defined by the experiment. Sense II independent variables may be called "*empirical* independent variables." Sense I and Sense II are very often confused. *Empirical* independent variables may be specific, singular "realizations" (operational or reductive "symptoms") of a *systematic* independent variable; they are not, however, to be identified with the systematic independent variable to which they are ordered. Sense I independents are terms in the *construct language*; Sense II independents are expressions in *immediate data language* (cf. "explanation," item {3}). A Sense II independent variable need not be a "realization" of a Sense I independent; empirical rela-

tions between experimental variables which are ordered to no extant theory are often investigated.

SENSE III. "MATHEMATICAL" INDEPENDENT VARIABLES

All terms in a statement of functional dependency of which a given term (the dependent variable) is a specified function. This corresponds roughly to the usage of "independent variable" in mathematics. We give this rather obvious usage for purposes of completeness.

It might be added, at this point, that in most instances *systematic* independent and dependent variables are introduced into a system and given empirical meaning by some stipulated linkage(s) to a set of *empirical* independent or dependent variables (this is one way of elucidating what is meant by so-called "empirical" or "operational" *definitions*). Thus, in the present analysis, a systematist may wish to employ some such distinction when discussing such questions as "mode of definition of representative variables" [item {2}(b)] and certain other questions introduced in later sections [e.g., items {3}(c) and (d)].

{3} Initial evidential grounds for assumptions of system

(a) Identify the chief classes of experimental and/or empirical data which have served as the initial source of evidence on which the system was based, or have been used in any way to suggest the major assumptions of the system.

(b) Why was this material considered "strategic," or in some sense "fundamental," relative to:

- (1) other sources or varieties of data within the same empirical area,
- (2) data in other empirical areas for which the system is intended to hold?

(c) Isolate the chief *empirical* independent and dependent variables (in "theoretically neutral," "immediate data language" terms) in the evidence on which the system is based.

(d) Show how empirical independent and dependent variables (as expressed in "immediate data language") are linked to *systematic* independent and dependent variables (construct language).

Explanation

In the discussion of (a), it would be interesting for the systematist to consider whether, in general, the system has thus far been based primarily on *extant* empirical data, or whether the systematic program has been contingent on the prior extension, or "opening up," of a field of data by the individual systematist, or group of investigators working within the systematic context.

In (d) we have reference to the distinction between *systematic* independent and dependent variables (Sense I) and *empirical* independent and dependent variables (Sense II), precisely as made above (cf. "explanation," item {2}).

For uniform understanding of items (c) and (d), it might be useful to specify what we mean by "immediate data language." One may say that all empirical ("operational") definitions of a system are constructed from a linguistic base that may be called the "data language" of the system in question. *Immediate data language* is the language, presumably univocally intelligible to all competent workers in the field, in which empirical or operational definitions of systematic terms are put forward, and against which primitive and derived statements of the system are compared. In general, then, "immediate data language" tends to appear in two contexts in connection with an empirical system:

1. in statements which are explicitly intended to provide operational definitions of terms in the construct language, and
2. in descriptions of experimental (or general empirical) conditions, observations, and the results of statistical or mathematical transformations of observations which the systematist or investigator is relating in some way to the construct language of the system.

One may distinguish "*immediate data language*" from another sense in which "data language" is often used in methodological discussions—i.e., as the "epistemic reduction basis" of the terms of a system. This involves reduction of the systematic (construct language) terms to the "ultimate" confirmation language to which all proper statements of the system are, in principle, reducible. We are not concerned with "data language" in this latter sense in the present group of discussion topics.

44} Construction of function forms

(a) How are independent-intervening-dependent variable—or, in the case of "positivistic" systems, independent-dependent variable function specifications constructed?

(b) Rationale of, and grounds for confidence in, the procedure.

(c) Contemplated modifications or extensions of the procedure as the theory develops.

(d) Grounds for favoring employment or nonemployment of intervening variables.

Explanation

When thrown into independent-intervening-dependent variable form, any system will contain stipulations, at one level of explicitness or another, with respect to the interrelations among these variables. Such construct linkages will vary from rather general adumbrations of the functional relationships to highly specific descriptions of function forms. Thus, "function specifications" may range from "purely qualitative" verbal descriptions through varying degrees and modes of quantitative explicitness, depending on the systematic intentions, the area under systematization, etc.

Such function-form specifications are, in one sense, free and creative "constructions" on the part of the theorist. In another sense, however, they "come from somewhere," and are "arrived at" on the basis of some set of

rules, however implicit. It would be most useful if systematists participating in the present study, would make an attempt to explicate or reconstruct their procedure in arriving at the specification of function forms. In the case of some systems, construct interrelations may register in a relatively *direct* way the interrelations among *empirical* variables, as determined in specific experiments or empirical studies which are believed to have fundamental significance. Such relationships may be "transposed" to the systematic variables in a variety of ways, ranging from empirical "curve-fitting" to verbal descriptions of the trend of the findings. In the case of other systems, the construct linkages may apparently be arrived at by "rational analysis," but in ways which are differentially based on inductive evidence, and which may range in form from the positing of rational equations to the stipulation of verbally formulated, qualitative interrelations. In still other cases, the technique of function construction may be partly "empirical" and partly "rational," as combined into various concrete strategies.

15} Mensurational and quantificational procedures

(a) What procedures are either specified or presupposed by the system with respect to the "measurement" (in the broadest sense) of the systematic independent and dependent variables?

How would the "level" or type of mensurability presently characteristic of the systematic independent and dependent variables be located by the systematist within the terms of the logic of measurement?

(b) To what extent do the procedures for "measurement" of the systematic independent and dependent variables satisfy the mathematical requirements of whatever quantitative techniques are employed for the description of function forms?

(c) What is the systematist's estimate of the principal difficulties in the way of increasing the mensurational and quantitative adequacy of the system? Future plans with respect to the mensurational and quantitative development of the system.

(d) Views of the systematist with respect to limitations, *in principle*, on "level" of measurement and degree of quantitative specificity of:

- (1) his own system,
- (2) systematic efforts in psychological science generally.

Explanation

Obviously, certain of these discussion topics will not be relevant to many of the systematic formulations sampled within the present study. Some formulations will be nonquantitative, in principle. Others will be prequantitative in their current form. In such cases, it would nevertheless be of great interest for the systematist to discuss items (c) and (d).

16} Formal organization of the system

(a) Status of the system with respect to explicitness of axiomatization, and of derivational procedures employed.

(b) What factors (e.g., “strategic,” “empirical”) are responsible for the present mode of formal organization of the system?

(c) Views of the systematist about the ultimate level of formal explicitness for which it is desirable, *in principle*, to aim.

Explanation

Explicitness of axiomatization and derivational specificity or rigor can clearly vary over a very wide range, from informal exposition to detailed hypothetico-deductive development within the resources of mathematical notation and symbolic logic. It would be interesting if, in the discussions of the above topics, the systematist would present his views on such questions as the degree of “formalization” which he feels it may be fruitful to aim towards, in areas other than those to which his own systematic work is relevant.

In the discussion of “formal organization” a recapitulation of the definitional techniques employed within the system would be highly useful. Ideally, this would include a reconstruction of the roles of “implicit” (i.e., “postulational”) definition, “explicit” definition, empirical or “operational” definition, and, in certain cases, “coordinating” definition, as these are respectively realized within the system.

47} **Scope or range of application of system**

(a) Actual scope, as the system is currently constituted.

(b) Intended, ultimate scope and grounds for this delimitation.

Concrete plans and programmatic devices for extension.

(c) Interrelations, present and potential, with formulations of other systematists in:

(1) areas coextensive with system, and

(2) other empirical areas.

48} **History of system to date in mediating research**

(a) Itemization of the chief experimental or empirical research studies, or clusters of such, which the system has directly (i.e., by logical implication) or indirectly (i.e., by suggestive or heuristic guidance) instigated.

(b) What specific components of the system—e.g., orienting attitudes, general but incompletely specified “explanatory mechanisms” or constructs, specific lawful assumptions, methods—have been responsible for the research instigated by the system?

49} **Evidence for the system**

(a) Current status of the “positive” evidence for the system (to the extent that this is not covered in item 48} above).

(b) Major extant sources of incompatible or “embarrassing” data.

(c) Specification of experimental designs which would be regarded as "critical" or important tests of principal foundation assumptions.

(d) Types of data which, in the opinion of the theorist, the theory accounts for more successfully than do alternate formulations. Classes of data which alternate formulations handle more successfully.

{10} Specific methods, concepts, or principles of the system believed valuable outside the context of the system

(a) Methods, concepts, or principles deemed fruitful for systematic advance in areas outside the projected range of application of the system.

(b) Chief methods, concepts, or principles believed to be of long-term significance, independently of the over-all structure or detailed assumptional content of the system.

{11} Degree of programmaticity

(a) Evaluation of the over-all extent to which the systematic program has been realized, at the given time.

(b) Estimation of the extent to which the system is tending towards convergence with other coextensive systems, articulation with systems having different empirical domains, subsumption of more limited systems, or subsumability under more general ones.

{12} Intermediate and long-range strategy for the development of the system

(a) What classes of empirical relationships does the theory most require knowledge about, and in what priority order?

(b) Estimate of the chief conceptual and empirical difficulties working against the development of the system.

(c) Estimates, based on the systematist's experience, of the chief barriers blocking *general* theoretical advance in psychology.

NOTE ON THE USE OF DISCUSSION TOPIC INDEX NUMBERS

As a convenience for the reader interested in the relation of essays to the discussion topics and in the cross-comparison of positions on key issues, index numbers corresponding to the twelve discussion themes have been inserted at relevant places in the Table of Contents preceding each of the essays. These numbers are placed in brackets immediately following the germane rubrics of the author's plan of discussion.

By and large, correspondences between authors' organization and the discussion topics are straightforward, and can easily be identified from the author's formulation of headings. Not infrequently, however, an author's system of headings may, in one or another way, be out of phase with the discussion rubrics, even though some or all of the relevant issues are considered. This circumstance has led to the following conventions:

The section designated by a given author-heading may be relevant to two or more themes. In such cases, the brackets will contain the requisite plurality of index numbers, e.g. {3, 8, 9}.

In cases in which a section, or some part of it, is *primarily* relevant to a given theme but includes brief, partial, or implicative consideration of a number of others, that is indicated by a + after the index number of primary relevance, e.g. {2+}.

When a section encompasses a number of discussion topics but gives them markedly different attention or emphasis, it has occasionally seemed worth setting the bracketed numbers in an order which roughly reflects this, e.g. {4, 5, 3}. Since such discriminations of relative emphasis cannot always be clearly made, there is no implication that index numbers are *not* differentially relevant when they are given in consecutive numerical order, e.g. {4, 5, 6}.

We should note, also, certain *general* restrictions on the use of index numbers:

With very few exceptions, they have been used only in conjunction with *major* subdivisions of the papers (i.e., headings of high "value"), the exceptions having been mainly cases in which essays contain a final section specifically for the purpose of bringing aspects of the preceding discussion

to bear on the themes. In such cases, index numbers have been inserted to identify the themes dealt with in relevant subsections.

Index numbers uniformly pertain to discussion themes as a whole, and do not separately identify the subitems which invite differentiated discussion under each theme. Once the correspondence with a given theme is identified, the reader will find that, in most instances, the bearing on particular sub-themes is easily discriminated.

In several papers, the author's plan of organization is such as to preclude the insertion of index numbers. In some of these (e.g., Ellson, Skinner) the author has preferred a type of discursive presentation sans headings— or has used so few of them that any use of index numbers would have been nondiscriminating. In a few cases (e.g., Pirenne and Marriott, Kallmann), the author's organization is so markedly out of phase with the discussion themes as to make any use of the numbers either confusing or unnatural. Nevertheless, it will be found in most of these cases that it requires little effort to determine the author's position with respect to many of the thematic issues. There are a few essays, however, to which certain of the suggested themes are not relevant in principle in that the concern is primarily with presystematic issues (e.g., Harlow).

Whatever the explicitness of relation of each paper to the themes, it should be emphasized that each is a self-contained essay, having *sui generis* properties in substance and form. Any cross-comparison or integration of findings which the reader may wish to conduct must depend on his own active discriminations; it will not be provided ready-made by any mechanical device. The present system of indexing is offered merely as a convenient starting point for comparative analysis. It has been kept typographically inconspicuous, and used in conjunction only with molar rubrics, both of author and thematic organization, so as not to interfere with the organic unity of each presentation.

Some Trends of

STUDY I. CONCEPTUAL AND SYSTEMATIC

Volumes 1, 2, and 3

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SIGMUND KOCH

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INTRODUCTION

Thirty-six men have responded with gallantry and dedication to an unusual challenge. In examining their own inquiring histories, they have written history. In reflecting on and assessing their inquiry, they have changed history. In the conjoint ordering and reordering of inquiry, they have made history. While so doing, they have shown what analysis can be when the creative function is not quarantined from the critical. And in this process, the very canons of analysis which have regulated action in our science for many years have been rid of staleness—perhaps transformed.

The essays in these volumes will well repay the efforts of independent analysts—be they interested in psychology's near history or its prospects, in problems in the enaction of psychological science or of science. The educator and student will find them valuable and will wish to pursue their own analyses. Even the self-determining citizen may wish to form his own perception of the place of psychology in the pattern of modern knowledge. It is thus vital that nothing that could be construed as some "official" summary or statement of conclusions stand between the reader and the essays.

So strong has been the tendency of recent scholarship (in psychology and elsewhere) to press complexity into stereotype and to sloganize the subtle that the editor has been of two minds as to whether general commentary should be included. Yet *certain* trends seem so clear as to warrant the hope that one's impressions are not arbitrary. And when these trends are taken together and seen against the cloth of recent history, it is difficult not to feel that there are objective senses in which the import of the study can lead to a profound clarification, even redefinition, of the ends and instrumentalities of systematic effort. It is difficult, that is, to remain silent. Yet it is meet that this epilogue be the next thing to silence.

The present statement will thus be confined to trends which seem, as it were, to announce themselves. Among these, it will select only a few of the most insistent, and these few will be reported in the roughest grain. The trends examined will not be *conclusions* suggested by or imposed on the "data" of the study. They will be more like tracings of pervasive attitude, interest, and judgment clusters *within* the "data." The interest will be in those clusters which can most clearly give a "fix"

on the position of systematic psychology relative to its recent history. That will entail a selection of clusters that appear most critically to qualify or depart from the ideology and practice of recent systematic psychology.

The Time-reference of the Study

If trends are to be seen relative to recent history, that interval requires bounding. This we can do with no more sharpness than the vaguely tapering margins of any "unit" of history. The assumption in planning the study was a time-reference of some three decades—give or take five years. This seems reasonable, if only for the fact that many contributors—certainly the senior ones—are reporting on formulations which originated, or first achieved influence, in the early 'thirties.

There is a more significant sense in which the results of the study must be seen against the past three decades. Most will agree that during this period there has been a relative cohesiveness in the temper of systematic psychology. Cultural history is a dangerous art, but it would be difficult to write a cultural history of the past thirty years which did not find them unified by a relatively homogeneous set of attitudes toward the ends and instrumentalities of systematic work. This short epilogue is not the place for such an effort.¹

The present statement must go on the assumption that certain secrets of recent history are open ones. Thus it is fairly evident that at some point during the interval 1925–1930 there was occurring a transition between an era of "schools" and what might be called the "Age of Theory"—an age during which all activities were subordinate to the production of a commodity called "theory" in a quite special sense defined by the age. It will be evident that the Age of Theory was initiated by a revived and driving desire to ensure that psychological knowledge become cumulative and sharable in the sense that such properties are believed characteristic of other sciences. It will be evident that the Age of Theory perceived the argumentation of the schools as cross-purposeful and sterile because there seemed no agreed-upon decision procedures for its resolution. Many other things will be evident:

The search for a "decision procedure" did not have far to go. It was coincident with the exportation into the public domain of a bold and positive view (or family of such) of the nature of theoretical science—a reconstruction based mainly on certain of the outstanding achievements of physics. Logical positivism, neo-pragmatism, operationism had

¹ An attempt to isolate certain of the continuities in the ideology governing systematic practice in psychology since 1930 is made in the "postscript" volume of the series, *Psychology and the Human Agent*. The section of that book on the "Age of Theory" tries to establish in some detail what must here be presupposed.

made available a substantial body of doctrine which was open to construal as providing a formulary for the "construction" of theory. As the Age gathered momentum, the belief became increasingly widespread that the "new" generalizations of theoretical practice promised a technology for the "construction" of theory in any field. Theoretical publications in psychology tended increasingly to divide concern between translating the new "science of science" into stipulations of the objectives of "sound theory" for psychology, and presentation of formulations intended to approximate such objectives. All will recall the development of a dense secondary literature devoted exclusively to explanations of proper theoretical technique, the adaptation of the new view of science to special problem contexts of psychology, etc.

It is hardly necessary to reconstruct the atmosphere of the Age of Theory, particularly that of its classical interval, say, from the mid-thirties to the mid-forties. The regulation of systematic work by the directives and imagery of hypothetico-deduction, the sub-culture surrounding operational definition, the lore concerning the intervening variable, the belief in the imminence (if not achievement) of precisely quantitative behavioral theory of comprehensive scope, the broadly shared judgments with respect to strategic foundation data, the belief in automatic refinement and convergence of theories by the device of "differential test," the fixed vocabulary for the comparative dissection and analysis of theory—all of these are easily recalled, if indeed recall is necessary. The rather stable geography of dominating theoretical positions and the standard contexts of apposition and opposition will also come easily to mind. These scattered fragments define an ideology not discontinuous with that of the present period.

At this level, caricature is inevitable. The past thirty years have seen much change: there has been a wide and shifting dispersion of systematic ideas. If this were a history of the Age of Theory, it would be necessary to document the conceptual and methodic inventions, and the changes and cutbacks even in elements of Age of Theory *ideology* which have occurred, say, in the past fifteen years. It would be necessary, for instance, to acknowledge the shift in confidence indicated by the substitution of terms like "model" (and associated imagery) for "theory" and, indeed, such cutbacks in aim as are registered by that from "comprehensive" to "limited" systematization. It would also be necessary to acknowledge the extension of established systematic frameworks to new ranges of phenomena, and the appearance of new formulations, new problematic interests.

Plan of Discussion

Perhaps most perdurable and potent in Age of Theory ideology has been a set of cognitive-strategic and epistemological agreements shared

by large numbers of investigators. The viability of these has been ensured by their depth within the presupposition chains of inquirers. Even *such* agreements represent no absolute contracts. Their construal has varied across men and over time—but always within tight ranges. Such “agreements” are in a sense the postulates of the Age of Theory; if not the logical premises, the psychological ones. In this brief statement it would be well to raise questions concerning the bearing of the study on commitments of this order. We shall concentrate on findings which relate to five classes of such commitments. These pretend to represent no exhaustive classification of Age of Theory “premises;” they form, however, contexts with respect to which one can derive a first impression of the study’s general import. They are:

- I. *The Intervening Variable Paradigm for Theory “Construction”*
- II. *Problems Concerning the Generalization Range of Psychological Laws*
- III. *The Observation Base of Psychological Science: Its Relation to the Legitimate End-terms of Systematic Analysis*
- IV. *Mathematization of Systematic Relationships*
- V. *Formalization and Psychology*

These commitment-classes, it must be stressed, are each no more independent of the others than can be expected of attitudes and judgments of men.

Our task, then, will be briefly to consider each and inquire what currents of questioning, shifts of judgment, realignments of values, intimations of change are shaping up. The task will be bounded not only by the limited number and generality of the trends selected but by the mode of discussion contemplated. The aim is to exhibit trends; not to report them in detail. Though certain trends seem to point towards some form of “resolution” of the problems to which they are responses, we will not essay such interpretation at this place. Nor will it be possible to give the position of all contributors with respect to each topic. We seek merely to isolate major currents of change and questioning which seem common to *large groupings* of the contributors—in some cases to all of them. If this epilogue can entice the reader back to the essays in quest of new relationships, or prompt him to form further questions concerning their bearing on history, its purpose will be well met.

I. THE INTERVENING VARIABLE PARADIGM FOR THEORY “CONSTRUCTION”

Decisive among the commitments governing Age of Theory ideology has been the doctrine connected with “intervening variables” and their function in psychological theory. First introduced in the early ’thirties by

Tolman as a modest device for illustrating how analogues to the subjectivists' "mental processes" might be objectively defined, the concept of the intervening variable was soon after elaborated by Tolman and others into a paradigm purporting to exhibit the arrangement of variables which must obtain in any psychological theory seeking reasonable explanatory generality *and* economy. As is well known, Hull in the late 'thirties identified what he had previously tended to call "logical" or "theoretical" constructs with the intervening variable, and in subsequent formulations of his theory assiduously adhered to the independent-intervening-dependent variable pattern.

The appeal of the intervening variable paradigm to Age of Theory systematists was twofold. First, the criterion of "firm anchorage" of hypothetical theoretical concepts *via* explicit functional relations to "antecedent" and "consequent" *observables* seemed neatly to fill the strong requirement of the age for a theoretical *decision procedure*. If inferred explanatory concepts were to be unequivocally *linked* to observables, no longer need there be fear of irresponsible constructions whose role within the theory is instant to the whim of the theorist (what Hull called "anthropomorphism . . . in behavior theory"). At the same time, the paradigm seemed to render into orderly and intelligible terms the *problems* confronting the psychological theorist: e.g., he needed three classes of variables; he needed the interconnecting "functions"; he needed a mode of inferring or constructing these functions; etc. Moreover, the schema was readily reconcilable with various elements of the science of science lore which had powerfully determined Age of Theory ideology since inception. The demand for explicit linkages with observables could be equated with *operational definition*. The statements interlinking the three classes of variables could, if one so desired, be asserted as *postulates*, thereby making place for the paraphernalia and imagery of *hypothetico-deductive* method. The fervent drive towards the *quantification* of systematic relationships characteristic of the era could become the quest for quantitatively specified intervening variable functions. And so on.

It was inevitable, then, that a lush literature develop concerning intervening variable doctrine—a literature which, in some instances, came close to suggesting that proper explication of the intervening variable paradigm could provide a *technology* for the "construction" of theory. Indeed, even as early developed by Tolman, and later applied by Hull, the intervening variable schema was associated with a "strategy" for constructing intervening variable functions. In briefest terms, this strategy was to select or design a series of *defining experiments*, the variables of which would be placed in correspondence with (that is, "represent" or "realize") the theoretical variables whose relationships

were under determination. Standard curve-fitting techniques were to be applied to such experimental results. The resulting equations or "curves" were then presumably to hold for the theoretical variables whose relations were in question. Though such a strategy can be (and has been) elaborated in widely varying ways, its rationale has rarely been questioned.

It cannot be our purpose even to sketch the range of issues dealt with in the massive literature on intervening variables—or those implicit in the actual systematic formulations which have presupposed the schema. Here, as elsewhere, acquaintance with the general content of recent history must be presupposed. What has seemed important to establish is the weighty, if not central, position of intervening variable doctrine in Age of Theory ideology. The issues raised in this context have long ramified (and still do) into virtually every area in which questions about systematic or presystematic procedure have been entertained. It was for this reason that the distinctions re independent-intervening-dependent variables were made so prominent a part of the Study I discussion outline. And it is for the same reason that we will be repeatedly led back to considerations concerning the doctrine of variables in examining *other* trends.

The over-all tendency of the study is to call the intervening variable paradigm and much of the associated doctrine sharply into question, and to do this in almost every sense in which questioning is possible. Virtually every contributor has shown a disposition to qualify some aspect of the doctrine: in some instances only diffidence seems to prevent qualification in all aspects. Because of the scope of the issues, we can give only the most general sense of a few findings. These we report as they bear on three (not unrelated) questions:

1. *The "strategy" for constructing intervening variable functions*
2. *Generality of intervening variable functions: achieved and in principle*
3. *The problem of "determinate linkage" to observables*

1. The "Strategy" for Constructing Intervening Variable Functions²

The defining property of the Age of Theory—the quest for the *rule* of the theoretical process—is vividly symbolized by the interest in a strategy for constructing intervening variable functions. As we have seen, such a strategy was associated with Tolman's original analysis, and

² An "intervening variable function" is any functional relationship involving an intervening variable as *at least one* of its terms, i.e., a relation between independent and intervening variables, *or* intervening and intervening variables, *or* intervening and dependent variables.

in fact some form of the "defining experiment" procedure has remained a part of intervening variable doctrine until this day.

The most decisive thing that can be said about this issue is that the originator of the doctrine has now come full-circle relative to the feasibility of "standard" defining experiments. And, in general, his conception of the significance of the intervening variables within his own theory has markedly changed. It is well that this author's famous propensity for freshening up his vocabulary from time to time not lead the reader to take the present change lightly. For it represents something of a *bouleversement* with respect to certain of the deepest attitudes regulating the entire direction of his previous effort. Tolman's position is best conveyed in his own words. Because of its historic significance, we give a rather full citation.³

When it comes to the question of intervening variables, on the other hand, this is where the schools differ. My own particular brand of intervening variables were admitted to come primarily from my own phenomenology. Thus Köhler's designation of me as a cryptophenomenologist was probably correct. I do, however, attempt to objectify my intervening variables and to suggest standard defining experiments for getting empirical pointer readings for them.

Actually, however, it should be admitted that I really have considerable doubts not only about the practical feasibility of such experiments (since they would involve a tremendous amount of time and labor) but also about the validity of the results which would be obtained. (Italics mine.)

My proposal was that one should set up standard defining experiments in each of which the obtained response or responses (i.e., performances) could be conceived as depending primarily upon, as being a direct pointer reading for, the variations of one particular intervening variable as this latter is dependent upon the controlled and prescribed manipulations of one or two independent variables. It was assumed that one could thus acquire a sort of table showing just what the values of each intervening variable would be as the result of such and such values of the correlated and controlling independent variable or variables. And it was assumed, further, that these relations of the values of each intervening variable to one or two independent variables would hold in new, nonstandard, nondefining situations as well—so that the values of the intervening variables could be predicted from the values of the independent variables in the new situation.⁴ But I wish now to emphasize that this last assumption might well

³ Regrettably, we cannot make a practice of giving other lengthy citations in this epilogue without risking its conversion into a detailed statement of findings. It is difficult to refrain in that there are so many passages in the study which are *both* historically important and quotable.

⁴ The defining "strategy" as given here is substantially the same as that advocated in Tolman's "classical" early discussions of such issues, but it will be obvious that his view as to the character of his theoretical constructs has changed.

prove invalid. For there may be all sorts of interactions between the variables (independent as well as intervening), in the new nonstandard situations, interactions which could not have been predicted from the results obtained in the standard defining situations by themselves. For, in these latter, rigid controls of all but one or one small set of independent variables would have been imposed. Hence, I have considerable doubt concerning not only the practical feasibility but also the validity of the proposal.

It might be said, however, that there is another possible way of conceiving my intervening variables. This would be to admit that they are merely an aid to thinking ("my thinking," if you will). All anyone really sees are the empirically stipulated independent and dependent variables. In developing notions of what happens in between—such as beliefs, expectancies, representations, and valences and finally what I call performance vectors and their interactions—all I really am doing is setting up a tentative logic (or psychologic) of my own, for predicting what the dependent behavior should be and how it should be affected by variations in such and such sets of independent variables (Vol. 2, pp. 147–148).

Tolman is *not without support* within the present study in making such an evaluation of "defining experiment" strategy. Lazarsfeld has made exactly the same evaluation in the course of his analysis of the need

In recent years (cf. "A Psychological Model," in T. Parsons and E. A. Shils (eds.), *Toward a General Theory of Action*, Harvard University Press, Cambridge, Mass., 1951), Tolman has moved from what was initially a "functional" conception of his intervening variables (i.e., meaning uniquely constituted by linkages with independent and dependent variables) towards an "hypothetical construct" interpretation. In the present article he identifies them as "hypothetical constructs" but of a special sort: e.g., "And intervening variables . . . will have in part the properties of *hypothetical constructs* and not merely be intervening mathematical quantities. However, the 'surplus meanings' of my intervening variables which make them into hypothetical constructs are not at this stage primarily neurophysiological, as it is suggested by MacCorquodale and Meehl that they should be, but are derived rather from intuition, common experience, a little sophomoric neurology, and my own phenomenology" (Vol. 2, p. 98). It thus becomes appropriate for Tolman to talk of defining experiments as "pointer readings" for intervening variables. The early position (strictly interpreted) would have precluded such a metaphor—though in effect the defining experiment strategy, *as even then envisaged*, presupposed that an *empirical* variable in an experiment could in some sense directly mirror or "reflect" the "values" of a corresponding intervening variable as a function of the independent empirical variable manipulated in the experiment. The matter is, of course, academic in that it is clear that *now* the notion of a "defining experiment" is itself a metaphor for Tolman—one which functions as a kind of self-imposed check on his "own phenomenology" which he claims (on excellent grounds) to "like." The defining experiment is, in other words, a *thought experiment* in the classical sense of this device of the philosopher of science. There seems little doubt that Tolman would agree that the "defining experiment" has *always* functioned for him as a thought experiment, rather than as a workable gimmick for the manufacture of theory.

for progress in the methodology of social science index formation. After quoting Tolman's strategy of the defining experiment, as presented in his 1951 article, "A Psychological Model,"⁵ Lazarsfeld points out:

The idea is that we can find one specific indicator for each intervening variable. Everything else being constant, the variations in the indicators correspond to the variations in the intervening variable. *We have grave doubts whether such a procedure is feasible even with animal experiments.* And we are confident that it is the wrong idea as far as the study of human behavior is concerned (Vol. 3, pp. 482-483; italics mine).

Later on p. 483, Lazarsfeld states:

There is just no way to develop a "standard experimental setup" or "standard defining experiment." We will have to face the fact *that to an intervening variable there will correspond a variety of indicators and that they will have to be reconciled in some way.*

It is interesting—and indeed symbolic of the pervasiveness of major systematic issues across the most widely disparate areas—that this critique of one of the ruling assumptions in learning and behavior theory should come from a sociologist.

Though most other authors do not in a comparably direct or explicit way challenge the feasibility of the "defining experiment" procedure, widespread convergence towards such a challenge is evidenced by many other questions and difficulties that are raised. Thus, for instance, the powerful attack by Brunswik (as represented by Postman and Tolman, Vol. 1) on the assumptions of "systematic" experimental design in general would rule out the possibility of constructing useful theoretical relationships via the "defining experiment," as would the doubts expressed by Cattell (Vol. 3) concerning the adequacy of "univariate" experimental methods for the isolation of variables which behave as significant "functional units" (whether these be intervening, or *systematic* independent or dependent). Naturally, an observer such as Skinner (Vol. 2), whose "scientific practice is reduced to simple looking" through the "microscope" provided by his methods, has no need for "theoretical phantasy" nor thus for intervening variables or any strategy for their inference. And Guthrie (Vol. 2) is not only still inclined to view his (now greatly changed) formulation as devoid of intervening variables, but his evaluation of many matters, ranging from the limits of psychological prediction to the limited utility "of the laboratory studies of the past generation," makes it clear that he would dismiss "defining experiment" strategy as irrelevant to his purpose and infeasible *in*.

⁵Tolman, E. C., "A Psychological Model," in T. Parsons and E. A. Shils (eds.), *Toward a General Theory of Action*, Harvard University Press, Cambridge, Mass., 1951,

general. As we will see later, *all* authors in this study who work at the levels of epistemic complexity set by the problems of social psychology and personality raise questions concerning the status of their concepts which suggest any "defining experiment" basis for construct inference to be utterly beside the point.

Though on this topic as elsewhere, we restrict comment to only a scattered sample of findings, it might be noted that both the remaining topics in the present section have strong implications for the "defining experiment" issue.

2. Generality of Intervening Variable Functions: Achieved and in Principle

Whatever else an "intervening variable" may be, it is, by intention at least, *a device for facilitating scientific statements of some generality*. It is hardly necessary to remind the reader that influential Age of Theory formulations have often put forward intervening variable functions of unrestricted generality, in some cases with attempts at *quantitative* variable linkages.

In order to encourage concern with the generality of intervening variable functions (and generally of lawful psychological statements)—and especially a disentangling of intended, actual, potential, and feasible generality "reference"—the Study I discussion topics suggested a distinction between *systematic* and *empirical* independent (and dependent) variables. Such formulations as that of Hull's 1943 theory and Tolman's early systems had tended to represent the antecedent and consequent conditions discriminated by the theory as direct "observables." But the stipulated independent and dependent variables were in fact far from this: such notions as "past training," "maintenance schedule," "heredity" (Tolman), or Hull's "S" or "C_D" (conditions constitutive of drives) or "G" (reinforcement) can be seen to discriminate enormously broad and heterogeneous classes of possible "operations" and/or observations. These are therefore *systematic variables* in the sense that they clearly represent rather complex epistemic constructions made within the *system language* of the theory in question. The numerous *individual* operations and/or observations "designated" by each such systematic variable would, in the terms of our distinction, be called *empirical variables*.⁶

⁶ The distinction between systematic and empirical variables, as given in the discussion outline and paraphrased here, is regarded as a crude rendition of an intricate epistemological picture—so intricate that fuller rendition would have freighted the already formidable discussion outline too heavily. One must distinguish *epistemic levels* (or levels of abstraction) in the analysis of *systematic* independent or dependent variables. A variable like the Hullian "C_D," for instance, is, if taken literally as "conditions constitutive of all drives," a complex class of classes of classes . . . abstraction of indeterminate order. Such a "variable" would

Now if one examines the lists of independent and dependent variables given for such theories closely, it should be clear that the stipulated theoretical laws are making enormously general commitments—from the magnitude of which the theorist himself was often protected by the tendency to *equate* his *systematic* antecedent and consequent variables with “observables” (i.e., *empirical* variables). In this way it was easy to overlook the fact that, say, an intervening variable function based on (or verified by) values of the *specific empirical* variables manipulated and recorded in a single experiment was often formulated in such a way as to assert this function for huge and indefinite classes of empirical variables (i.e., merely by transposing the function into the “corresponding” *systematic* terms of the theory language).

It would be too much to say that such optimistic or uncritical tendencies to over-generalize systematic relationships are entirely absent in the formulations of Study I. But there is massive evidence of a disposition by contributors to be far more realistic and conservative in estimates of achieved, potential, and even intended generality of their theoretical functions than has been the fashion in recent decades. On the other side of this coin is a sober recognition of the fact that whatever degree of generality may be attainable for a given theoretical statement must be *purchased* by progressive testing (either of a direct nature or in terms of consequences) in situations other than that of initial determination.

Indeed, it is of high interest that in Study I a person working within the tradition of one of the more optimistic theories re problems of the generalization range should make one of the most conservative assessments on record of the achieved generality of intervening variable func-

denote the class of conditions constitutive of hunger, thirst, sex, pain avoidance, etc., etc., which in turn are constructions upon the classes of alternate conditions constitutive of each (say, hours of food deprivation *or* per cent weight-reduction for the case of hunger). These latter condition-classes are in turn themselves constructions upon the classes of alternate procedures for the manipulation of each (say, hours of deprivation after establishment of such-and-such a feeding rhythm, or after satiation in such-and-such a way, or with respect to such-and-such a diet, and so on). The *level* at which a theorist sets (i.e., defines or discriminates) his systematic variables, independent or dependent, is an option of the theorist. *Relative to this level*, the “elements” in all lower classes in the hierarchy may be regarded as *empirical variables* (or, by ellipsis, the names or defining-property designations of such classes may be so termed). What the theorist *cannot set by option* is whether the empirical variables thereby hypothesized as covarying in the way stipulated by the theory *do, in fact, covary*. In other words, the theorist may *aim* for any given level of generality, but if the aim is not to be idle, he must know to *what* he is committing himself. An analysis of the sort here hinted at helps make such awareness explicit.

tions. In an acute analysis of the "experimental design required but seldom used to justify intervening variables" (Vol. 2, pp. 276-280), Neal Miller indicates that the minimum significant condition is when there are at least two independent experimental operations and two independent measures (that is, two *empirical* variables on the independent and dependent side, respectively). This indeed seems a modest demand. Yet he then points out: "Although many behavior theorists have used intervening variables, there are relatively few experiments which use the design required to test and justify such variables" (p. 277).

As is generally known, Miller has in fact carried out a number of experiments calculated to test for such modest degrees of generality. Of these, he says: "I have been interested in applying the appropriate type of experimental design to test whether some of the simplest situations in which we commonly assume intervening variables can actually be accounted for in terms of a single such variable. In many cases the different measures show the type of agreement that would be expected if they were all pure measures of the same intervening variable. But in other cases, there is disagreement" (p. 278). It should be emphasized that the situations Miller here refers to (whatever their actual empirical complexity) are indeed simple relative to the situation-ranges for which it has been fashionable to postulate intervening variable functions: they typically involve a limited assortment of discrete "operations" and "measures," respectively—a single primary drive (not "drive in general") being the hypothesized intervening variable. Similarly, when discussing certain programmatic extensions of his miniature system on conflict behavior, we find Miller saying: "The attempt to extend the system to the types of experimental situations that will really test the general utility of the intervening variables spotlights difficult problems which are latent in many of our efforts to construct psychological theories" (p. 225).

It is worth noting that in his concern with such modest problems of generality, it is not the generality of *quantitatively specified* intervening variable functions that Miller has in mind. On the contrary, he takes pains to set himself off from the Hullian tradition in this regard, pointing out that "there certainly is no virtue in the misleading trappings of pseudo-quantification" (p. 281). And he champions "the strategy of putting one's theoretical notions through qualitative tests first before plunging into laborious attempts to quantify them" (p. 281). In considering extensions of the conflict theory to problems of psychotherapy and personality, he is sharply aware of the "need for better definition or scaling of empirical variables" even with respect to qualitative applications (p. 227). And indeed, in evaluating the applicability of the theory to extra-animal problems, we find him saying: "In all these areas, ex-

cept the animal experiments, rigorous testing of the application of the theory is severely limited by the difficulty in specifying the relevant conditions and in measuring the relevant responses, or in other words, defining precisely the empirical data variables" (p. 231).

In general it is difficult to conceive of a further cry from the classical Age of Theory attitude towards the generality of intervening variable functions. If Miller does not propose the abandonment of defining experiment strategy, he is certainly apprized of the generality-limits of those formulations which have based what are intended to be highly general theoretical relationships on functions deriving from the empirical variables of single defining experiments. He may not be saying that the defining experiment is infeasible in principle; he is certainly saying that it has given restricted results in practice. Admirably, he has embarked on a program of cross-checking multiple empirical independent variables on the one hand and dependent variables on the other hand, in an attempt to translate the defining experiment procedure into a device for establishing the "qualitative" coherence of intervening variable functions at modest generality levels.

Spence, as represented by Logan (Vol. 2), and certainly as evident in his recent book,⁷ also makes a more modest estimate of the immediately achievable generality of intervening variable functions than is characteristic of classical Age of Theory doctrine. Though the attempt to specify *quantitative* intervening variable functions is distinctly present, there is a disposition to make "lawlike" commitments only as among variables defined in terms relatively local to the data from which they derive, and to cross-check basic relationships in a variety of experimental contexts. Moreover, as Logan points out in conveying the flavor of the "Hull-Spence approach":

More abstract concepts provide greater generality but are difficult to formulate adequately. Frequently, therefore, the intervening variables are anchored informally at one level and more formally at another. If, at any particular time, the theorist is not able to provide a satisfactory anchoring for more than a limited portion of what he expects ultimately to achieve, he may give the more general formulation as an informal suggestion to permit trying the theoretical structure in areas outside its more formally relevant ones (p. 310).

This can be seen as certainly a guarded position in connection with questions of generality. As the section from which this quotation derives makes clear, the "formal anchoring" of intervening variables to which

⁷ Spence, K. W., *Behavior Theory and Conditioning*, Yale University Press, New Haven, Conn., 1956.

Logan here refers is in terms of independent and dependent variables defined at levels close to the data for which the function holds.

The rather conservative estimate of the achieved generality of function specifications made by the intervening variable theorists within the Hullian and neo-Hullian tradition would, of course, be accepted and in many cases exceeded by those (see pp. 736–739 above) who doubt the feasibility of defining experiment strategy (whether this doubt be general or relative to a man's own systematic problems).

It should of course be understood that the main question at issue here is the *warranted or ascertained* empirical generality of intervening variable functions that, in fact, have been hypothesized. To conclude that most such functions have little or no ascertained generality is not to conclude that their generality may not "overlap" their defining base. Indeed, a function could prove valid over the entire universe for which it is asserted, even when asserted on no evidence. It is fair to say, however—and by no means inconsistent with the trends of contributor opinion—that in psychology the happy accidents which might eventuate in such a state of affairs are most unlikely. Whatever generality intervening variable theory, in any form coherent with the Age of Theory conception, is to achieve will have to be won. Estes, whose systematization of learning phenomena can be cast loosely into correspondence with the intervening variable schema (Vol. 2, pp. 449–450), has well demonstrated how arduous is the process of establishing even limited evidence for transsituational invariance of parameters in modest cases involving determination of parameter values for his acquisition function in one situation and predicting to a closely similar one. This he has succeeded in doing in only a few instances (pp. 406–415).

3. The Problem of "Unambiguous Linkage" to Observables

Implicit in the present topic are questions of profound moment to the future of psychology. Here we can develop only a few hints. Even adequate *broaching* of the relevant issues would be an extensive enterprise.

The critique of "defining experiment" strategy is a sharp challenge to traditional intervening variable doctrine. It deprives that doctrine of its central recipe for the construction of theory. It implies that if the search for a *rule* of theoretical construct formation is to continue, the emphasis must shift from empirical towards imaginative *rational* strategies: in fact it can be taken to suggest that no "recipe," no matter how general or libertarian, is feasible. The present conservative estimate of the achieved generality of intervening variable functions in extant theories also sharply qualifies conventional doctrine. But neither challenge is fundamental. What *is* fundamental to intervening variable doctrine is

the purpose for which it was called into existence—to serve as a *decision procedure*, a prophylactic guarantee against “irresponsible” theorizing. And this core component of the doctrine is clearly the demand for “explicit,” determinate, or “unequivocal” specification of all construct relations; in particular the demand for “secure anchorage,” unambiguous linkage, as between critical intervening variables and “observables,” independent or dependent.

It will immediately be seen that the demand for unambiguous linkage to observables is nothing other than a translation into intervening variable language of the demand for *operational definition* (or empirical definition via other criteria having similar intent). Since this translation occurs in a context in which the major problem is that of introducing relatively high order *theoretical* concepts, it raises the issue of how theoretical concepts in psychology are introduced, validated, applied, and in some sense made to maintain intimacy with the empirical world.

This may impress the reader as a stale and supererogatory set of questions. Do we not, after all, have an extensive backlog of secure answers in terms of the operational criterion, the verifiability theory of meaning, the reduction sentence, various other forms of the empiricist criterion of meaning, etc.? If such analyses do not give us an ultimate theory of scientific definition, can there be any question but that their general tendency is decisive. *The results of this study powerfully challenge complacency on such matters. Our contributors pose questions for the theory of definition in the psychological and social sciences, neglect of which can be rewarded by only the most fitful kind of comfort.*

It is only too evident that all authors in these volumes who deal with problems requiring constructions of considerable epistemic complexity have persistent and severe difficulties in considering their formulations in terms of the intervening variable paradigm. What is impressive is not the existence of such difficulties (many will come as no surprise), but the absolute regularity with which certain of them recur among inquirers who often have quite different problematic concerns. Some of these difficulties are pointed up clearly and resolutely; others, though sorely pressing, are expressed in a more shadowy and implicative way. Some seem in themselves of little significance; some are associated with what may seem a “misunderstanding” of the best methodological precept. Taken as a pattern, *all* are significant. Here are some items, almost at random.

a. Many men say or imply that all of their theoretical constructs are at a “homogeneous” level with respect to any distinction that can be made between intervening variables on the one hand and independent and/or dependent variables on the other. They say or imply this in different ways. Thus, e.g., Rapaport (Vol. 3) illustrates in detail that the same (psychoanalytic) “variables” may occur as independent, interven-

ing, or dependent, depending on the context of application. Cattell (Vol. 3) apparently takes a similar position vis-à-vis the status of variables identified by factor analysis. Or they say it as, e.g., Newcomb, and Rogers (Vol. 3) do, by maintaining that they do not employ intervening variables but only independent and dependent. Or they say it, as do Murray (Vol. 3), Cartwright (Vol. 2), Katz and Stotland, Thelen, Parsons, Newcomb (Vol. 3), and others, by thinking of the arrangement of their concepts on a "systems" analogy such that the array of theory-language variables can—for purposes of the given analysis—be "entered at any point." Or they say it by maintaining or suggesting (Rogers, Parsons, etc.) that *any* of the systematic concepts can, for purposes of the given application or research, be operationally defined. They imply or suggest it further by slurring over the distinction between systematic and empirical variables, or specifically regarding their theoretical concepts as *at once* systematic and empirical (e.g., Rapaport, Vol. 3, p. 110).

b. Despite ambivalence, there is a reluctance to "use" the intervening variable paradigm. Thus Cartwright (in representing Lewin) and Murray more or less skirt the intervening variable jargon. Katz and Stotland, Parsons, and Rapaport seem to agree that the independent-intervening-dependent variable distinctions give no particular insight into the character of their theoretical formulations, but only become relevant in connection with empirical applications in which sub-sets of their concepts are linked with aspects of research situations.

c. When systematically defined independent and dependent variables are introduced or mentioned by students of the more "complex" man-pertinent processes, it is uniformly made obvious that these are—contrary to Age of Theory lore—very far from direct "observables," that in fact such variables are at an enormous distance from the scientific observation base, as conventionally conceived in psychology. Such "variables" are typically represented not merely as abstract, but as *hypothesized* theoretical constructions, as genotypes rather than phenotypes: in short, as something very much like intervening variables in so far as "constructive," "dispositional," "inferential" status may be concerned. Indeed, there seems little doubt that most systematists of the person and the social context would accept without embarrassment a view of their constructs which held them *all* "intervening variables."

d. Though students of the person and the social context uniformly and with little evident ambivalence speak the language of operational definition, specific illustrations of definitional practice and much explicit metatheory regarding the character of their concepts are patently and markedly at odds with the letter of operational law. Sometimes this point reaches the recognition threshold as, e.g., when Katz and Stotland say:

We hold that one of the real barriers to general theoretical advance in social psychology is the distance between genotypic constructs and our phenotypic measures. In physics, the concept of atmospheric pressure is fairly close to its operational measurement. In physiological psychology, many concepts are similarly tied to their operational measurement. In personality theory and in social psychology, however, concepts like ego strength, defense mechanisms, role systems, and role conflict are so remote from their measurement that we have no single, clearly required set of operational measures.

We believe this is a basic difference between the social and the natural sciences (Vol. 3, p. 471).

Examination of definitional practice—e.g., Rapaport's instructive analysis of how psychoanalytic construct language might be ordered to a concrete observation involving a slip of the tongue (Vol. 3, pp. 116–121), or Rogers' "case history" of the self construct (Vol. 3, pp. 200–212)—will show that rules of construct application are uniformly given in a way which leaves much to the discretion of the applier. The applier is typically expected to discriminate the presence, absence, or "value" of a "variable" within an intricately shifting pattern of events. The state of affairs "designated" is presumably associated with the most extensive range of phenotypes; moreover, the applier cannot assume that the same phenotype is always an indicator of the same "variable" (or "value"), in that given phenotypes are often conceived to be associated with quite different systematic "variables" or configurations of such. The applier must therefore simultaneously estimate and weight the "values" of a manifold of variables on the basis of a presented observation-pattern which (by presumption of most systematic accounts) is complex and unique. This simple story is for the *clinical* case. The case for *experimental* application (realization) is only less "fluid" to the extent that an arbitrary "simplifying" linkage is made which in effect puts each empirical variable of the experiment at the very thin end of a vastly tapered wedge originating at the "corresponding" systematic construct. Bear in mind, for instance, the oft-noted circumstances concerning the purely (and usually vaguely) illustrative character of the empirical variables set in correspondence with systematic terms, say, in most experiments designed to "validate" psychoanalytic principles. Far from validating or in some sense sharpening the theory, it is a matter of great good fortune when the experiment turns out—in the estimation of the proper language community—to be *illustratively* relevant.

In reporting later trends, we will have occasion to note other points which document flat discrepancies between metatheory and operational law. Thus, here we note only in passing the unanimous emphasis of all students of the person and the social setting—not forgetting others as

widely varied in approach as Tolman (Vol. 2), Lewin (treated by Cartwright, Vol. 2), and Guthrie (Vol. 2)—on so-called *psychological* definition of major systematic independent and dependent variables. To take the specific instance of systematic independent variables, it is strongly stressed by these men that specification of the principal antecedent conditions of action for phyletically high order organisms involves a specification of their inferred *meaning* for the organism. Concepts put forward to meet such a requirement cannot be justly defined by “standard” operational procedures without liberalizing such procedures out of all recognition—or identity.

If we may reduce to a single “trend” the most varied, searching, tortured, oblique grapplings, the central question would seem to be this. Granted that operational (or “reductive”) symptoms for systematic variables (whether independent, intervening, or dependent) form an open class—one which can be contracted or expanded on further empirical notice—*how open* a class of “observables” are we to presuppose for the application of concepts of the type we are forced to use in saying anything significant at the level, say, of human personality, social transactions, etc.? Every student of the person and the social setting in this study seems to be saying (in widely different ways) that their concepts have essentially *this* in common: *they are such that any application rule which relates a given concept directly or indirectly to an observation base of the sort specified by currently accepted criteria would involve something tantamount to an infinite disjunction of “operations” or “reductive symptoms”* (or, more fully, “test condition-test result conditionals”). And in fact it is more than doubtful whether *even then* the meaning of the concept would be adequately conveyed.⁸

⁸We speak here of “meaning” in so far as conveyed by *empirical* definition. The meaning of a systematic concept is never “adequately conveyed” by empirical definition alone; it is determined in a complex (and so far not well described) way by the position of the concept in the systematic or theoretical network, the meaning thus being constituted by other classes of definition as well: e.g., implicit definition, explicit definition. There is much uncertainty as to the relations among empirical and other classes of definition in scientific formulations; an uncertainty currently manifested by the difficulties philosophers of science are having with the explication of what is involved in the “interpretation” of formal systems. Even more fundamental, there is high uncertainty in the philosophy of science over the explication of *each* of the major definition-types, and especially *empirical* definition. Here it is well to note that though psychology stabilized its view of empirical definition during the early 'thirties in terms of various forms of the operational criterion, and, beginning in the 'forties, in terms of a loose fusion of the “operationist” account with such positivistic schemas as the “reduction sentence” (Carnap, 1936–1937), *technical* philosophical meaning theory was in continuous flux throughout that period, and increasingly so to the present day. The “official”

These inquirers are either explicitly noting or implicitly responding to the fact that their concepts and concept relations, no matter how inferred or validated, "designate" complex and often subtle relational attributes of observed phenomena, the "terms" (embodiments) of which relations are fleeting, labile, various, and easily blurred or masked by the simultaneous presence of innumerable other "terms" of an equally fluid character. More generally, these men are by way of asking: have we not been premature in extending a theory of empirical definition which holds as a useful approximation in physical science (or at one time seemed to) to psychological and social science? In so doing, they are asking whether it is at all sensible to expect any type of "theory" adequate to the phenomena with which it deals to be subject to the kind of "prophylaxis" presumably imparted by adherence to the intervening variable schema.

It should be noted, indeed emphasized, that the problems in the theory of definition so conspicuously opened up by these inquirers are not unique to them. Bearing in mind Tolman's doubts concerning "objective" pointer-reading, the epistemic complexity that learning theorists like Miller are beginning to acknowledge as characteristic of their systematic independent and dependent variables, and, indeed, the very conservative feelings of the same theorists re the entire question of established generality of intervening variable functions (which means, of course, validity "across operations" and "across measures"), it should be clear that a problem of precisely the same order exists even at these presumably "simpler" levels. Finally, we should note that Licklider (Vol. 1), in his penetrating analysis of auditory formulations, repeatedly points to the problem of indeterminacy in the linkage between his intervening variables and his final dependent variable as perhaps the most troublesome puzzle in his thinking. Even at the level of sensory theory, then, we do not avoid such difficulties. There can be no doubt that one of the major convergences in this study is a vast, if at the moment variably applied, pressure towards re-examination of our fundamental commitments with respect to problems of empirical definition, and thus, of course, a

definitional epistemology in our science has thus been long out of date in its (philosophical) area of origin. Though for some purposes it would be instructive to explore relations between the Study I trends re definition and certain of the newer, liberalized philosophical formulations, it would be unwise to do so in this place. For this epilogue is committed to *remaining within* the data of systematic inquiry in *psychology*, in the conviction that the type of methodological analysis of primary use to working scientists must center on the *work of scientists*. The final volume of the series, *Psychology and the Human Agent*, provides an appropriate context for examining the relations between philosophical and "indigenous" methodology, and surveying certain of the pitfalls—defined both by history and by principle—in problem solution by cross-disciplinary import, extra-disciplinary export, or indeed pursuit of the interdisciplinary common market.

reevaluation of the "unambiguous linkage" criterion implicit in the intervening variable schema.

II. PROBLEMS CONCERNING THE GENERALIZATION RANGE OF PSYCHOLOGICAL LAWS

From the discussion of intervening variables, it is already evident that concern with the conditions and limits of *generality* of lawful psychological statements is widespread and acute among the authors of these volumes. This *should* not seem surprising. Is not, after all, the quest for stable relationships having wide descriptive (and/or explanatory) spread relative to the domain of study *and* high predictive specificity, *definitive* of the scientific enterprise? Yet there is a sense in which explicit and searching concern with the question of generality can be seen as having momentous import. For there have been times during the Age of Theory when the prevailing view has come close to suggesting that highly specified laws of unrestricted generality (across indicators, measures, situations, individuals, groups, species, occasions, and other conditions) could be had almost by fiat. It is an interesting paradox that a climate in which investigators typically reported *experimental* results (in scientific journals) in the most "local" and situation-bound terms was at the same time one in which theorists (often the same persons) translated such findings into *theoretical* laws potentially adequate to "all behavior."

This lack of realism of the Age of Theory towards the conditions of generality is manifested, of course, in the strategy of the "defining experiment." It is shown in many other ways. It can be seen in the widely distributed belief that certain limited clusters of foundation data can, if "correctly" identified, provide the basis for "postulates" adequate to "all" psychological phenomena. It is evident in the related belief that such foundation clusters will result from the intensive investigation of a limited behavior-class under conditions established by a special experimental method. It is evident in the casual character of the rationale for interspecies transposability of findings (or principles based thereon); in the failure to show particular concern with checking the *intraspecies* generality of findings, or indeed their transsituational or any other kind of generality, not excluding trans-experimenter generality. It is evident in many other ways. Indeed, it is revealing that an era much preoccupied with the analysis of "generalization gradients" as a substantive psychological topic did not even begin to *classify* the "gradients" along which psychological systematists generalize their findings.

If in recent years psychology has bypassed concern with a "logic" of generalization suitable to its subject matter and phase of development, that concern comes through as something like a *leitmotiv* in the present

study. It is sounded in the most varied ways by different contributors and in varied contexts by given contributors. Here we supplement the findings and judgments re generality encountered in the discussion of intervening variables by mention of a few of the additional contexts in which relevant considerations arise.

1. Revivified Emphasis on Problems of Observation and Classification

Far from representing their formulations as *advanced approximations* towards comprehensive or highly general psychological theories, many contributors in these volumes may be found showing an intense and by no means neatly resolved interest in such pristine problems as those surrounding the conditions, techniques, and meaningful descriptive units for observation and classification in psychological science. To take scattered examples: There is penetrating interest in the strategy for achieving useful observational categories for the analysis of animal behavior under field conditions, as reported by Hinde in his discussion of ethology (Vol. 2). There is conspicuous concern with problems of descriptive classification registered by almost all learning and behavior theorists in the study, particularly in connection with the flurry of reanalysis (much of it of the most radical sort) of the stimulus and response concepts—in itself one of the most instructive convergences of the present study. Gibson (Vol. 1) shows a significant interest in the strategy of phenomenal observation as a condition for extending the classical concerns of sensory and perceptual psychology in a fruitful direction. Cartwright (Vol. 2) represents Lewin's constructs (and the initial task of psychological theory generally) as in the first instance a set of descriptive categories designed to represent psychological phenomena in a meaningful way. Skinner (Vol. 2) advocates the choice of "a basic datum" which will reduce scientific practice "to simple looking." Murray, and Asch (Vol. 3) eloquently argue the need for concepts which might prove descriptively adequate to the topography of experience as well as that of action. Almost all contributors to Volume 3 stress the need for significant observational analysis of behavior under "natural" conditions. Lazarsfeld represents problems of classification as basic to the social sciences.

Virtually all the investigators mentioned in the above paragraph are inclined to conceive of the status of psychology as still primarily at the level of a search for significant variables.

2. Shift Away from Single Species Preoccupation (and Related Matters)

The mere fact that these volumes make it possible to scan a wide range of systematic interests (across both subject matter and persua-

sion), refreshingly readjusts any picture which sees the rat as monopolizing attention. But more significant is an increased disposition to assess the limits of inter-species transposability of findings (e.g., Guthrie, Vol. 2; Murray, Vol. 3) and, within those limits, to ask realistic questions about the strategy of such generalizations (e.g., Miller, Logan, Vol. 2). Supplementing this is, of course, the trend towards phyletic diversification of subjects, as represented in this study by Hinde's analysis of ethology (Vol. 2), and by the emphasis on comparative materials of such physiologically oriented psychologists as Hebb, and Morgan (Vol. 1), and Harlow (Vol. 2).

In general, it can be said that only a few authors in these volumes would disagree with Guthrie when he says: "Practically all research results in prediction, but if it is merely the prediction of how rats will behave under certain complicated conditions found only in a number of psychological laboratories, we have not furthered knowledge or science" (Vol. 2, p. 173). And indeed, only the same few would find uncongenial Guthrie's further statement that: "The use of the laboratory rat almost exclusively as a subject escapes the complications of human learning, not because rat learning is essentially simpler, but because we are protected from many aspects of it since the subjects cannot contribute their own suggestions" (p. 193).

3. Conservatism re Limits of Prediction in Psychology

Another manifestation of concern with the problem of generality (in this case the *limits* of generality) may be found in the generally conservative estimates of the limits of prediction in psychology made by the authors in these volumes. There is not only a recognition of rather severe limits in principle imposed by well known characteristics of psychological subject matter, but realistic recognition of the contingency of *what* may be predicted on the systematic aims and methods. Moreover, there is a tendency to become quite specific in discriminating the multiplicity of senses in which "predictiveness" may be asserted as a property of a lawful or lawlike statement, and to recognize that not only the "content" (dependent variable event-classes) to be predicted but the mode of prediction is an option of the systematist. Illuminating discussion of such matters may be found in virtually all essays, but perhaps most explicitly in Licklider, and Postman and Tolman (Vol. 1); Tolman, Guthrie, Cartwright, Miller, and Estes (Vol. 2); and Thelen (Vol. 3).

Perhaps most significant, there is a growing disposition to recognize that certain *objects* of prediction may in some sense be intrinsically interesting and worth investigating, while others may not. If we may again refer to the quotable Guthrie: "A system may be productive of research, but research has no value in itself. It is knowledge that we are after rather than research and the test of a system is the light it throws on

an area, and in psychology, not just the amount of prediction it makes possible, but the ability to predict what is worth prediction" (Vol. 2, p. 173).

4. Increased Modesty of Aim and of Claimed Achievement

As one would expect from many of the points already made, it is fair to say that the study gives general evidence of an increased modesty in defining feasible goals of systematization, both long-range and short-range, and in representing the extent to which such goals have been realized. The contrast, in this regard, with characteristic Age of Theory ideology is sharp and poignant. Claims as to global applicability of theories or the joint achievement of high generality and "strong" quantitative specificity are nowhere made. Theorists having relatively broad-scope intentions for the most part stress the narrowness of the sectors in which these have even begun to be realized, while limited-scope systematists are showing increased interest in the relations between their areas and others, and in moving outwards towards wider domains.

III. THE OBSERVATION BASE OF PSYCHOLOGICAL SCIENCE: ITS RELATION TO THE LEGITIMATE END-TERMS OF SYSTEMATIC ANALYSIS

One of the deepest sources of agreement during the Age of Theory is in a common conception of the *legitimate observation base* of psychological science. Here again we must presuppose rather than reconstruct history. But it is fairly evident that, during the second decade of this century, the "objectivist" epistemology of behaviorism achieved strong dominance, and that by about 1930 it established virtually undisputed sway. Yet the transition interval leading from the era of schools to the Age of Theory (say 1925–1930) was marked by uneasiness over the mixed metaphysical-methodological *grounds*, and the inconstant criteria developed by *classical* behaviorism in defense of its epistemology. Psychology needed a clear and, so to say, "connotationally uncontaminated" rationale for objectivism, one based on *consistently* methodological grounds. The "operational" criterion seemed to provide this, as did later certain of the more sophisticated formulations of the empirical criterion of meaningfulness of the sort developed by logical positivism. The Age of Theory has generated an extensive and varied literature attempting an optimal rendering for psychology of one or another of the analyses of the conditions of empirical significance made available by the newer logic of science. And it has generated an even more varied range of practice in response to the resulting methodological "directives."

If *interpretations* of technical meaning criteria imported from the

philosophy of science were free and various, certain core beliefs concerning the legitimate observation base for psychological statements were common to all of them. It is significant that these commitments were historically prior to the importation of such meaning criteria, and that *after* importation they remained untouched by the frequent and radical changes in meaning theory, which continued in normal course of professional epistemological scholarship.

Such rock-bottom commitments concerning the observation base were partially characterized in considering the "determinate linkage" problem as raised by intervening variable doctrine. Here—still in crudest caricature—we supplement the account. Perhaps fundamental are these:

a. All lawlike statements of psychology containing dependent variables not expressible in, or reducible to, publicly verifiable and thus "objectively" observable *behavior* indices are to be excluded as illegitimate. More positively, this assumption stipulates that *dependent variable terms* of the observation base designate referents which meet the test of *independent simultaneous observability by a plurality of observers*. Such dependent variable terms are to be defined in the same observation terms as are at the basis of physical science (weak form), and perhaps are even translatable into or reducible to actual descriptive and explanatory concepts of physics (strong form; more characteristic of classical than of neo-behaviorism). The prototypical case of an admissible dependent variable is, of course, the notion of *response*, or more specifically a "measurable" *index* of response, in some one of the varied, yet often unspecified, meanings of "response."

b. Similarly, it was demanded that legitimate *independent variables* of psychology designate referents which can pass the test of independent, simultaneous observability *and* are definable in either the observation language of physical science or the concepts of physics. It should be noted that in the case of the independent variable, the strong-form requirement of translatability or reducibility to the *concepts* of physics has retained more general currency (as, e.g., in the "physical energy" criterion for the definition of the stimulus) than the analogous requirement for the dependent variable. The prototypical case of an admissible independent variable was, of course, the notion of the stimulus, again in some one of many rather unseparated meanings. It has often been presumed, however (especially by neo-behaviorists), that among admissible independent variables are also certain intraorganismic "states" of a sort wholly or partly unspecifiable in stimulus terms, but such that the indicators for which, or operations for the manipulation of which, can also be expressed in physical observation language.

During the Age of Theory, these assumptions were embedded in, or rendered into, the language of the various "operational" or empirical meaning criteria imported from the methodology of science. The rather casual character of the relation between such technical criteria and these commitments concerning the observation base is indicated by the widespread presumption that mere use of the language of stimulation and behavior, "S" and "R," entails a built-in guarantee of semantic significance.

The commitments re the observation base of psychological science are at so fundamental a level during the Age of Theory that no *given* reconstruction can sound or be "right." Yet if anything is central to the age, it is some such set of attitudes. A telling measure of the strength of their hold is that individuals whose problematic interests are clearly compatible with a quite different epistemological rationale have often made a point of squaring their interests with such commitments. The rather fluid semantics over time of the word "behavior" gives well known evidence for this tendency; originally conceived as a class of events having some functional relationship to effector processes, this term has been increasingly enriched by the most various adjectival extensions (as, e.g., central behavior, cortical behavior, perceptual behavior, conscious behavior, fantasy behavior, etc.). Also significant is the fact that individuals whose problematic concerns have in recent decades caused them to bypass the dominant epistemology in important respects have given little explicit attention to these implications of their work. In general, it is a fact of some interest that during an interval of the science characterized by frequent and dramatic systematic conflict, there have been few direct challenges to the prevailing conception of the observation base.

Coherent with (if not enforced by) the behaviorist emphasis on phenomena presumably designated by the S and R end-terms of systematic analysis are other characteristic aspects of the behaviorist system of orienting attitudes. Thus, we have the well known stress on peripheral behavior determinants generally, and the corresponding de-emphasis of central and perceptual factors. We have the characteristic interest in laws relating environmental stimuli to behavior and the corollary bypassing of laws of cognition. It is difficult to say, of course, where behaviorist epistemology shades into behaviorist theory or pre-theory. It is important, however, to see that we have here a relatively organic set of commitments, one which has seemed remarkably stable over time, being common both to classical and neo-behaviorism. These orienting attitudes are so related that the fruitfulness, plausibility, etc., of any given one must be some function of the fate of any and all of the others as the behaviorist program becomes translated into action. Thus, in the present section we place primary emphasis on findings of the study rele-

vant to the conception of the observation base, but we make limited reference as well to factors which form a cluster with basic behaviorist epistemology.

An outstanding trend of the study is the presence of a widely distributed and strong stress against behaviorist epistemology (both in the narrower and broader senses just distinguished). It is evident from all sides. It is strikingly evident among behavior theorists who themselves have powerfully molded behaviorist tradition. It is evident among "near-behaviorists" like Tolman, whose methodological behaviorism has become more vestigial, both in its actual effect on his theorizing and in the role he imputes to it in his metatheorizing. Many of those whose problems have *not* been set by the emphases of behaviorism seem more disposed than formerly to question the adequacy of its epistemology—even if they are not always ready to relinquish the "objectivist clang" of the independent and dependent variable language which has seemed so necessary a condition of respectability in recent decades.

In sampling a few of the trends, we begin with (1) the radical reanalysis of stimulus and response (and related developments concerning the specification of "basic" systematic independent and dependent variables) evident in many of the essays. We then briefly consider (2) the generally increased interest in perception and central process on the part of certain S-R theorists, and (3) the evidence for a revived concern with experiential analysis.

1. Reanalysis of S and R

a. Among S-R theorists. Though stimulus and response have in some sense always been under reanalysis, it is rare that this enterprise has proceeded with the abandon evident in the present study. The radical flavor of the trends towards reanalysis of the stimulus and response concept is well conveyed by these words of Neal Miller:

In general, stimulus-response psychologists have tended to bypass problems of the type we have just been raising [the definition of S and R]. By intuition and trial and error, they have concentrated on experimental situations in which the stimulus and response were so simple and manageable that the lack of more precise definitions or laws concerning these variables was not a practical problem. Using such situations, stimulus-response psychologists have concentrated on determining the laws governing the connections of responses to stimuli. *Thus, stimulus-response psychologists may be said to know and care relatively little about either stimuli or responses; they are specialists on the hyphen between the S and R and could more aptly be called "hyphen psychologists," or to use Thorndike's term, "connectionists"* (Vol. 2, p. 242; italics mine).

The *direction* in which current reanalysis is tending is well symbolized in some incisive and brilliant paragraphs of Guthrie's essay. Since Guthrie is there evaluating assumptions concerning the treatment of S and R of an entire generation—one which includes himself as a distinguished member—it is well that we quote him rather fully:

What, then, will be the terms in which general rules of behavior are stated? What will constitute the antecedents and what the consequents? What can we look for as stimuli and what as responses? . . .

In the physical sciences, this problem has been solved by minimizing the role of the observer and by the use of conventional instruments of measurement with the attainment of a high degree of objectivity, . . .

In psychological observation it would, of course, be a great advantage to reduce both stimuli and responses to this point of high agreement. *But there is reason to believe that this reduction cannot ordinarily be obtained in psychology. The phenomena in which the psychologist is interested are not specified in terms of mass, length, and time. They involve categories not reducible to position on a scale. In fact, they normally involve patterns of situation and movement that require recognition by a human observer, and this recognition is of an order indefinitely more complex than the recognition of relative position involved in comparing a length with a scale* (Vol. 2, p. 164; italics mine).

During the first century of the development of psychology, we have made great efforts to be objective. We hoped to achieve this by limiting ourselves to the categories of physics and using as the weather signs of behavior only the physical or chemical events normally activating sense organs. The determinations of absolute thresholds in the various senses, the hope that response could be treated just as movement in space which was the crude interpretation of behaviorism, failed to carry us very far toward the understanding of behavior. The reason for this is that we cannot reduce the classes of psychological facts which make up the data we must deal with to component movements in space. Patterns of stimuli and patterns of response have their psychological significance and usefulness tied to their patterning—pattern as pattern must be recognized and dealt with. *Machines can be devised to respond to pattern, but the human observer remains the only practical tool we have for the recognition of patterns in their variety and multiplicity.* . . .

The history of our effort to use as the weather signs of behavior simple physical or chemical changes involving sense organs is an interesting one. One difficulty it encountered was that these stimuli did not always stimulate . . . *But the real failure goes deeper. The patterns of physical change that occasion response, we find ourselves inevitably describing in perceptual terms. It is not enough that they be available in the physical situation nor is it enough that the organism's attention orient sense organs to receive them; it is further necessary that they have meaning for the responding organism* (p. 165; italics mine).

To object to treating a simple physical change as a stimulus or a

muscular contraction as a response is not to deny that all stimuli are analyzable into such physical changes or to deny that any specific response is analyzable into muscular contractions and glandular secretions. That should be assumed. *It is, however, a denial that the psychological description of behavior can be made in physical terms. It requires psychological terms which will name recurring patterns of physical change usually requiring identification by an observer which will include recognition of their stimulus value usually judged by time relation to the response* (p. 166; italics mine).

It is already evident that the leading proponent of contiguity theory and a leading representative of reinforcement theory agree in making a severe judgment of the practice of past decades in explicating the nature of "stimulus" and "response." Beyond this, there is much in the *positive analyses* of Guthrie and Miller that is consonant.

The "liberalization" of S-R theory put forward by Miller in his essay is very liberal indeed. Nowhere is this more clear than in the discussion of his method of "functional behavioral definition." In this sequence (Vol. 2, pp. 238-242), Miller draws together and states more boldly than ever before certain tendencies in the treatment of S and R long present in his work. In his present statement, "a response is any activity by or within the individual which can become functionally connected with an antecedent event through learning; a stimulus is any event to which a response can be so connected" (p. 239). It is clear from the discussion at this place that, like Guthrie, Miller is disposed to cut S-R theory off from any "physical energy" criterion of the stimulus. It is clear, too, that Miller, like Guthrie, would consider the determination of any given S or R as an experimental or empirical-observational problem. It is clear, also, that both would now think of S or R in an abstract and, so to say, "ontologically neutral" way as corresponding to *any antecedent condition* which can be shown to have a stable relation to any *consequent*, or vice versa. There would be further accord in acknowledging that such antecedents and consequents are variable event classes (different instances of which occur from occasion to occasion), and that they are in every case *constructions* or *discriminations* made by the observer.

What is not equally clear (though by no means ruled out) is whether Miller would put the same stress as Guthrie on the need to specify stimuli in "perceptual terms"—in terms which acknowledge that "it is . . . necessary that they [stimuli] have *meaning* for the responding organism" (p. 165; italics mine). On Miller's conception, this might or might not emerge as a constitutive property of stimulation from application of the method of "functional definition."

Though in other S-R formulations of the study we do not find evidence of comparably radical departures from previous practice in the

treatment of S and R, there is still definite responsiveness to certain of the difficulties bequeathed by earlier treatments. Thus Logan, in his reconstruction of the *empirical* independent variables for the Hull-Spence approach, cites a variety of "physical energy" examples as constitutive of the *systematic* independent variable, "stimulus" (Vol. 2, p. 315). But after detailing Spence's distinction between the "situational stimulus" (which "can be described in such physical terms as frequency, amplitude, wavelength, etc.") and the "'proximal' or 'effective' stimulus" ("that fraction" of the situational stimulus "which is perceived at any one time"), he adds:

The effective and situational stimuli are not isomorphic, but the rules by which one determines the effective stimuli from knowledge of the situational stimuli are not yet very fully understood (p. 314).

With respect to *response*, Logan quite illuminatingly points out:

As with the independent variables, adequate definitions of the systematic dependent variables have not been made at a high level of abstractness. There are formidable problems to be resolved when one attempts to formulate such definitions. For example, latency is the time required to initiate a response. If one is running rats in an alley, one must decide how rigidly to control the subject's orientation at the time the door is opened, whether to provide any distinctive ready signal, and when to define the response as having begun. Certainly one's measures are affected by these decisions. And comparable problems of selecting criteria are involved for each response measure (p. 326; italics mine).

Perhaps the most conservative note struck by any S-R theorist in the study is Estes' statement that ". . . by *stimulus* and all variants of the term I refer to environmental conditions describable in physical terms without reference to the behavior of the organism" (Vol. 2, p. 455). This note is at once modulated in the next sentence which defines "*stimulating situation*" as "all sources of stimulation that are mentioned in the experimenter's description of the experimental situation . . ." And it is further modulated by the fact that in Estes' theory the "S" is a set-theoretical concept conceived as a statistical population of elements which fluctuate from trial to trial. It is this (hypothetical) population of elements which is conceived in "physical terms without reference to the behavior of the organism." In an *application* of the theory, the set-theoretical construct is placed in correspondence with an *experimenter-described* "stimulus situation," the accuracy of which "we must, initially at least, assume" (p. 456). In the Estes formulation, the experimenter-discriminated *stimulus situation* is the empirical independent variable-class constitutive of the "physical" S as a *systematic* variable.

To this run-down of S-R theorists' positions on S and R, a final word should be added concerning Skinner. Skinner has not in his essay (Vol. 2) addressed the present issue. But it should certainly be acknowledged that Skinner has an ancient priority on the view that dissociates S and R from physical energy or specific movement criteria, and sees them as both experimenter-discriminated and experiment-defined. However, other trends of the present analyses such as Guthrie's emphasis on meaning, on perceptual specification, and in general on the importance of qualitative observation by human agents (in contradistinction to automatic recording), and Miller's utter flexibility in conceiving S and R to be applicable to *all* antecedents and consequents whatsoever (including central process and other hidden matters)—such trends had *not* been anticipated by Skinner. Nor is it likely that he would condone them.

We have considered the ferment of reanalysis, *among S-R theorists*, of S and R of such high interest as to merit extended treatment. But its true significance can be seen only within the entire pattern of findings germane to S and R. Here we can only briefly give the drift of widespread analysis by many authors. To simplify the task, we restrict consideration to findings concerning the "stimulus."

b. Treatment of stimulus variables in sensory psychology. The sensory psychologists in the study (Licklider, Graham, Pirenne and Marriott; Vol. 1) have a message of profound importance for behavior theorists and indeed all concerned with the systematization of "post-sensory" processes. As cogently stated by Conrad Mueller (Vol. 3, pp. 791-797), it is that in sensory psychology there is *great diversity* in mode of specification of stimulus variables—diversity in specificity, in complexity (i.e., "the length of the chain of definitions linking them to experimental procedure"), and in other ways.

Mueller emphasizes that "perhaps the greatest diversity" is in "the extent to which the experimenter adopts the language of physics," indicating that stimulus language in technical experimental contexts can range from the "extra-scientific language" of ordinary "objects" to the language of physical dimensions, and indeed to extensions of these dimensions (including "more and more of a commitment to theory, either physical, physiological or behavioral") prompted by the requirements of specific problems. He also stresses that stimulus specifications can "differ in terms of the extent to which the organism is involved in the definition," some stimulus terms being describable without such reference, but many others (e.g., the specifications of color stimuli) requiring incorporation of complex combinations of behavioral and physiological data and guesswork.

The general picture that emerges is one of thoroughgoing contextualism in mode of stimulus specification—a contextualism which always represents a creative response to the requirements of specific problems. The sensory investigator deals with his major “antecedent conditions” not at one homogeneous, prefabricated physical level, as current stereotype concerning sensory psychology would have it, but at varied and shifting levels which are always problem determined. The airing of this message, so copiously documented by the sensory contributions, in mixed company could have a most liberating effect on systematists in other areas. *It could embolden them to be similarly contextual and problem-centered in the identification and analysis of their independent variables* (empirical and systematic). It suggests further that the definition of the “stimulus” (i.e., as a systematic variable), or the “constitution of the S-class” is not, as ordinarily conceived, one problem but many problems—that even within a *given* systematic formulation (particularly if it lay claim to reasonable generality) it may be idle to seek some single defining property or specification-mode for S, or indeed any other “class” of antecedent conditions playing an analogous role within the formulation. If this message is liberating, be it noted also that sensory psychology stands as an impressive reminder that conceptual freedom and discipline are *not* incompatibles.

Sensory psychology contains other lessons of importance for the outside. In reading an essay like Graham’s on color vision (or, for that matter, Pirenne and Marriott on the quantum analysis of brightness vision, or Licklider on audition) it is only too obvious that sensory psychology has won its way to appropriate degrees of specificity in the identification of independent variables slowly and painfully and via the most complex interactions—sometimes cumulative and sometimes discontinuous—between hypothesis formation and experiment. It is also obvious that despite enviable advances in this oldest branch of psychological science, a vast range of problems, *including* ones of stimulus specification, is vastly open. We do not pretend that difficulties of the same *type* will confront systematists in areas the analysis of which demands independent variables of quite different types (including ones to which the metaphor of “stimulus” cannot be justly stretched) from those requisite for the analysis of sensory mechanisms. But the experience of sensory psychology should serve as a sobering reminder to those who anticipate sweeping or rapid progress in systematic formulations which aim towards even moderately ambitious combinations of generality and specificity.

c. “Stimulus” in perception psychology: the instructive case of Gibson. If problems of stimulus specification as posed by classical sensory psychology are still highly open, the *perception* psychologist, Gibson (Vol. 1), reminds us that there may be an indefinitely large range of

questions concerning stimulus specification of a type not only open, but *unposed*. Gibson points to the possibility that there may be many dimensions of stimulus specification—the “higher order variables” of stimulation—which have not been touched by classical sensory psychology. And he makes this possibility concrete and convincing by identifying certain variables “of adjacent and successive order” as, e.g., gradients specifying the “texture” of the “optical array,” which seem related in orderly ways to variables of experience and reporting behavior, some of which latter have also eluded discovery by classical psychophysics. Others of these dependent variables are of a sort which, if previously recognized, have not traditionally been regarded as in any direct way controlled by variables of stimulation. Gibson thus depicts for us the outlines of a heretofore neglected science of stimulation—one which could have profound consequences for a rephrasing of traditional questions of psychology.

Gibson’s program, of course, is that of achieving, by combined strategy of phenomenological analysis and experimentation, a specification of dimensions of perceptual experience and behavior in *physical* stimulus terms. But it should be stressed that in Gibson’s view, much of the relevant physics does not exist: “. . . the physics appropriate for the study of the perception of surfaces remains undeveloped” (p. 470). Nor, by the same token, does a suitable metric exist for many of the “higher order” stimulus variables. Yet the “simple co-ordering of judgments to stimulus variation can proceed without the sophisticated procedures of modern psychophysics” (p. 499). We thus have a bold program which looks towards a vast extension of the range of *physically* specifiable “stimulus” variables, but one which by seeming paradox (as against behaviorist epistemology) can only be advanced via experiential analysis.

There are several important morals here for problems connected with the legitimate observation base. Perhaps most instructive is the fact that Gibson, mainly on *phenomenological grounds*, looks forward to a far-reaching extension of the range of independent variable conditions which could come under physical specification, *at precisely the same time* that behavior theorists, on “*behavioral*” grounds, have become definitive in their relinquishment of a physical energy criterion of S. Any residual tendency of people to see a fixed tie-up between behaviorist epistemology and physicalism on the one hand, and phenomenologism and some heady type of anti-physicalism on the other (a view not in any case justified by history) can find little nourishment in this circumstance.

d. “Stimulus” and related variables in personality and social formulations. Formulations of the person and the social setting have been somewhat more free of a “stimulus” idiom for the characterization

of major independent variables than those less directly addressed to "complex" man-pertinent processes.⁹ Nor is it easy to generalize practice in these areas, because of the great variability of systematic aims and of conceptual posit. The fact that recent methodology has tended to bypass systematic experience in these areas is regrettable in that it could well be that preoccupation with man-relevant problems has led to certain lessons concerning problems of identifying and phrasing important independent variables which could not otherwise be learned. It is thus unfortunate that fuller consideration of the implications for this issue (and related ones) of practice in the epistemically more "complex" areas must be reserved for another occasion.¹⁰

It should first be noted, as part of the picture of variability, that in certain of the formulations there is relatively little concern with independent variables of a sort functionally analogous to "stimulus" notions (e.g., environmental inputs). This lack of concern is less a matter of principle than it is one of problematic priorities and perhaps practical feasibilities. Thus in formulations like those of psychoanalysis (Rapaport, Vol. 3), Lewinian personality theory (Cartwright, Vol. 2), client-centered therapy (Rogers, Vol. 3), and perhaps the Katz-Stotland type of attitude formulation (Vol. 3), the interest at this phase is mainly in the working out of concepts—often of rough grain—for the specification of *intra-personal* processes. In some of these theories, notably the Rapaport version of psychoanalysis and the Cartwright version of Lewin, there is keen appreciation of the neglect of *relations* between the intra-personal processes and structures on the one hand, and the "objective environment" on the other. In the case of other formulations represented in the study (Asch, and especially Murray, Vol. 3) there has been much explicit interest all along in problems having to do with the conceptualization of environmental relationships.

The major generalization that can be made is *the striking convergence—if not coalescence—of the current "liberated" S-R handling of the stimulus, and the common-denominator of practice among personality and social systematists in the treatment of environment-variables.* To those who have seen the methodological problems of psychology primarily in terms deriving from the study of learning theory, it will be illuminating to discover that the present "convergence" is largely uni-

⁹ This problem-instigated departure from convention has not in general been paralleled on the *dependent variable* side, where the language of "behavior" in some one of the many inflated forms of its *initial* sense (i.e., as being some function of *effector* activity) has wide currency.

¹⁰ Such matters are more extensively addressed in *Psychology and the Human Agent* (Vol. 7).

lateral: it is the S-R theorists who have moved and the man-preoccupied systematists who have (relatively) stood still.

Thus, for instance, it is most instructive to read, with the spirit of the present Guthrie-analysis of S in mind, the brief sequence (Vol. 3, pp. 26ff.) in which Murray recapitulates his concept of "press" and "latent-press." After persuasively developing the point that "it is not so much the physical attributes as such but the known or supposed man-pertinent capacities of objects which influence behavior," Murray introduces his press notion as the basic environment-specifying concept in his thinking. The press is a "subject-pertinent" property of an object (animate or inanimate, but most typically at Murray's level of analysis a property of an "alter"). It is, of course, jointly constituted by an environmental object-property and the perceptual or, as Murray would say, "apperceptual" processing of the subject. Translated into a "stimulus" idiom, the press could be characterized as a stimulus-cum-meaning relative to a specific subject. The "objective" definition of a press would be via an apperceived construction by "the psychologist, by selected judges, or by the conventional majority" (p. 27). This can, of course, be distinguished from the subject's protocol or the subject-definition as inferred by less direct means (e.g., projective tests, behavior indices and contexts, etc.). Comparison of such a treatment of "environmental" antecedent conditions with the extended quotation already given from Guthrie will show agreement on virtually every point. Nor is there anything flatly incompatible between Murray's account of "press" and the tendency of Miller's present analysis of S.

The same types of requirements which Murray attempts to meet in the concept of press (and related considerations) are responded to in a variety of ways and at varying levels of explicitness by all other students of the person and the social setting. The important fact is that they are universally *acknowledged*. This is manifested, for instance, by the strong emphasis of the systematists in this group on the need for "psychological" definitions of "environmental" variables at levels of analysis suitable to their problems. There is general recognition that "environmental" variables must be specified as systematically complex entities, most of which (for purposes of the problems engaged by these men) are artifacts of a human environment which embody either conventionalized or idiosyncratic meanings and which, moreover, enter the "causal equations" of experience or action as mediated by specific perceptual and cognitive processing by the "responding" organism. It seems agreed also that the "values" of such variables can be assigned in only one way: by individual or pooled experimenter (or observer) inferences which are themselves "perceptual" discriminations of a most complex sort.

It is clear from the contributions in these areas that little progress has been made with regard to a "logic" of environmental variable-specifications. Whether there is a "logic" or a set of such, other than rough and ready rule-of-thumb solutions to such matters, would seem a genuine question. Certainly no general definitional device or paradigm will handle the relevant problems. Indeed, we have seen that a *general* solution is precluded even at the level of sensory psychology, one of the few areas in which the use of "stimulus" language does not involve an abuse of metaphor. The *contextualism* of sensory psychology in its modes of specifying independent variables must be multiplied many times over at the level of analysis occupied by current "behavior" and learning theory. And this value must be multiplied by a still more generous factor at levels corresponding to the manifold interests of personality and social theory.

2. Increased Interest in Perception and in Central Processes

Here we restrict attention to certain dramatic changes, as against earlier Age of Theory practice, in the extent and character of the concern of S-R theory with perceptual, cognitive, and central processes. As is evidenced by recent widespread interest in problems of perceptual learning, such changes have been under way for a number of years. But they are documented with particular force in this study—especially in the papers of Guthrie and Miller.

It is well known that earlier neo-behaviorists tended to bypass concern with perception and other "central" phenomena *at the level of primary principles* in the hope that such matters might ultimately be dealt with as secondary or "derived" phenomena. A transition seems to be under way towards regarding these problems as requiring analysis at the very *foundations* of the scientific enterprise. For Guthrie, such analysis not only is taken as necessary to the proper conceptualization of the end-terms of systematic analysis (S and R), but also is seen as integral to the specification of any fundamental law which can aid in identifying the conditions under which learning takes place. Though Miller's approach does not demand that the latter condition be met, it certainly suggests that any "definition" of S and R must be so set up as to provide for the expectation that S will often be such as to require perceptual specification and to acknowledge the possibility that final R will often be contingent on complex central or cognitive processing.

To turn first to the Guthrie position, the seriousness of his redefinition of S and R is vividly registered in the radical alteration of his major (some would say his only) "rule" for identifying the conditions under which learning takes place—the principle of association (Vol. 2, pp. 185–189). The character of the change is evident from the short form

of Guthrie's new principle which reads "*what is being noticed becomes a signal for what is being done*" (p. 186). The corollary emphasis of Guthrie on the analysis of "attention" ("Attention becomes, in the present account, the point at which learning occurs") certainly betokens a profound shift in the direction of acknowledging central determinants of behavior, even if Guthrie prefers to hold on to the peripheralist language of stimulus and response for the phrasing of relationships at such "central" levels. Further, if indirect, acknowledgment of the influence of intraorganismic processes (not excluding, one takes it, central events) is revealed in Guthrie's statement that:

The complexity of the nexus of determiners of action requires that prediction allow for high degrees of error. Most instances of associative learning are cited after the fact, and do not constitute prediction (p. 189).

Turning now to the Miller position, it is well first to indicate that he is still soberly disposed to stay within the neo-behaviorist framework of orienting attitudes re central process. Thus, for instance, he says: "Although recognizing that much of man's behavior involves cognitions, I have preferred the strategy of trying to explain such behavior as the outgrowth of simpler, noncognitive mechanisms" (Vol. 2, p. 262). Miller's interests in the phenomena of cognition, as shown in the present mobilization of his thinking, are in fact so extensive as to make the preceding disclaimer distinctly necessary. The flavor of his approach to such matters is given in his discussion of "Thinking: central cue- and drive-producing responses" (pp. 242-248). He there points out:

One of the most important advantages of functional definitions of stimulus and response is that such definitions can be applied to central as well as to peripheral events. Instead of emphasizing anatomical location, our definitions direct attention toward the more significant problem of functional laws. *These definitions free the S-R theory of thinking from being restricted to proprioception, allowing the theory to exploit images, response-produced drives and rewards, perceptual responses, perceptual learning of acquired distinctiveness or similarity, and the possibility that central responses can contribute to the focusing of attention* (p. 242; italics mine).

The character and range of the interest in central processes is further illuminated by Miller's interesting discussion of "Relational responses to relational cues" (pp. 248-252) which he acknowledges as having importance in much behavior. In a brief section concerning "Multiple cue and response potentialities," Miller gives further evidence of the extraordinary flexibility of his conception of S and R:

By now it should be apparent that I believe that most stimulus objects present the organism with a multiplicity of potential cues. Thus, the organ-

ism may learn to respond to the absolute position of a stimulus object, to its relative position, to its absolute brightness, to its relative brightness, to its color, to its form, to its being the one object that is different, etc.

In the following paragraph he says:

Some of these cues may be relatively direct products of end-organ stimulation, *others may be the result of various levels of innate mechanisms for analyzing and processing such stimulation*, and yet others may be the result of learned cue-producing responses (p. 251; italics mine).

From all the above, it should be clear that Miller is not only justified but documenting an important historic trend when he says (p. 243): "It is obvious that the postulation of central responses, such as perception and imagery, reduces the gap between S-R and cognitive theory." When, in the next sentence, he says "there is still a difference in that we clearly assume that these central processes follow the same laws as do peripheral stimuli and responses," one can only wonder to what extent nature will cooperate with man's verbal preferences.

3. Revivified Concern with Experiential Analysis

Whether behaviorist epistemology is *logically* incompatible with the systematic utilization of experiential analysis is a question much in need of clarification, but one beyond the scope of this epilogue. It may indeed be possible, on *some* interpretations of the role of verbal report, to make a plausible case for the thesis that all meaningful questions having a presumptive experiential frame of reference can be dealt with, in principle, by behavioristic methods. But no one can deny that for more than forty years behaviorist epistemology has had the *pragmatic effect* of fostering a set of attitudes which tend to either devalue or divert attention from most problems which, by virtue of historical or extra-scientific associations, have an "experiential odor"—quite independently of whether the investigator believes the problem compatible, in principle, with behavioristic methods.

An important and quite general trend of the essays is an increased recognition of the role of direct experiential analysis in psychological science. This trend cannot be represented as a kind of phenomenological revolution. It is expressed quite variably, usually in tones which are either cautious, oblique, or qualified, and most often in a way which grants experiential analysis only the second-grade legitimacy of an accessory device to hypothesis formation. And even when experiential variables are considered legitimate or indispensable elements of system-language, there is relatively little *positive* consideration of the definitional and other methodologico-strategic questions thereby introduced. But the trends are definite and could be prognostic.

The general pattern of findings can best be indicated against the distinction implicit in the last paragraph between presystematic, "accessory" uses of experiential analysis and explicitly systematic uses for purposes of constructing variables having undisguised experiential reference and technical significance within a system- or "theory"-language. We summarily consider a few of the findings within these terms.

a. "Presystematic" analysis of experience. No one will dispute that experiential analysis in this sense is a necessary condition to the psychological enterprise. Nor has this point ever been disputed, even though at times it has not been widely advertised. As against the temper of the past few decades, however, we can say that there is evident in the thinking of Hebb and Helson (Vol. 1); Miller and Guthrie (Vol. 2); and certainly in the "insightful anthropomorphism" of ethologists like Hinde and comparative psychologists like Harlow (Vol. 2) a more far-ranging, sensitive, and explicit dependence on presystematic phenomenology than has been characteristic of earlier Age of Theory practice. Experience, and in general the phenomena and involvements of human life have been utilized as the matrix of problem and hypothesis formation, of initial estimates for the plausibility of assumptions, etc., in a more direct and less apologetic sense than has been usual.

b. Some transitional cases. Several authors in the study may be characterized as holding an attitude towards "experiential analysis" which falls somewhere between its presystematic and its systematic use. The most instructive case is that of Tolman who, as we have already seen in the discussion of intervening variables, now expresses the dependence of his theorizing on his own phenomenology in a way which makes the objectivist nuances of his theory language broadly metaphorical. The clear statements to this effect contained in the extended quotations from Tolman towards the beginning of this epilogue (pp. 736-737) are echoed at many places in his essay. Where it is not within the lines it is between them. In fact, after reading Tolman's presentation, it is more difficult than ever to avoid the impression that much of the power of his thinking derives precisely from his use of, or fidelity to, "common sense" conceptual categories of a cognitive sort. One feels more strongly than ever that whatever the inadequacy of the assumptions made in such a vocabulary, they will probably be in some sense less "vicious" than assumptions which, because of a principled commitment to some simplistic vocabulary, are forced into abusing and distorting ontology.

Also in this transitional group are certain of the students of the person and the social setting whose tendency to hold on to the language of "behavior" and associated imagery is rendered obsolete by the nature of their problems, their predictive aims, the character of their variables,

and even certain of their expressed metatheoretical attitudes. Time after time among these systematists we encounter circumlocutions and euphemisms which are being made increasingly unnecessary by the implications of their own work.

c. "Systematic phenomenology." Here we merely acknowledge that men like Gibson (Vol. 1), Prentice (representing Köhler, Vol. 1), Cartwright (representing Lewin, Vol. 2), Asch, Murray, and Rogers (Vol. 3), all regard variables having direct experiential reference as legitimate elements of systematic analysis. And most systematists in this group explicitly introduce such variables. Some among this number, like Gibson, confine the role of experiential variables to the systematic-*dependent* side of their relationships. Others, like Prentice in his analysis of Köhler's theory, regard "phenomenal" variables as either dependent or intervening but, at least by implication, proscribe them from the systematic-*independent* category. Still others, like Murray, are willing to contemplate the utilization of experiential variables at any position in the systematic array.

In general, though, it can be said that whatever the attitude taken towards experiential analysis, there has been no marked tendency among authors in the present study (with the single exception of Henry Murray) to join in any explicit way the many methodological and empirical questions that might be asked concerning fruitful and rigorous utilization of experiential data. Certainly the manifold current stresses against behaviorist epistemology invite such questioning. Yet issues concerning optimal techniques for experiential observation, the formulation of adequate dependent variable categories, the integration of behavioral and experiential data, the construction of theoretical concepts from experiential data, etc., have been addressed by indirection, if at all.

Summary re Observation Base

In this section on the observation base and its relation to the end-terms of systematic analysis, we have laid before the reader several trends which seem to define a growing stress—both internal and extrinsic—against behaviorist epistemology. Since, during the Age of Theory, certain core assumptions of this epistemology have determined the reigning conception of the observation base of psychological science, that conception seems called into question. As we have already seen, this same conception is sharply challenged by those findings of the study discussed in connection with the "unambiguous linkage" criterion of the intervening variable paradigm. The current analyses of S and R at the level of molar behavior theory make it fairly safe to say that use of these end-terms of systematic analysis has involved no guarantees of empirical significance or meaningfulness. If stimuli and responses are acknowledged

to depend for their identification on the perceptual sensitivities of human observers, then the demand for something tantamount to a language of pointer readings, whether as simple energy-source or movement descriptions, or as disjunctions of fixed stimulus "indicators" and response "measures," must be given up. And if this demand be given up, then much time-worn argumentation as to the intrinsic ambiguity of an experiential language, or in fact any language the end-terms of systematic analysis in which are not S and R, becomes idle and beside the point. If, further, the requirement is asserted that S be specified in a way which includes its inferred meaning for the organism, then *any* basis for a difference in epistemological status between an S-R language and what has been called "subjectivistic" language is eliminated. There may be objectivistic paradigms for the "representation" of meanings in certain simple cases; there is no behavioristic paradigm for their *determination* in most of the human cases that count.

If we consider the trends connected with S and R and add to them the other trends discussed in this section, we may conclude that no one of the descriptive *patois* available to psychology at the present time has a privileged status with regard to semantic significance—on any criterion of meaning. From the point of view of semantic purity or innocence, we can just as well talk a "crypto-objectivistic" cognitive language *à la* Tolman, an experiential language *à la* Murray, or a hypothetical construct language *à la* Hebb as we can a homogeneous language of S and R. The trends in this section, along with those in earlier sections, suggest there to be grave defects in the analyses of empirical significance that have ruled psychology for the past several decades. A re-examination of the theory of definition of a sort adjusted to the realities of practice and the demonstrable business of psychology seems clearly indicated.

IV. MATHEMATIZATION OF SYSTEMATIC RELATIONSHIPS

Perhaps the most passionate Age of Theory demand has been that for the mathematization of systematic relationships—preferably at levels of quantitative specificity at least comparable to classical physics. Especially during the mid-phase of the Age of Theory (say 1935–1945), it seemed to most a matter of course that the goal of science, and thus of psychological science, was over its *entire* range the statement of mathematical laws. The reconstruction of theoretical practice in physics which governed Age of Theory ideology seemed to make attainment of this goal a matter of destiny—in the expectation of many, rather short-range destiny.

In striking contrast to this requirement, it is a fact that throughout the early and classic Age of Theory, only a narrow range of mathe-

mathematical strategies was tried out (at least at the level of theoretical relationships having reasonably general intent). For the most part these were modeled quite literally on the use of mathematics in certain of the simpler contexts of classical physics—usages ranging from simple algebra and analytic geometry to application of differential equations. Lewin's programmatic use of topology might be noted as a lonely exception. The strategies ran a rather narrow gamut from the rational assumption of basic laws of learning (e.g., such relatively early Age of Theory developments as the rational learning analyses of Thurstone, Gulliksen and Wolfe, Woodrow, etc.), through attempts to build up descriptions of functional relationships via empirical curve-fitting techniques (the early Hull), to methods representing some combination of such "rational" and "empirical" ingredients (e.g., the later Hull). In retrospect, it seems also fair to say that the absolute volume of effort in these directions was small.

Indeed, one can only conclude that the strength of the Age of Theory autism for quantification led to some blurring as between aim and achievement. A kind of pseudo-mathematical jargon became common to all. This is indicated in the general fondness for the language of "variables" and "functions," the incessant use of terms like "parameter" and "parametric" in purely metaphorical contexts, etc. Moreover, the intervening variable schema proved a ready milieu for facile talk in this idiom of wish. The ideal case of the "explicit and determinate" construct linkages called for by the intervening variable paradigm is, of course, *precise quantitative specification*. When the assumptions of the systematist were not put forward in apparent quantitative form, there appeared almost always an aside representing them as transitional, first-approximation statements which would give way to precise mathematical specification with the inexorable advance of the given theoretical program.

The trends of the present study are in distinct contrast to the state of affairs above described. Relative to the mid-Age of Theory, there is a marked increase in realism concerning the prospects for strong degrees of mathematization, especially in formulations having relatively general systematic objectives. There is more modesty, contextualism, and gradualism in representing accomplishments, acknowledging difficulties, and estimating feasibilities. These attitudes gain all the more force in that they are correlated with the (well known) marked increased in recent years in the range of mathematical strategies tried, and in the volume of mathematico-theoretical effort.

We proceed to a brief sampling of the attitudes towards the status and prospects of mathematization in psychology of two echelons of

systematists: (1) individuals who make no marked use of mathematical procedures at systematic levels, and (2) individuals who do.

1. "Non-mathematical" Systematists

The attitude of the authors in this group is well represented by Tolman's simple statement: "I am very relaxed about this" (Vol. 2, p. 97). Moreover, there would be general agreement when he says:

Psychology today seems to me to be carried away (because, perhaps, of feelings of "insecurity") into a flight into too much statistics and too great a mathematization. . . . But to me, the journals seem to be full of oversophisticated mathematical treatments of data which are in themselves of little intrinsic interest and of silly little findings which, by a high-powered statistics, can be proved to contradict the null hypothesis (p. 150).

Indeed there would be wide sympathy with Tolman's further comment when specifically addressing the issue of quantitative specification of function forms:

As I have already indicated above, such attempted precision seems to me for the most part premature. Wherever one can do it, the experimental conditions are so overcontrolled, restricted, and specific that any valid generalizations from such attempts seem to me impossible (p. 150).

Similar sentiments have been expressed in a variety of ways by Hebb and Morgan (Vol. 1); Guthrie, Hinde, Miller, and Skinner (Vol. 2); and Rapaport, Rogers, and Thelen (Vol. 3). And the same position is clearly implicit in the presentations of such men as Asch, Katz and Stotland, Murray, and Newcomb (Vol. 3). These systematists should certainly not be construed as *anti-quantitative*: they are imposing no restrictions on the future but would, with varying emphases, agree with Hebb's statement that "Precise quantification with respect to theoretical entities should be expected only in late stages of development of the science" (p. 636).

Certain points recur with high frequency among members of this group. Thus, the general caution, evident in the Tolman quotation above, to the effect that there is often (and perhaps in principle) an inverse relation between quantitative specificity and empirical generality in psychological statements is often registered. There is also a tendency to rediscover the existence of certain respectable *but* non-quantitative sciences, or phases of given sciences, which had more or less dropped from view during the classical Age of Theory. The classificatory and descriptive branches of biology are often pointed to, as is evolution theory in its pre-mathematical forms. Meteorology comes under notice as a mixed case. A related point often made concerns the interpenetration of *qualitative* and quantitative analysis, even in the most highly quantita-

tive fields. People are beginning to remember with Hebb "how often it was the development of a new [substantive] *idea* that made quantification possible thereafter" (p. 636).

Such attitudes and judgments as have been sampled are not new. Many of the men who hold them now held them ten or even twenty years ago. What is new is the direct, non-apologetic, and sometimes even truculent way in which they are expressed. It is also instructive to note something which could have only become evident in a broadly deployed study like Study I—the wide spread of the present agreements across differences in conceptual predilection and problematic interest.

2. Systematists Working towards "Strong" Degrees of Mathematization

The diversification of mathematical strategies and increased mathematical effort characteristic of recent years is well documented by the present study. The contributions of Blank, Licklider, Graham, Pirenne and Marriott, and Helson (Vol. 1); Estes, Ellson, Frick, and Logan (Vol. 2); Cattell, and Lazarsfeld (Vol. 3), among others, give an illuminating sampling of the range of mathematical imagination that has been shown. These men show precisely that dedication to their methods that one would expect from creative scientists. And certainly, in varying measures, they are optimistic about the potentialities of their methods. *Yet their general estimates as to present achievements, prospects, and limiting possibilities for mathematization in psychology are not much less conservative than those of the systematists discussed above.* Relative to earlier Age of Theory doctrine, there is a remarkable increase in the disposition to define boundaries and point up limits. There is also a realistic concern with many knotty problems concerning the preconditions to significant mathematization, problems many of which were wishfully bypassed in earlier Age of Theory thinking.

A few examples of the current realism and contextualism among mathematically oriented psychologists may be of interest.

Those individuals who represent the position that probably did more in the Age of Theory to foster quantitative optimism than any other *now* estimate achievement and prospect in subdued terms. We have already seen that Miller sharply rejects "the misleading trappings of pseudo-quantification." Though he is "painfully aware of the disadvantages of a qualitative theory limited to ordinal scales and predictions of 'greater than,'" he nevertheless sees "some virtue in the strategy of putting one's theoretical notions through qualitative tests first" (Vol. 2, p. 281). Unlike many earlier workers in the Hullian tradition, Miller is critically sensitized to the indeterminacy of units in the ordinal scales used for most behavior measures, and thus by im-

plication the indeterminacy conferred upon function specifications at mathematical levels which presuppose stronger orders of measurement as a condition to significant empirical application. Though Logan, in representing Spence's position, does not depart so far from tradition as does Miller, the attitude towards quantification is certainly a gradualistic one. What in basic conception is still a Hull-type quantification program is seen as central to Spence's effort, but it is made clear that quantitative analysis is rather conservatively phased in with the development of relevant data, and that the resulting quantitative functions are so formulated (or conceived) as not to overlap their defining base by untoward amounts.

Turning to another quantitative program laid down during the Age of Theory, classic phase, there is certainly no tendency in Cartwright to overstate the significance of Lewin's use of topology (along with such modifications as are represented in his "hodology")—neither in a technical mathematical sense nor a psychological one. This would be clear if only from the candor with which Cartwright exposes the difficulties in conceptualizing Lewin's basic notion of the "life space" (Vol. 2, pp. 65–72). Moreover, in reporting the recent work of the Michigan group designed to represent relations formerly described by Lewinian topology in terms of *linear graphs*, a measured tone is maintained wholly uncharacteristic of the earlier Age of Theory. While on this topic it is well to note that whatever the ultimate fate of Lewin's mathematical notions, he had an early priority among *general* psychological theorists in choosing areas of mathematics for psychological exploitation more on contextual grounds relative to apparent problematic requirements than on grounds of emulating practice in physical science.

Information and (to a lesser extent) servo-mechanism theory have sometimes in recent years been associated with a type of free-roving optimism not dissimilar to that of certain earlier Age of Theory programs. And indeed, each of these formulations has developed its own flexible analogical *patois*. Yet, readers of Frick on information theory and of Ellson on linear frequency theory (Vol. 2) will come away with no impression that these formulations exhaust the universe of mathematical analysis relative to psychological phenomena.

They will find Frick emphasizing that substantive exploitations of information theory have to date been thinly scattered and must by the mathematical restrictions of the formulation be limited to rather narrow modes and levels of analysis. They will discover that many who have "applied" information theory have in the first instance misinterpreted what is essentially a formal mathematical system for a "substantive model" and have thus, "for instance, been able to confound thermodynamics and the statistical structure of language" (Vol. 2, p.

612). They will discover, further, that "the formalism of the theory is directed at a determination of the efficiency of communication, and the application of information theory to psychological data implies an interest in the efficiency, rather than the structure, of the process under study" (p. 613). And they will hear it said more forcefully than is usual that information theory, like *all* probability models, calls for strong simplifying assumptions with respect to the data to which its methods of analysis are applied—assumptions rarely approximated in fact. *Within such limits*, Frick develops cogent, if measured, grounds for the fruitfulness of information analysis as applied, e.g., to certain aspects of sequential behavior and behavior patterning, while Licklider (Vol. 1) gives admirable documentation of the fruitfulness of information analysis in certain aspects of auditory theory.

The case for linear frequency theory is presented with comparable sobriety by Ellson (Vol. 2). Though hopeful for the ultimate prospects, he makes it entirely clear that not in a single study of human behavior has the central condition for the applicability of linear frequency analysis—the criterion of linearity—been met.

The expanding class of stochastic models for the systematization of learning is well represented by Estes (Vol. 2) in the presentation of his theory. Though Estes has a careful program for the extension of his formulation from its base in the description of simple acquisition functions for lever pressing, he is equally careful to circumscribe the senses in which he seeks generality and the ranges within which it has been achieved. He is also very explicit in defining the simplifying assumptions of his model and the tight restrictions in problem formulation, independent and dependent variable characteristics, experimental design, etc., necessary even for their approximate satisfaction.

A most interesting departure from earlier Age of Theory mathematical strategy may be found in the work of Lazarsfeld (Vol. 3). Earlier Age of Theory thinking had seen the problems of *scaling* and (quantitative) theoretical construct formation as relatively independent problems. Scaling methods developed in such contexts as psychophysics, test theory, and attitude measurement, while efforts towards quantitative construct inference proceeded in the hands of learning and "behavior" theorists. Against this background, it is of interest to see the *scaling* theorist, Lazarsfeld, making ingenious efforts towards the extension of a mathematically sophisticated scaling method to problems of theoretical concept formation. The contrast with learning theory is instructive in that Lazarsfeld joins the problem of social science theory at a far more "primitive" level—that of *initial concept formation*. Unlike typical Age of Theory practice, the question is not one of establish-

ing mathematical construct *linkages*, but merely one of establishing constructs which *could* be linked (or prove "linkable") in future theoretical analysis. Though there is a certain sweep in Lazarsfeld's programmatic extension of latent structure analysis to the general problems of concept formation in all social science, there is also, as everywhere among the present group of authors, no tendency to suppress difficulties or overstate achievements. Basically, his concern is to open up a line of methodological speculation which has been bypassed in the rush towards "high order" theory. The procedures are meant to "clarify how we create 'underlying' concepts like traits, attitudes, group characteristics, etc." whose "role is to summarize a variety of empirical observations and to store them, one might say, for systematic use in a 'theory' which we hope will one day develop" (Vol. 3, p. 485); moreover, it is "difficult to predict whether LSA will be useful if applied to conceptually more complex intervening variables as they appear, e.g., in learning theory" (p. 537).

Not inconsonant with the gradualistic overtone of Lazarsfeld's analysis are certain points in Cattell's discussion of factor analysis (Vol. 3). That Cattell makes strong claims for the utility of factor analytic methods is more than slightly evident, but there is certainly no suggestion that these methods preempt other mathematical approaches. The range of application is clearly restricted to the initial isolation and identification of variables and there are definite implications to the effect that factorial methods are not relevant to theoretical analysis in any ultimate sense.

Finally, as Conrad Mueller notes in his supplement on sensory theory, the *sensory* psychologists in this study, while reviewing their own work and that of others, illustrate the use of a very wide range of mathematical methods in specific theoretical contexts. Indeed, the diversity and richness of mathematico-theoretical strategies in this area are greater than in any field of psychology. Whether such methods are extensible to areas in which independent and dependent variables are epistemically more complex is, of course, uncertain. And whether comparable success can be expected from *other* mathematical methods in such areas is also uncertain. Here as before, the most useful export from the sensory area could be a rather general *lesson*—that of contextualism. It is interesting to contemplate that during the classic Age of Theory when hopes were large for the discovery of some canonical quantitative method adequate to all problems of psychology, the repository of mathematical experience accumulated by sensory psychology was rarely consulted. Instead, the methodology of the day consulted the history of other sciences rather than its own. If what we have been calling "contextualism" was *then* too painful a lesson to bear, there are many indications that this is so no longer.

V. FORMALIZATION AND PSYCHOLOGY

Presupposed by and regulating all elements of Age of Theory ideology is, of course, the hypothetico-deductive model of scientific theory. The relevant issues are so widely ramified that in a sense we have been discussing them all along. Yet precisely because of this dense texture of ramifications, the understanding of recent psychological history requires careful *direct* consideration of the role of the hypothetico-deductive model. Here again we must relinquish adequate consideration for scattered generalizations.

It is well to start with a distinction—that between the “hypothetico-deductive *model*” and the “hypothetico-deductive *prescription*.” The hypothetico-deductive *model* is an *epistemological reconstruction* of the theoretical method in science. It is a *technical* reconstruction based upon a long tradition of work in logic, the philosophy of science, and general epistemology. Its codification in the logical positivism of the late 'twenties and early 'thirties was perhaps the clearest response to certain of the relevant questions until that time. Though based only on selected formulations of a relatively advanced character in classical and modern physics, this codification had great technical importance, in that it seemed to mark the beginning of an adequate answer to a central problem plaguing the history of scientific philosophy—that of how to state the relationship between the rational and empirical components of science. On the other hand, the hypothetico-deductive *prescription* would be the recommendation, in any given scientific context, that inquiry be regulated by the immediate aim of hypothetico-deductive systematization *and* that the results of inquiry be set in correspondence with the *explicit* requirements of the hypothetico-deductive (i.e., axiomatic or postulational) model. We should notice at once that the plausibility of the hypothetico-deductive *model* as a reconstruction of practice in given areas of science, or even its desirability as an ultimate goal in various sectors of the scientific enterprise, says nothing about the *general* feasibility, or even fruitfulness, of the hypothetico-deductive *prescription* at all phases of a given science, in all areas of that science, or, indeed, in all sciences.

It is understandable that the molders of the Age of Theory—so ardently in search of the “rule” of theory, of a decision procedure which might contain assurance of forward movement—interpreted the hypothetico-deductive *model* as the *prescription* that explicit axiomatic methods be applied “here and now” in psychological and social science. Indeed, this interpretation, in its more austere versions, held failure to conform to some explicit pattern of formalization as a mark of obscurantism and a confession of conceptual bankruptcy.

As in the (closely related) case of the mathematical ideology of the Age of Theory, it is here necessary to note a similar discrepancy between prescription and practice. To be sure, through the mid-thirties to the mid-forties there were attempts, sometimes laborious ones, within influential theories to approximate the forms of hypothetico-deductive procedure. These ranged from the relatively informal "derivation" in natural language of "theorem" sequences from qualitatively stated postulates to use of the combined resources of symbolic logic and mathematical notation in the axiomatic treatment of limited ranges of data. But again the absolute volume of such effort was small. Instead, the atmosphere became permeated with an "imagery" of hypothetico-deduction—the use or presence of which often seemed interpreted as equivalent to hypothetico-deductive practice. A language of "postulates," "derivations," "primitive terms," "defined terms" (and in more rarified cases, logical variables, constants, arguments, predicates, operators, functors and connectives) became the tongue of psychological commerce. Since the more powerful forms of hypothetico-deductive systematization involve quantitative postulates which may then be manipulated by appropriate mathematical rules of inference, this language was, of course, one with the mathematical language of "functions," "equations," "variables," "constants," "parameters," etc., that we previously sampled. And of course the language of operational definition and that of the intervening variable schema as it developed in the mid-thirties and later also finds its place in the imagery system of hypothetico-deductive method.

The use of this imagery was not *merely* decorative and idle. The acceptance of the hypothetico-deductive *prescription* had important consequences for the prevailing conception of the aims of psychology, the conception of where psychology stood in relation to its aims, and thus the indicated route for further progress. It was, for instance, assumed by many that a backlog of significant empirical knowledge existed adequate to the "construction" of broad-scope, if not comprehensive, theories conforming to the requirements of the hypothetico-deductive model. It was believed that psychology was at a stage such that theoretical differences would inevitably and almost automatically be resolved by the "differential test" of "derivations" from rival "postulate sets." Perhaps of most serious import for the character of actual practice was a cluster of beliefs to the effect that adoption of the forms of the hypothetico-deductive model (or the imagery of its forms) guaranteed that the scientific enterprise would be "self-corrective." Such beliefs led, for instance, to the strange expectation that the initial plausibility of a "postulate" is of little moment in that proper adherence to the forms of hypothetico-deductive method would almost certainly refine its adequacy or lead to its early demise.

Once more we may report that the trends of the study are in definite contrast to the earlier *Age of Theory* position. Here, too, a more gradualistic and contextual attitude is shown in delineating achievement and prospect. Few authors in this study would "scrap" the hypothetico-deductive *model* as the stipulation of a methodological ideal, ultimate approximation of which would be highly attractive. Most, however, would challenge the feasibility of the hypothetico-deductive *prescription* (in the sense of any highly explicit or "strong" axiomatization) as an immediate program for all domains of systematic effort, or indeed for *any* systematic enterprise contemplating reasonably broad empirical reference. This challenge is, of course, delivered with different force and on different grounds by different men. It certainly cannot be said that the imagery of the hypothetico-deductive prescription no longer has effects—especially those more indirect ones which determine problem selection and modes of problem formulation, and color the content and statement of programs for inquiry. Nor can it be said that there is not frequent discomfort, sometimes guilt, over the inability to claim a more advanced status against a hypothetico-deductive "measure." But the hypothetico-deductive prescription has lost much of its force.

In sampling attitudes, we roughly distinguish three positions. If the reader finds that each step on our "scale" is characterized in some complexity, he may be assured that this is as nothing to the quiddities of individual positions which, for obvious historical reasons, are intricately stratified in this area.

1. Belief That the Hypothetico-Deductive Model Represents Scientific Practice in an Incomplete and Possibly Misleading Way; Conviction That the Hypothetico-Deductive Prescription Is Infeasible

This position is perhaps most fully documented by Skinner, whose entire essay may be interpreted as a reaction to hypothetico-deductive prescriptionism. The incidence of his critique is well conveyed by the following generalizations about his own scientific behavior:

The notes, data, and publications which I have examined do not show that I ever behaved in the manner of Man Thinking as described by John Stuart Mill or John Dewey or as in reconstructions of scientific behavior by other philosophers of science. I never faced a Problem which was more than the eternal problem of finding order. I never attacked a problem by constructing a Hypothesis. I never deduced Theorems or submitted them to Experimental Check. So far as I can see, I had no preconceived Model of behavior—certainly not a physiological or mentalistic one, and I believe, not a conceptual one. The "reflex reserve" was an abortive, though operational, concept which was retracted a year or so after publication . . . It lived up to my opinion of theories in general by proving utterly worthless

in suggesting further experiments. Of course, I was working on a basic Assumption—that there was order in behavior if I could only discover it—but such an assumption is not to be confused with the hypotheses of deductive theory (Vol. 2, p. 369).

From a quite different incidence, Guthrie challenges both the feasibility and the sense of the hypothetico-deductive prescription:

The fact that it had taken Russell and Whitehead some 400 pages to establish the conclusion that one plus one equals two, and that every intervening step could be challenged and would require more proof, and that the steps of these added proofs would require still more, has made me impatient with the notion that there can be any completely rigorous deduction, or ultimate validity in an argument. This scepticism colors my notions of the nature of scientific facts and scientific theory (Vol. 2, p. 161).

Other important views of Guthrie concerning this issue are scattered through his essay. Here we might add another of his points—one which cautions against premature formalization in a way echoed by many other contributors:

It will be a very long time before we are prepared to formalize our account. The problem of reinforcement vs. contiguity should be settled before embarking on a system. When the choice is made too early, and the fundamental definitions and categories become official and items that all graduate students must master for the purpose of nationwide examinations, we may find ourselves committed to unproductive efforts (p. 193).

Tolman also declares himself a member of this group in these quite definite words:

All I can say here is that my system is based on hunches and on common-sense knowledge. It is certainly not “hypothetico-deductive.” I have not the type of mind that can remember which were my axioms and which were my deductions. In any event, if a system were a SYSTEM, which I do not believe psychology to be, it would be largely arbitrary which one took as axioms and which one took as derivations. To attempt to build psychology on the analogy of a closed mathematical or logical system seems to me a “bad error” (Vol. 2, p. 150).

Though it is rare that other authors in this group express themselves with the same lack of ambivalence as do those just cited, *many* make points of the same order. Thus, for instance, Rapaport, who generally stresses the need for the systematic tightening of psychoanalysis, in an excellent brief discussion of “The desirable level of formalization” says:

Actually, axiomatization has always been a late product in every science. Centuries of Egyptian geometry preceded Euclid. Newton had not only Galileo and Kepler, but thousands of years of physics behind him.

Sciences do not arise from, but culminate in, axiomatics. Axiomatic systems do not reveal the tracks of a science's development; they conceal them (Vol. 3, p. 135).

Several men in the study who do not directly address the issue of "formal organization" show by the entire trend of their essays and by positions expressed on other matters that they would fall into the present group. Murray, Asch, and Thelen (Vol. 3) clearly would, as would probably Gibson (Vol. 1).

2. Belief That Formalization Is Desirable in Short-range Future, but Strong Awareness of "Dangers" and Difficulties

Cartwright's observations are of particular interest in that the mid-Age of Theory Lewinian tendency was to warn against the "freezing effect" of "premature formalization," but nevertheless to flirt with the possibilities of rendering components of the theory in qualitative-verbal "axiomatic" form. The "warning" now comes through in more resolute terms than formerly:

Contemporary psychological theory is in danger of losing touch with empirical reality. The placing of too great a value upon formal elegance in the construction of theory may well create an insurmountable chasm between the theorist and the psychologist who is interested in the naturally occurring behavior of people. Unless future interest in "model building" is closely guided by an unbiased reference to empirical facts, formal elegance will be purchased at the cost of empirical applicability. . . . While the traditional emphasis of Lewinian psychologists upon the hypothetico-deductive method is still appropriate, one should not forget that formal theory is useful in an empirical science only in so far as it serves as an aid to description.

. . . Too much of current psychological research, I fear, is designed not so much to discover new facts as to confirm some derivation from a limited formal theory. It is in the formulation of research problems that the major advances of psychology take place. Careful observation, recording, and measurement of naturally occurring events and of "experiments of nature" will for a long time to come be the most important *source* of the significant problems of psychology. For this reason, it would seem wise for psychologists to avoid any premature judgment that painstaking observation is inferior or antithetical to rigorous theory (Vol. 2, pp. 80-81).

3. Demonstration of Some Degree of Achieved Axiomatic Explicitness in a Limited Area, plus Measured Optimism over the Prospects for Extension at Comparable Levels of Axiomatization

A highly mixed group of individuals could be said to take this position in one way or another. These would include the sensory psychologists,

those concerned with limited-scope mathematical models (whether primarily substantive or “methodic”), and people like Miller and Logan who, in subdued form, present a picture of theoretical method rather closer to dominant mid-Age of Theory conceptions than most other authors in the study. Heterogeneous as this group is, certain important generalizations can be made. Thus, for instance, none of these men give evidence of an overweening commitment to axiomatic method *as an end in itself*, nor do most of them *go out of their way* to generate optimism about the general feasibility, or even fruitfulness, of formalization in areas outside the limited context in which they work.

Licklider well illustrates the characteristic problem-centeredness of the sensory worker (with respect to this issue as elsewhere) in indicating the basis of his choice of an “analogue level” rather than an axiomatic mode of formulation. Thus he points out:

My own experience in thinking about auditory problems leads me to doubt that a highly formal axiomatic approach would be very helpful to me at the present stage. On the other hand, informal exposition (of the present kind) fills up a great amount of space if the problem is complex . . . As a compromise, it may be convenient to think of the auditory process as a system of operations upon variables in approximately the way that analogue computer experts visualize their computational problems. . . .

Between the axiomatic and the analogue levels, there is, I believe, complete translatability. The axiomatic level is better for examining theories *as theories*. . . . The analogue level is better matched to most people’s ordinary modes of thought and is, therefore, likely to facilitate interactions between theory and experiment. . . .

In expressing a mild preference for the intermediate or analogue level of formulation over the axiomatic, I am suggesting only that auditory theory is in a formative stage and will probably not soon mature. There is more need to line the theories up with the facts than there is to state them in esthetically pleasing form. Even in mathematical logic, it appears, the road to understanding involves processes of thought quite different from those that are reflected in the final efficient, consistent, step-by-step deduction from postulates (Vol. 1, pp. 50–51).

It will, of course, be evident from the presentations of Frick and Ellson (Vol. 2) that as *formal* mathematical systems information theory and linear frequency theory can be axiomatized with considerable rigor. Frick, however, takes the position that at *substantive* levels, information theory primarily (a) specifies a technique for data analysis, and (b) “narrows” the universe within which the systematist may construct empirical models in which information analysis may be used. Thus, there is no necessary carry-over from the axiomatic status of the formal theory to that of any substantive model constructed with its “aid.” Ellson, on

the other hand, represents linear frequency theory as a model which may in principle be given an empirical "interpretation" by "the addition of a few empirical definitions which assert equivalence between (a) empirical operations and (b) terms and logical operations in the model or in statements derived from it" (p. 657). Nevertheless, as Ellson makes abundantly clear, no such interpretation for any sizeable range of behavioral data can be valid at the present time.

Estes takes a position having much of the ring of classic Age of Theory ideology re formalization:

One frequently hears the argument that so long as an empirical science is in a primarily exploratory stage, theories must be informal and qualitative. I do not question that informal and qualitative theorizing is sometimes necessary and even rewarding, but I do have doubts as to both the necessity and the wisdom of being long satisfied with it. The disadvantage of permitting a haze of ambiguity to cover an entire theory is that the theorist, like anyone trying to navigate in a fog, can never really tell how far he has come or whither he is heading. Although we cannot get rid of ambiguity entirely, we can localize it by making our theoretical concepts and assumptions precise and permitting indeterminacy only in the correspondences between theoretical and empirical variables. . . .

It will be clear from our analysis of the present theory that all linkages among constructs are explicit and determinate and all derivations of theorems are accomplished by exact mathematical reasoning. Interpretive rules, on the other hand, are somewhat open ended. . . .

This ring, however, itself somewhat muffled, is further subdued in the following paragraph:

I would like to emphasize that my brief for rigor in theorizing does not imply any great love of formality for its own sake. In the developmental stages of a science it is not healthy for theories to stand still long enough for exhaustive logical analysis. The kind of formalization I consider necessary to sound theory construction consists in progressively sharpening the definitions of concepts and exposing concealed assumptions at the same time that the theory continues to undergo correction and refinement in the light of experimental applications (Vol. 2, pp. 472-473).

A still more nearly classic Age of Theory emphasis is rather generally evident in the essays of Miller and Logan (Vol. 2). Yet Miller, in developing his qualitative systematization of "conflict," is concerned mainly with using this as a constructive device for isolating certain of the oft-bypassed problems of systematic work. And Logan, in representing the Spence position, certainly claims no high degree of axiomatic specificity, nor does he regard it as "practicable at the present time to write a fully formalized behavior theory of any general significance" (p. 329).

A CONCLUDING PERSPECTIVE

It can in summary be said that the results of Study I set up a vast attrition against virtually all elements of the Age of Theory code. If all contributors are not eager to express their intransigence in neon script, neither do they conceal their doubts and questionings. No one is prepared to retreat one jot from the objectives and disciplines of scientific inquiry, but most are inclined to re-examine reigning stereotypes about the *character* of such objectives and disciplines. There is a longing, bred on perception of the limits of recent history and nourished by boredom, for psychology to embrace—by whatever means may prove feasible—problems over which it is possible to feel intellectual passion. The more adventurous ranges of our illimitable subject matter, so effectively repressed or bypassed during recent decades, are no longer proscribed.

For the first time in its history, psychology seems ready—or almost ready—to assess its goals and instrumentalities with primary reference to its own indigenous problems. It seems ready to think contextually, freely, and creatively about its own refractory subject matter, and to work its way free from a dependence on simplistic theories of correct scientific conduct. The day of role playing as a route to reassurance may be drawing to a close. If our science cannot, in terms of attainment, feel secure, it is at least the case that the dance of respectability, as called from the wings by some fashionable theory of proper science, is no longer a dependable source of security.

This preparedness to face the indigenous must be seen as no trivial deflection in the line of history. It is well to bear in mind that the dependence of the Age of Theory on prescription from extrinsic sources is but the most recent chapter in a consistent story of such extrinsic determination of ends and means:

The institutionalization of each new field of science in the early modern period was a *fait accompli* of an emerging substructure in the tissue of scientific knowledge. Sciences won their way to independence, and ultimately institutional status, by achieving enough knowledge to become sciences. But, at the time of its inception, *psychology was unique in the extent to which its institutionalization preceded its content and its methods preceded its problems.* If there are keys to history, this statement is surely a key to the brief history of our science. Never had a group of thinkers been given so sharply specified an invitation to create. Never had inquiring men been so harried by social need, cultural optimism, extrinsic prescription, the advance scheduling of ways and means, the shining success story of the older sciences.

The “scientism” that many see and some decry in recent psychology was thus with it from the start. It was conferred by the timing of its

institutionalization. If psychology had been born a century, three centuries earlier, it would have been less "scientistic." There would have been that much less science, and science-of-science, to emulate. Those who use the term "scientism" dismissively are sensing a problem but decrying the inevitable. Yet, few who fairly look at the brief history of our science could agree that the *balance* between extrinsically defined tradition and creative innovation—prescription and production—has for any sizeable interval been optimal. From the earliest days of the experimental pioneers, man's stipulation that psychology be adequate to *science* outweighed his commitment that it be adequate to man. From the beginning, some pooled image of the *form* of science was dominant: respectability held more glamour than insight, caution than curiosity, feasibility than fidelity or fruitfulness. A curious consequence—even in the early days when such trends were qualified by youth—was the ever-widening estrangement between the scientific makers of human science and the humanistic explorers of the content of man. It is, for instance, significant that a Freud, when he arrived, did not emerge from the laboratories of 19th century experimental psychology; nor was the ensuing tradition of work particularly hospitable to his ideas until rendered desperate by the human vacuum in its own content.

The history of psychology, then, is very much a history of changing views, doctrines, images about *what to emulate in the natural sciences*—especially physics. In the 19th century, this meant the *extension* of experimental method to subjective phenomena; for early behaviorism, it meant the *use* of experimental method exactly as in physics (objectively). By the late 'twenties, there was much objective experimentation but few bodies of clearly stated predictive principles comparable to the crowning achievements of physics: its theories (e.g., Newtonian mechanics, relativity theory). Instead, experimentation sometimes seemed aimless, "theoretical" hypotheses but loosely related to data, and debate idle. We thus get, beginning around 1930, the emulation of natural science *theoretical* method. If the resulting Age of Theory soon tended to subordinate pursuit of the indigenous to the easier consummations of dependency on extrinsic models, this was no new compromise.

It is anyone's guess as to whether we are still *within the Age of Theory*. This epilogue has barely suggested the scope of the attrition developed by the Study I analyses against the "reigning" image of systematic practice; yet there is a tendency *still* to funnel activity through its contours. Much of the attrition, though real, is still *implicit* in practice. There is a new contextualism abroad, a new readiness to consider problem-centered curiosity a sufficient justification of inquiry, but much effort is still invested in apologetically reconciling such impulses with Age

of Theory code. Schedules have been re-defined; systematic claims localized or, if general, made more modest; pre-theoretical knowledge has found a higher priority in the economy; a wider range of subject matters has begun to assert the right to autonomous systematic development; and a wider variety of formulations has been granted "theoretical" citizenship. But the images which govern *positive* systematic action are still, in the main, Age of Theory images. Often when they do not *govern* action, they serve as its rationalization. Despite the fact that action can only be fully free when at peace with its presuppositions, there has been very little direct effort towards the creative emendation of Age of Theory doctrine.

Yet, the stresses against that doctrine not only are severe and far-ranging, they are sufficiently clear and specific to show where creative thought is needed, and even in some cases to point directions. Indeed, one of the ways in which the substance of this epilogue can be read is as a preliminary isolation of those contexts in which our contributors seem most persuasively to call for a rectification of Age of Theory doctrine. Certainly no two students will agree in the diagnosis of such focal contexts of questioning in all particulars. There are in fact a sufficient number of convergences in this study to offer comparable weights of evidence for *many* diagnoses. But it is difficult to believe that there will not be fundamental overlaps.

One area, for instance, in which such an overlap would seem inevitable concerns the need for a theory of definition adequate to the demands of psychology. Each of the five principal trend-areas reviewed in this epilogue, and especially the two reviewed most extensively (i.e., "The intervening variable paradigm" and "The observation base"), converges on this issue. Indeed, it is probable that however one sliced the trends of Study I, they would still converge on this issue. Any re-centering of Age of Theory ideology which would truly liberate psychology for confrontation of the indigenous must give high priority to the many trying and subtle problems essential to a just understanding of empirical definition, and its place in the systematic enterprise. To develop such a suggestion further, however, would take us close to a type of concern which, by our original definition, is not within the province of this epilogue. Here it is meet merely to point up the need for sustained and continuing effort in this direction, and to register the writer's intention to return to this theme in the concluding volume of the series.

Some who may be still inhabited by the autisms of the Age of Theory, if even residually, will no doubt tend to experience the tenor of the findings here reported as depressing. To such persons it should be urged that it is important to distinguish between the actual situation in

psychology and certain of the traditional criteria against which its progress has been measured. The fact that the status may seem dim when measured against such criteria as are contained in the intervening variable paradigm, the demands for operational definition, for strong orders of quantification and axiomatization, or indeed most other requirements of the Age of Theory disenfranchises no *positive accomplishment* within psychology. Though it is possible to say that the directives implicit in Age of Theory doctrine have resulted in a constriction of the range of interest, and perhaps some impoverishment within that range, the important fact is that, as always in science and in problematic action generally, there has been a definite gap between activity and its rationale. That the contributors to this study have so sweepingly challenged Age of Theory values in terms of the actual tendency of their own creative work is itself an indication of the size of that gap.

The fact that the maturity of our science has often been over-represented (relative to some extrinsic standard of scientific maturity) says nothing with regard to the sound advances that have been made. If this progress still mainly involves the search for fruitful variables, rather than the finishing touches to elaborate general theories, it is at least the case that certain of the initial skirmishes have been joined. What emerges from the critique of Age of Theory ideology made by our authors is a far more open and liberated conception of the task of psychology, the role of its investigators and systematists, than we have enjoyed in recent history. There is refreshing recognition of the role of creativity in all aspects of the scientific enterprise and a willingness to confront the fact that creativity cannot be reduced to rule or scheduled.

In general, we are given reason to expect a widespread and profound readjustment of rationale and action in our science. If the limits of methodological, strategic, or programmatic thinking relative to their *constructive* role in the development of science are now seen as tighter ones, such thought is not therefore to be regarded as less important or valuable. On the contrary, this circumstance makes "methodological" thinking, and more generally the second-order analysis of inquiry, all the more important. But if such thinking is to have fruitful consequences, it must be directed at the *realities* of science. It must not inflate these realities into a myth of perfection, or indeed, impose some facile myth of perfectibility upon science. Many concrete and extraordinarily pressing questions are opened up by the specific gaps between conventional methodological rationale and the realities of the systematic endeavors represented in this study. It is in the creative confrontation of these gaps, and in the readjustment of methodological depiction and imaging to action that many important tasks for the immediate future

lie. Such readjustment cannot fail to have salutary consequences for further action. Only from attempts to achieve this more sensitive accommodation of rationale to action can there emerge a type of "methodology"—a type of application of man's critical agency—which could be of use to the practicing scientist.

Such has been the prestige in recent decades of the sources from which psychology has derived its conception of the scientific process that many individuals will perhaps be shocked at the strong implications generated by our study concerning the inadequacies of that conception. In fact, it is more or less inevitable that their sense of shock will be in direct proportion to the clarity and force of those implications. It is no secret, of course, that the primary source from which the Age of Theory borrowed its notion of the scientific enterprise, especially at theoretical levels, was the philosophy of science and, most directly, the logical positivism of the late 'twenties and early 'thirties. In this epilogue a special attempt has been made *to stay within the data of Study I* and thus to determine what might be learned about systematic practice in psychology by studying systematic practice in psychology. Pursuit of the indigenous is best advocated by precisely that pursuit. But at this point it is well to stress that if there be any who are troubled by the discrepancy between the results of this study and philosophical precept, their worries are outdated. The trend of philosophical analysis for more than twenty years, and conspicuously for the past ten, has been towards a liberalization of precisely that view of theory which conditioned the psychological Age of Theory—a liberalization which by now must be adjudged immense. Contributing to this liberalization has been not only logical positivism but such cognate movements as neo-pragmatism and English analytic philosophy. It is of high significance to note that each one of the trends of the present study which may seem so radical when viewed against an Age of Theory measure is entirely consonant with the newer philosophical views. Psychology, then, *still* bases its understanding of vital questions of method on an extrinsic philosophy of science which (in some areas) is twenty years or more out of date.

Consonance does not mean derivability; still less does it mean identity. What psychology needs to know about its goals and stratagems is far too subtly embedded in the tortuous quiddities of inquiring action for the philosopher to be of appreciable help. It would, of course, be as callow to maintain that philosophical analysis is of no relevance at all as it would be to entrust philosophy with total responsibility for the mapping of our future. But the need for testing, culling, transforming, supplementing, adapting philosophical insights within a context utterly controlled by responsiveness to the indigenous is absolute. And, indeed, it is probable that in the long run psychology will have more to contribute

to central problems of epistemology and others traditionally associated with philosophy than vice versa.¹¹

This is only one of many epilogues that could have been written. The five major topics selected for discussion seemed to offer the most direct access to the significance of the study for history. Regrettably, there has been little opportunity to discuss the fundamental substantive contributions that have been made in virtually every essay—the changes and refinements in established positions and the fertile new ideas that have been generated. We have had no opportunity to discuss important contentual convergences: the massive evidence of a tendency for what were formerly discrete and rather insulated viewpoints to come closer, or even merge in significant respects; the tendencies towards joint acknowledgment of problems discriminated only by local groupings of inquirers in the past. All these matters, however, and many others are open to inspection and collation by readers, each of whom will compose the only kind of epilogue that can be truly meaningful.

Perhaps most discomfoting of all is the fact that our thin and selective mobilization of results loses the true quality—the stimulation and often excitement—of the individual essays. The ultimate import of this study is to be found in no “trend,” but in the fact of *men* speaking in languages sufficiently robust to defy fusion.

¹¹ Volume 7 of this series—*Psychology and the Human Agent*—develops concrete grounds for this assertion. Also in that volume certain of the newer developments in the philosophy of science are considered, and an attempt is made to specify the senses in which they are coherent with (yet far from equivalent with) the trends of the present study. Many readers, of course, will already be apprized of the *directions* (and they are not few in number) taken by the newer philosophy of science in its liberalized reconstructions of the nature of theory, empirical and other modes of definition, etc. Volumes I and II of the *Minnesota Studies in the Philosophy of Science* (H. Feigl and M. Scriven (eds.), University of Minnesota Press, Minneapolis, 1956 and 1958) register these developments admirably and within a context addressed to questions of psychological methodology. Perhaps the most dramatic index to the *extent* of the philosophical liberalization is Carnap's article, “The Methodological Character of Theoretical Concepts,” in Volume I, which repudiates almost totally his earlier (1936–1937) analysis of empirical, and thus operational, definition in terms of the “reduction sentence”—an analysis which has dominated psychology ever since importation in the early 'forties.

SIGNIFICANCE OF
SENSORY PSYCHOLOGY
FOR CERTAIN
METHODOLOGICAL
PROBLEMS

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INTRODUCTION

Some difficulties in the statement of methodological problems. Much conventional methodological discussion in psychology seems to present gross dissections to the experimenter; not gross in a logical sense, but gross in terms of providing categories into which the working material of the science fits. This feeling probably arises because many of the analyses of concepts, definitions, theories, etc., have been based on relatively limited samples of the material available for examination, and those that are selected are far from a random sample. This restrictiveness has shown itself, first, in a tendency to pick material that permits a high degree of compactness and formal simplicity. While such analyses are instructive when they are first presented, it must be remembered that the conceptual and theoretical material that forms a simple picture may not be the material that contains the interesting trouble spots, and if methodological discussion is to aid in the assault upon scientific questions rather than serve the functions of a "mopping-up" operation, it will have to continue to represent the diversities and subtleties of the working

material of the science. In this sense, concepts such as force, mass, electrical resistance, the electron, etc., have clearly been overworked. At this stage of the science it would be much more instructive to encounter a discussion that could do justice to the plethora of particles in modern atomic theory of the last decade or two. It might be more helpful to discuss concepts in psychology, not just with the methodological tools developed in the context of the classical physics, but with tools able to handle and illuminate the subtleties of the introduction of the concept of the meson and the ensuing period of discovery of four or five kinds of mesons or the invention of a particle as elusive as the neutrino, which is introduced to save the conservation assumptions and is given a mass of near zero and a cross section that would permit it to go through our sun without being detected.

This restrictiveness shows itself in a second way in the stereotypy or rigidity in the interpretation of what these sample analyses show. For example, scientists have frequently made excellent *attempts* at developing a language for talking about what they do and then acted as if it were the *final* language for such a discourse. Perhaps Bergmann [1] has described a more general trait when he characterizes psychology's response to operationism by saying, "The root of the trouble was that some psychologists in their enthusiasm mistook the operationist footnote for the whole philosophy of science, if not for the whole of philosophy." This tendency toward stereotypy is also exhibited in the targets for the present criticism. In this sense, some of the psychoanalytic concepts and concepts such as resistance at the synapse have been criticized, if not too harshly, certainly too frequently. This has created an atmosphere that, to take the example of synaptic resistance, makes it unlikely that a kind of literature will be read that has, in the last decade, provided some evidence that, in fact, there are some relatively long-term changes in the passage through synapses that result from use. In a variety of ways psychologists have gone along with, and contributed to, this stereotypy with respect to methodological issues. It is against this background that a discussion of some of the general characteristics of the sensory area may be useful.

Some contributions of sensory psychology to methodological problems.

In many ways the sensory area is in a special position with regard to many methodological questions in psychology. Exactly why this is the case is not easy to determine. It may be due partly to the fact that the modern history of this area covers a longer period than is true of most other areas of psychology. It may also be related in part to the fact that this history has been more intimately tied to the history of the physical and biological sciences than most other areas. Whatever the contributing factors, there seems to exist a greater diversity of empirical and theo-

retical procedures than is typical of most areas of psychology. The more closely one examines the sensory area, the more convinced one becomes that there is little justification for being dogmatic with respect to many of the issues that form the core of methodological discussions in psychology. For example, one can find ample evidence to argue for an intimate link between physiological and behavioral data; there are many instances in which the physiological data suggested specific experiments and where physiological theories made specific predictions about behavioral data. But one can also find examples of comparable success in organizing data in the absence of adequate physiological data and in the absence of such theorizing. There are many examples of useful concepts derived directly from physical or physiological data or theory, and there are many concepts that have no such linkage. The same might be said for other topics, such as operational definitions, etc.

I should like to select four points from the outline that was offered as a common starting point for the discussions in this study and let these serve as a focus for considering some general characteristics of the sensory area. These points deal with the questions of definition, quantification, and the types and sources of concepts. The topics will be discussed with the aim of illustrating the diversity of conceptual material that sensory psychology offers for methodological discussion in psychology, in this way providing a broader informational base upon which methodological principles in psychology may be formulated.

SOME CHARACTERISTICS OF SENSORY PSYCHOLOGY

The variety of definitions of stimulus variables. There are many ways of suggesting the full measure of this diversity of conceptual material in sensory psychology. An examination of the terms used in the essays (especially those in Vol. 1) which consider sensory or perceptual matters would reveal, for example, that stimulus terms can range in specificity from the highly specific terms encountered when a physical dimension has been adopted directly, such as distance or energy, to terms representing broad classes of complex stimuli, such as those referred to by the terms graininess, timbre, etc. They can also vary in their complexity, i.e., the length of the chain of definitions linking them to experimental procedure, and in many other ways. What is presented below is a small sample of some obvious differences that may begin to suggest the scope of the problem.

1. Perhaps the greatest diversity in the description of the stimulus relates to the extent to which the experimenter adopts the language of physics. At one extreme the stimulus language does not go beyond the common extrascientific language. This language is essentially one of ob-

jects—chairs, walls, light bulbs, mountains, railroad tracks—which represent the descriptive detail in the protocol. Thus, the definition reduces rather directly to a denotative operation. At the other extreme is the language of physical dimensions, such as changes in energy, wavelength, exposure time, etc. The former has led to an emphasis on such experiments as size constancy and the illusions, the latter to a study of the specific sensitivities, visibility and audibility curves, frequency (audition) and wavelength (vision) discrimination, etc. Most of the subject matter in what is loosely called sensory psychology and some of what is called perception utilize some detailed physical analysis, and again the amount of analysis is a source of diversity.

2. The stimulus specifications using some physical analysis can differ with respect to the complexity of their reduction to primitive physical terms. These terms may involve, as mentioned above, a direct adoption of primitive physical dimensions, such as distance or time, or they may involve extensions of such dimensions. As these extensions proceed beyond the primitive physical terms, they usually involve more and more of a commitment to theory, either physical, physiological, or behavioral or some combination thereof. As an example of the complexity and refinements of some of these terms, consider examples from audition and vision.

As a first example, consider the concept of frequency as it appears in physics and in the psychophysics of audition. At the early stages of measuring something like the audibility curve, a stimulus definition in terms of elementary physics, e.g., with an understanding of the steady-state output of an oscillator and its amplitude and frequency, would get us through most situations. Many of the subtleties of terms like oscillator and frequency can be ignored. But there are many occasions in audition where the descriptive language becomes more elaborate, not merely because the physical theory is available for use, but because the behavioral data suggest that a refinement in stimulus definition or some comparable adjustment is needed. For example, beginning with an elementary level of description, it would seem operationally straightforward to study the frequency sensitivity of the ear for various stimulus durations. Consider an experiment, for example, that proposes to measure thresholds at various frequencies ranging from 100 to 1,000 cps and stimulus durations from 1 msec to 1 sec. Operationally, what one would probably have in mind is an experiment involving the output of a sine-wave generator and a mechanism for switching in the sine-wave stimuli for various lengths of time. With this line of thought, what one would mean by the experimental proposal stated above is that the stimulus would have a waveform with a value of zero up to time t_0 ; up to t_1 the stimulus would be a segment of a sinusoidal function whose period was

specified; after t_1 the waveform would again return to zero. Even intuitively this problem would seem to get troublesome when one uses exposure times of the order of 1 msec for frequencies, say, below 500 cps, for here one is presenting a subject with a waveform that is a fraction of 1 cycle. Such a curve seems to lack the obvious properties associated with the term frequency. More important than one's intuitive uneasiness is the fact that the discrimination data that he obtains from this experiment emphasize that there is a problem for the psychologist; something seems to happen to those psychophysical functions involving low frequencies and short exposure times [2]. Unfortunately, intuition does not carry one very far in solving the problem. At this point, mathematical and physical theory may be brought to bear in furnishing some supplementary language for analyzing the stimulus. This is done, first, by making the notion of frequency very specific and then, with no loss of rigor, converting the concept of frequency into one of great generality. The specificity is achieved by passing beyond intuitive notions of repetitiveness and defining frequency in the following way: any function for which some nonzero value T can be found, so that $f(t) = f(t + T)$ is true for all values of t , is called periodic, and T is called the period. Frequency is then defined as $1/T$. The generality is achieved by the important development of Fourier, who showed that any waveform (with a few restrictions of little relevance to any psychophysical discussion) can be represented as a set of sinusoidal waves and by showing what the frequency spectrum of a function $f(t)$ is.

The development may increase the complexity of one's description of auditory stimuli, but it provides a language of great generality. Regardless of how the development is conceived, two considerations must be of some concern. First, one must employ some device for talking about a variety of waveforms and waveforms of all durations. Second, the experimental data require some treatment of the differences among frequencies in their dependence on duration. The question of the usefulness of this particular refinement in stimulus definition depends on the extent to which the auditory system is conceived as a frequency-rendering system. Perhaps some other type of refinement is required. Licklider, in Vol. 1 of this series, has presented an interesting and detailed discussion of the general problem and certain kinds of additional analyses of the temporal aspects of the auditory system. Such analyses provide the machinery by which a treatment of auditory discrimination proceeds. No claim need be made that these analyses add more information about the stimulus than is contained in a denotative description of the stimulus waveform; whether this is true depends on the definition of information. But these analyses do provide specific ways of expressing such information in terms that will be relevant to subsequent theory.

As a second example, consider a similar type of situation encountered in vision in the use of small linear extents. Once again, it would seem operationally meaningful to investigate the absolute threshold for long thin lines as a function of, say, the width of the line. For wide lines, an experimental operation such as reducing the width of line can be interpreted as decreasing the extent of the image formed by an optical system, such as the eye. But as the width of the line approaches the dimensions of the order of 1 min of arc such an operation (decreasing width) ceases to affect the image size to any appreciable extent, but rather changes primarily the intensity over a fixed linear extent. This phenomenon, a problem in diffraction, is common to all optical systems, and its analysis is directly available in the physicist's treatment of the nature of light. But once again, the analysis is not applied in the psychophysical situation just because it is available in physical theory, but because there are psychophysical data that pose certain problems, for example, the substitutability of area and intensity at threshold, the general indiscriminability of area and intensity changes for very small areas, and a variety of problems in visual acuity.

3. The analytical steps in defining the stimulus can also differ in terms of the extent to which the organism is involved in the definition. Some terms, regardless of their complexity, can be defined without reference to the detecting organism. Such would be the case, for example, with the term frequency discussed above and such terms as wavelength composition in vision. The *raison d'être* for a particular analysis may involve notions about the organism's behavior or about how some component of the organism works, and thus the *evolution* of the definition may involve the organism in an important way; but the specification can proceed without such a reference. Again the concept of frequency illustrates this point. One may be interested in a frequency specification of the stimulus because of a view of how the ear or the auditory system analyzes complex input waves or because of what is known about certain kinds of auditory discrimination. Nevertheless, one can specify the frequency components of a given waveform without referring to the ear or the auditory system or its function. A similar situation exists in vision in the use of angular dimensions. If one measures the absolute threshold for circular stimuli of varying sizes placed in a dark field, he can obtain different measurements for each distance the stimuli are placed from the observer. If, however, one takes some function of the distance and the size of the stimuli, which turns out to be the angle subtended by the stimuli, these many functions are unified. Viewed historically, the gradual emergence of this specification was due to a complex interplay of the data on visual discrimination, the general development of optical theory, and the physical analysis of how the eye works. This does not alter the

fact that the specification of a term such as visual angle involves no more of a commitment concerning the organism than where the eye will reside in space.

Although many of the definitions of terms encountered in the sensory area can be written without reference to the detecting organism, there are a large number of remaining terms whose definition requires some information about how the organism works. This may be seen, first, in some of the terms that represent what might be called the more proximal stimulus variables. This increasing involvement shows itself if one turns his attention from the term visual angle to a term such as the retinal image, for here it becomes obvious that one is shifting to an analysis based on notions from physical optics and applying it to the eye as a physical system. This requires definitive experimental information concerning the physical properties of the eye, for example, its focal length. The retinal image cannot be computed without a specific number referring to this quantity.

This reference to the organism may involve either behavioral data or physiological data or both, and in the course of the history of the term it may shift from one to the other. The notion of critical bands in the frequency tuning of the auditory system is of this sort. One may evaluate this tuning on the basis of psychophysical data, such as masking data or data on frequency discrimination, or he may, after the ingenious experiments of von Békésy [6] on the mechanical properties of the inner ear, directly evaluate this tuning on the basis of physical measurements of basilar membrane vibrations.

The involvement of the organism in such dimensional definitions can become very complex. One of the most complex examples in the sensory area is the specification of color stimuli in vision; the reader is referred to Graham's article (Vol. 1) for this intricate example. Another, and perhaps more familiar, example in vision is the class of dimensions called the photometric dimensions. These dimensions are described by Judd [5] in the following way:

If it is desired to convert the radiant flux (watts) entering the pupil of the eye to luminous flux (lumens), the additivity law is applied. The radiant flux is analyzed spectrally so that for each portion $\Delta\lambda$ of the spectrum the spectral radiant flux P_λ is known. Then, by multiplying the spectral radiant flux by the absolute luminosity K_λ (lumens per watt) for that wavelength region, we find the spectral distribution of luminous flux. But, by the additivity law, the total luminous flux F is equal to the sum of the parts making up this spectral distribution, thus:

$$F = \sum_{\lambda} P_\lambda K_\lambda \Delta\lambda$$

where $\Delta\lambda$ is a wavelength interval so small that further reduction fails to alter the sum significantly.

With respect to this so-called additivity law and the general problem of the psychophysical discriminations involved in this type of dimension Judd also says:

The conversion of a stimulus specification from radiant to luminous terms is based upon the additivity "law" of luminance: if a stimulus of luminance B_1 (such as is produced by a spot of light on a screen) is added to a second stimulus of luminance B_2 (such as is produced by a second spotlight shining on the same screen), the luminance B of the combination stimulus is defined as the sum of the luminances of the component stimuli; that is, $B = B_1 + B_2$. This law has frequently been studied because it is the basis of photometry (Dresler, 1937; Kohlrausch, 1935; Urbanek and Ferencz, 1942). It has been found to fail unless the eye is kept throughout the series of comparisons essentially in a fixed state of adaptation. That is to say, the law fails unless there is pure cone vision, pure rod vision, or some constant combination of the two. Some reports indicate that it fails anyhow.

If a spot of red light is adjusted to the same brightness as a spot of yellow light, and a spot of green light is similarly adjusted to match the brightness of a second yellow light, the red and green lights added together are often found to be darker than the sum of the two yellow lights (Dresler, 1937). Since it is impossible for an observer to report with high precision and reproducibility which of two spots of light of widely different chromatic character is the brighter, these failures of the law have not been taken very seriously. The usual explanation is that the observer mistook the high saturation of the red field, relative to that of the yellow, for brightness and so obtained a spuriously high estimate of its luminance in the first place. The next time the observer tests the additivity law his observations are somewhat conditioned by the first experience, and soon he has learned to make photometric settings in accordance with the law. Thus the additivity law provides a basis for a convenient photometric technique that correlates excellently, though not perfectly, with visual experience.

Thus, the status of this kind of variable involves the interpretation of mistakes the subject may make (confusion of saturation and brightness) and the gradual acquisition of the behavior that is in accordance with the "law" utilized in the definition.

In a sense, a photometric dimension is a "physical" dimension, in the trivial sense that probably all dimensions in a behavioral science are "physical." It is an energy function in wavelength coupled with a "weighting" function. The weighting function is a convention—it is the "agreed-upon" variation of sensitivity with wavelength for the "average" observer. Thus, it is observer-determined, although it is not a correction on an individual basis. These photometric dimensions have many ad-

vantages in that they unify many disparate functions when wavelength is a parameter. They also have some disadvantages, particularly when their origin and definition is forgotten. For example, if one plots a series of dark-adaptation curves for various wavelengths and uses a typical photometric unit, he observes that the curve showing the greatest drop, and the lowest terminal threshold, is one representing the blue end of the spectrum. In the past, this has led to the statement that the dark-adapted eye is the most sensitive in the blue end of the spectrum. Since sensitivity is quite generally defined as the reciprocal of the energy required at threshold, this statement is clearly incorrect, as any of the determinations of the dim-visibility curve will show. What happens in such a treatment of the data is that the photometric units used involve a correction based on the visibility curve for the light-adapted eye. Unfortunately, the dark-adapted eye exhibits a different visibility function, and what such a graph actually shows is a combination of what might be called the basic change in sensitivity, defined as indicated above, and the change from one visibility function to another. The differences among the different wavelengths reflect the magnitude of this difference in the "weighting" term at different stages of adaptation.

The variety of definitions of response variables. The diversity of description of the response terms in experiments in sensory psychology is more difficult to evaluate because less is known about what the problems are and how to treat the subject matter. There are differences, first of all, in the manner in which response-class membership is determined. Some experiments involve an apparatus component for defining the topography of the response being measured. For example, the subject may be asked to press one of n keys or turn one of n knobs. In other experiments the experimenter performs this function. For example, the subject may be asked to give one of n verbal responses, say, yes-no, large-small, very heavy-heavy-medium-light-very light, or he is asked to assign a number from one to ten or one to one hundred to the stimulus. Here the experimenter plays the key role identifying instances of the class of responses used. From the point of view of a detailed analysis of the sensory experiment much needs to be done to clarify the relation of these two procedures to each other and to the related studies in lower animals.

Even more complex in terms of any theoretical analysis are those experiments in which the experimenter reports what he sees or hears when he performs some operations. While it is true, as pointed out by Licklider (Vol. 1), that this kind of observation will continue to act as an important guide for the experimenter's behavior, there can be little doubt that such a procedure offers difficulties for any attempt to outline a rigorous behavioral account of this experimental area.

The range of application of quantitative techniques. The sensory area is also diversified with respect to the quantitative techniques used, and this diversification is one of degree and kind. The degree of quantification can range from its complete absence to some of the highly formalized techniques illustrated in Vol. 1. Some examples are to be found in the articles by Blank, Graham, Licklider, and Pirenne and Marriott. As examples of this diversity one encounters elements of matrices and determinants in the development of the color equations for specifying color stimuli; one sees the notion of the line integral in certain theories of color vision; there are many uses of ordinary differential equations in the many versions of photochemical theory in vision as exploited by Hecht [4] and extended by many others. One sees the utilization of many aspects of probability theory, e.g., the theory of random time processes, as discussed extensively by Pirenne and Marriott, and the use of certain specialized probability notions, such as information theory and decision theory, in the theories of the ideal observer and certain formulations in perception. One encounters the use of the Fourier integral in many problems in audition, e.g., frequency analysis as discussed above and in the article by Licklider; one sees also some of the general features of measure theory operating in establishing geometrical properties of a hypothetical visual space so clearly illustrated in the article by Blank.

These quantitative procedures serve a variety of functions. They may represent refinements in the descriptive language of the stimulus, they may provide a specialized technique for discussing stimulus-response correlations, or they may offer a means of solving for functional relations between theoretical variables.

Variations in the nature and source of concepts. Finally, it is probably with respect to the nature and source of concepts that sensory psychology can provide its most unique emphasis in a methodological discussion. While there are many concepts that are primarily behaviorally derived and that perform functions similar to those discussed under the heading of intervening variables in the MacCorquodale and Meehl sense, there are numerous specific concepts, with many quantitative empirical and theoretical properties, that owe their existence and properties to data and experiment in another discipline. There are many examples of such concepts in the fields of audition and vision. For example, a theory by Helmholtz suggested that the ear acts as a series of tuned resonators. The phrasing of the theoretical statements and the context in which they were introduced carried the implication that the basilar membrane would possess certain physical properties that would permit this resonance, e.g., that the basilar membrane would exist under greater lateral than longitudinal tension. Following the formulation of Helmholtz there were many different conceptions of how the ear worked. In

addition to the helmholtzian resonance model, there was a class of theories that conceived of the cochlear canals as tubes filled with fluid with the basilar membrane serving as an elastic partition. Many different theories resulted within this framework, depending on the physical properties assumed for different parts of the inner ear. If one assumes a certain damping, he can generate a series of standing waves, i.e., a different standing-wave pattern for each frequency. With other assumptions, traveling wave patterns can be generated that have maxima at different positions for different frequencies. If the elastic partition is considered to be relatively stiff, the whole length will vibrate approximately in phase, and one has a vibration mechanism something like a microphone. All these assumptions can lead to statements about the discriminability of pitch and other psychophysical data, but it is obvious that they can survive simultaneously only in a factual vacuum regarding the physical properties of the ear. With the advent of the now classic experiments of von Békésy on the mechanical properties of the inner ear, this freedom of conceptualization is restricted. The physical consequences of a theory such as that of Helmholtz are not observed; von Békésy's results suggest that the basilar membrane is not under significant tension. The consequences of a telephone or microphone conception of the basilar vibration are also not born out; the vibration of all parts of the membrane are not in phase. Thus, the ways of generating predictions or "explanations" of the psychophysical data that have a common starting point in viewing the ear as a physical and physiological system can be separated on the basis of nonpsychophysical experiments such as those of von Békésy. At the same time these data can provide the basis for further extensions of the theory or theories that survive the fact gathering. Licklider's article clearly indicates this interplay of the behavioral and physiological data in the molding of auditory theory and experiment.

The notions of photochemistry behind many theories in vision also emphasize this complex interplay between psychophysical data and theory on the one hand and physiological data and theory on the other. The concept of a photochemical action in vision arose early in the modern history of photochemistry itself. The visual aspects of this problem date back to the discovery of visual purple by Boll in 1876, although it was really Kuehne (1879) who gave the details of visual-purple extraction and studied many of its physical properties. It became clear very early that this material in the retina was bleached in the presence of light and regenerated in the dark. This seemed to offer an interesting link with what was known about the changes in sensitivity of the human eye in light and dark adaptation—light, in bleaching visual purple, left less photosensitive material available to absorb light. Thus, to get the

same photochemical effect before and after light adaptation, it was necessary to present more light after light adaptation. A second line of evidence was also of great significance. Koenig, as early as 1894, first clearly showed that a quantitative agreement existed between the visibility data (i.e., the sensitivity of the eye to different wavelengths) and the absorption curve of visual purple. A decade later Trendelenberg showed that the rate of bleaching of visual purple also agreed with the visibility curve; that is, regions of the spectrum easily seen are regions that bleach visual purple rapidly. This kind of correspondence led to a number of quantitative formulations of the action of the visual system based on the kinetics of a regenerative photochemical system.

The important point is not that such correspondences (and there are many others), as suggested in the preceding paragraph, exist but rather that they are part of a joint, dependent development of two sections of science, behavioral studies of what one sees and the physiological and biochemical studies of the visual system. The behavioral data influenced physiological research and the physiological data influenced the behavioral research. For example, one form of this photochemical conceptualization as formulated by Hecht in the early 1920s conceived of a photochemical-chemical cycle wherein light bleached a photosensitive substance, and this bleached material, in the presence of other materials, reformed the photosensitive material via a chemical (nonphotic) reaction. Since the regenerative link in this cycle was a purely chemical one, it was expected to exhibit certain properties characteristic of chemical reactions, and this suggested a number of specific directions in which research might proceed. Since the velocity of reactions of the type envisioned are typically temperature dependent and since the regenerative link was viewed as critical in the increasing sensitivity as a function of time in the dark (the psychophysical problem of dark adaptation), Hecht [3] predicted that dark-adaptation curves would be more rapidly changing functions at high temperatures than at low temperatures. This problem was studied behaviorally in cold-blooded animals; the outcome of the experiments was that the dark-adaptation curves were temperature dependent in the predicted way.

A second, and more striking, example in vision centers on a large number of human behavioral experiments dealing with the so-called Wald and Clark effect. By the middle 1930s the biochemical research had shown that the visual-purple cycle was more complicated than originally conceived and that it possessed at least three important stages: visual purple, retinene, and vitamin A (and perhaps a number of additional transitional stages). The data clearly indicated that the velocities of the changes from one stage to another were not identical and that it was possible to vary the procedures for presenting the incident light in

such a way as to yield different combinations of concentrations of the various substances involved. For example, the data indicated that it should be possible to find two adaptation procedures that would produce the same resultant concentration of visual purple but yield different combinations of concentrations of retinene and vitamin A. The data on the velocity constants for the component reactions clearly suggested that the rates of regeneration of visual purple in these two cases would be different; the rate of return to visual purple from vitamin A was different from the rate of return from retinene. Coupled with the general assumption of the photochemical theory of that period that sensitivity was a function of the concentration of the photosensitive material, this biochemical research led to an important set of expectations with respect to dark-adaptation curves. The general expectation was that adaptation curves that started at the same point could be made to follow a variety of courses to terminal threshold, depending on the prior conditions of adaptation. The Wald and Clark experiment and a large number of experiments that followed have confirmed and elaborated this point [7].

Needless to say, the preceding paragraphs should not be interpreted as saying that concepts arrived at in this manner, such as the concepts of a photochemical theory, etc., are *the correct* concepts; they may be right or wrong in the same sense and in the same ways as concepts derived from other behavioral data. Rather, two points may be emphasized. The first is that data and theory of a physiological sort can and do guide and generally interact with behavioral experiments and programs of research in the sensory area in the same way that one set of behavioral data or concepts will interact with another. No new questions about "levels of discourse" need enter into this interaction that are not encountered in many other contexts in psychology where one moves from one "level" to another, for example, the interpretation of the complex topography of many human problem-solving situations in terms of a simpler and more easily specifiable topography of an animal acquisition experiment. The second point is a return to the emphasis on diversity, for there are many examples where this interaction with physiological data has been a minor factor. For example, most of the developments that have taken place in color vision have been little supported by specific physiological data.

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Topics followed by an asterisk are those treated by all or most authors (often extensively). These pertain mainly to the "crosscutting" systematic and methodic issues raised by the themes of analysis and related editorial proposals. In most instances, page references for such topics are given *only* to basic definitions or explanations. Individual author treatments of many asterisked topics can be located by reference to the tables of contents appearing with each article, in conjunction with the use of discussion topic index numbers (see Note on the Use of Discussion Topic Index Numbers, pp. 724-725).

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