THE STIRLING COUNTY STUDY

A Research Program in Social Factors Related to Psychiatric Health

Eighth Annual Report

June 30, 1958

Alexander H. Leighton, M.D.
Director

Like all previous annual reports, this report, especially its substantive articles, is confidential. The research position of the Stirling County Study would be seriously undermined by a premature disclosure of material which, in its present form, is not ready for publication.

This report was prepared by Mrs. Ruth O. Kent.

TABLE OF CONTENTS

PREFACE

As in the two previous years, the chief purpose of the Eighth Annual Report is to indicate progress toward publication of the volumes which report the results of the Study.

Part I is a brief statement of the areas in which work has advanced in completing for presentation to the publisher the three volumes reporting on the goals, the methods, and the findings of the research. It should be noted in this connection that, to assist in the final analysis and preparation of these manuscripts, an additional grant was made by the Milbank Memorial Fund during the year.

Following the usual model of these reports, Part II includes articles which have been selected from various aspects of the project as illustrations of the type of work being undertaken.

Part III contains brief descriptions of related research projects which have grown out of the Stirling County Study, most especially the work in planning cross-cultural mental health research.

This report is based on discussions presented in earlier reports, and thus assumes on the part of the reader a familiarity with much of the background of the project and its purposes, as well as the use of pseudonyms to designate place names and individuals mentioned in the substantive reports.

The Eighth Annual Report is the last of the Stirling County Study Reports as such. Accounts of the next year's work of the Stirling County Study will appear as a subsection of the next report, June 30, 1959, which will come out as the First Annual Report of the Cornell Program in Social Psychiatry. This arrangement is based on the incorporation of the Stirling County Study with the Midtown Studies of the Payne Whitney Clinic, New York Hospital, to form the Cornell Program in Social Psychiatry on July 1, 1958.

Staff Changes

In September Dr. Alexander H. Leighton, Director of the Stirling County Study, went to the Center for Advanced Study in the Behavioral Sciences at Stanford, California, where he held a postdoctoral fellowship for the year 1957-58.

Dr. Bruce Dohrenwend, Social Analyst, left the project in January to take a research position at Columbia University, and the main task of statistical analysis was assumed by Mr. David B. Macklin.

Upon completing his doctoral degree, Dr. Norman A. Chance left to join the staff at the University of Oklahoma.

During the summer, Dr. Toshio Yatsushiro, a former Stirling staff member now at McGill University in Montreal, came to Cornell to do special work related to Volume III of the Study.

Miss Regina Buckley held a year's appointment as a Research Assistant, working with Dr. Allister M. Macmillan on the analysis of the Health Opinion Survey.

Mr. Donald A. Kennedy held a research assistantship for the summer in connection with the cross-cultural study (see Part III).

Another addition to the staff, holding the appointment of Research Assistant, has been that of Dr. Edward Llewellyn-Thomas, a physician in one of the towns of Digby County. His research is discussed in Part I.

Mrs. Louise G. Richards held a year's appointment as a Research Assistant and worked with Dr. and Mrs. William D. Longaker on the Clinic-Community Study, which is described in Part III.

Drs. Seymour Parker, Robert N. Rapoport, and Marc-Adélard Tremblay, and Professor Emile Gosselin continued to cooperate on a part-time basis in writing the volume on sociocultural analysis of the Stirling County Study.

Dr. Allan R. Holmberg, Professor of Anthropology, joined the Advisory Group. Advisors who continued to serve the project as consultants were Dr. John S. Harding (Psychology), Dr. Philip J. McCarthy (Statistics), Dr. Edward A. Suchman (Sociology), and Dr. Robin M. Williams, Jr. (Sociology).

An Advisory Board has been formed for the Cornell Program in Social Psychiatry which begins July 1, 1958. Members are Dr. Oskar Diethelm (Chairman), Professor of Psychiatry; Dr. Edward A. Suchman,* Professor of Sociology; Dr. Alexander H. Leighton, Professor of

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* Beginning with the Spring Term, 1958, Dr. Suchman was on a leave of absence from Cornell University. His position on the Board was taken by Dr. Allan R. Holmberg, Professor of Anthropology.

Psychiatry (Social Psychiatry); Dr. Walsh McDermott, Professor of Public Health and Preventive Medicine; Dr. William F. Whyte, Director of the Cornell Social Science Research Center; and Dr. Theodore P. Wright, Vice President for Research of Cornell University. Dr. Charles C. Hughes, Research Associate, is Secretary.

There have been a number of changes in staff at the Western Nova Scotia Clinic. Dr. Herbert N. Davy returned to England in March to take up a research position. The post of Chief Psychiatrist and Executive Director was then filled by Dr. G. A. W. Angus, who had arrived from Western Australia in September to engage in research as Associate Psychiatrist of the Clinic. The Clinical Psychologist and Clinic Administrator, Mr. Bernard Hébert, began a leave of absence in the autumn. Mrs. Elizabeth Kerr-Wilson, Social Worker, resigned, and Mr. Lionel Vallée and Mrs. Renée Vallée joined the staff as Social Workers. The position of Clinic Secretary was assumed by Miss Emilie Melanson upon Mrs. Gladys Brown's resignation.

Acknowledgment is due a number of colleagues and agencies for their assistance during the year. Recognition by name is given in those sections which discuss their participation.

Part I - PROGRESS STATEMENT

1. Theory and the Frame of Reference

The year spent at the Center for Advanced Study in the Behavioral Sciences afforded Dr. Alexander Leighton an opportunity for considerable work on the first volume, the report which outlines the general purpose and theoretical frame of reference for the Stirling County Study as a whole. At the Center Dr. Leighton had the benefit of comments from other Fellows as well as continued consultation with the staff in Ithaca. By the end of June, three-fourths of the volume had been redrafted.

Explorations in Social Psychiatry, edited by Alexander H. Leighton, John Clausen, and Robert N. Wilson, was published in December by Basic Books, Inc. In the summer and early fall, Dr. Leighton prepared the volume for publication, first working on the manuscript with the editor, and then reading the galley proofs. The book, sponsored by the Social Science Research Council's Committee on Research in Psychiatry and the Social Sciences, includes some of the findings of the Stirling County Study in the chapters which were written by members of the Stirling staff.

2. Study of the Sociocultural Environment

A research conference was held in Digby in July, the purpose of which was to discuss the then existing draft of the second volume, Cove and Woodlot. A new plan for the integration of quantitative tables into

the body of the book was formulated. On the basis of this conference a further, more condensed draft was planned and completed in November. This draft subsequently underwent two further revisions during the course of the year, one of which was done by Dr. Charles Hughes following conferences with Dr. Alexander Leighton at the Center in California in January. The revisions benefited from comments of several meetings of the Advisory Group, and in late June the book is in its semifinal draft awaiting the opportunity to integrate it more closely both with Volume I and Volume III.

The introductory section of the volume includes a general picture of the county--historical, economic, social, and cultural patterns--and a discussion of how the "focus areas," communities chosen for special study, were selected. Following this are separate chapters describing the focus areas themselves. The study of each focus area contains a summary of data pertinent to the indicators of sociocultural disorganization, and a résumé of the main sentiment systems which characterize each area. A final chapter has been prepared which is a discussion of the implications of these contrasting sociocultural environments for psychiatric disorder.

Mr. John Collier, Jr., a professional photographer formerly on the staff of the Stirling County Study, has continued work on his book discussing and analyzing the use of photography in social science interviewing, and has completed a second draft of it. Some of the photographic material used in Mr. Collier's book is reproduced in Volume II of the Stirling County Study reports.

In addition to the work that has been directly concerned with completion of Cove and Woodlot, there have been other studies of the sociocultural environment which have contributed useful material. The Ph.D. thesis completed by Dr. Norman Chance is an example of this sort of work. Dr. Chance's research, concerned with analysis of the effects of economic decline over a century in a semi-urban town in the county, was described in last year's annual report. It was presented as a Ph.D. dissertation in September, 1957.

Mr. William Jenkins has continued his economic analysis of farming in the county, which was described in the Sixth Annual Report. While still holding his position in the Extension Service in the Nova Scotia Department of Agriculture, he has continued work on his doctoral thesis to be presented to Harvard University. As previously, he has commented on various sections of drafts of Cove and Woodlot.

The work of Professor Edgar McKay of the University of Maine on an analysis of economic and sociological changes over the last century in Bear River, a former shipping point for lumber, has continued. The field notes which he has taken in connection with his analysis have been incorporated into the general body of anthropological field note materials that the project has maintained. He has also submitted various preliminary drafts of sections of his report.

3. Study of Psychiatric Epidemiology

The year was marked by the completion of the psychiatric evaluation of 1,010 protocols which were prepared from the interviews of the

county sample. The evaluations are a crucial part of the epidemiological report, Volume III, most of which has now been drafted by Dr. Dorothea C. Leighton.

On the basis of the evaluations other analyses were made, such as the Focus Area Analysis, presented as a working paper in Part II. Much special work was done on the focus area evaluations, both in defining the terms used and in trying to discover the differences between the well-organized and disorganized areas and between the French and the English. Part of the work on the focus areas was done with Dr. Edgar Anderson of the St. Louis Botanical Garden, adapting his method of making graphic representations of patterning of specified characteristics in plants to our patterns of psychiatric symptoms.

Another major accomplishment during this period was completion of the recalibration of the Health Opinion Survey (HOS) of the Digby County area. Several analyses were made of the responses of the French and English ethnic groups and on the basis of these it was decided that a single scoring system would be adequate for both groups. This was developed by a discriminant function analysis of the same sort used in the original HOS standardization. The two criterion groups were a sample of 93 hospitalized neurotic patients (including the 78 neurotic patients used in the original HOS standardization) and a random sample of 100 respondents from the Family Life Survey (FLS) of the Stirling County Study. All 20 of the HOS items included in the FLS were used in the final version of the scale with weights determined from this discriminant function analysis.

A cutting point has been set up to separate individuals who are "presumably well" from those who are "potential patients" in terms of their scores on the HOS. Approximately two-thirds of the entire FLS respondents fall into the "presumably well" group, while only 2 of the 93 patients fall into this group.

		Weighted FLS (N=1,010)	Criterion Group Combined, Diagnosed Neurotics
"well"		66%	2%
don't know	"potential patients"	10%	3%
"sick"	"potential patients"	24%	95%

This gives a rough indication of the probable validity of the HOS as a screening instrument for several psychoneurotic conditions. However it overestimates the validity of the scale to some extent because the evidence is derived from the groups on which scoring weights were computed.

Dr. Allister Macmillan made two trips to California to confer with the Drs. Leighton. They discussed the HOS chapter--how to compare the HOS-scored results on protocols with the phychiatrists' evaluated results--as well as the organization of Volume III as a whole. Dr. Macmillan gave further assistance in the work on the volume by reading and offering extensive critical comments on the chapters which were drafted, especially on the HOS.

Analysis of the hospital survey revealed that scores of patients who were hospitalized for non-mental illness (e.g., general organic illness) were no different from scores of the general population, in

this case represented by a sample of non-Negroes from Hants and Kings Counties. Maternity cases tended to score as more healthy than the general population. The medical and obstetrical people under hospital treatment thus answered like "well" people.

The factor analysis of 40 HOS items by Mr. Arnold Simmel was also completed during the year, using data collected in the original administration of the questionnaire outside of Digby County. This analysis showed that the major portion of the item variance can be attributed to a single common factor which presumably represents a tendency toward psychoneurotic illness.

Further work was done on the School Study with the assistance of a graduate student at Stanford. The Personality Index tests of the school children of Digby had previously been scored by the California Test Bureau. The mean scores of the distributions were calculated, the distributions being divided into residence groups, as well as various age and sex groups.

Dr. Toshio Yatsushiro made an extensive review of the literature in several fields which bear on the problems of ecology as they may be related to the study of the person in his sociocultural environment, especially the study of psychiatric disorder seen in an ecological framework.

Adding longitudinal perspective to the epidemiological work is a study being conducted by Dr. Edward Llewellyn-Thomas, a physician in one of the towns of the county which has been the focus of intensive social science research. He has been making a study of the physical

and psychiatric well-being of people in their normal life context. As a follow-up to the Family Life Survey, he has given physical examinations to the people in the community who were interviewed in the survey. He has also recorded their life histories. The study was partially interrupted in the spring when Dr. Thomas went to Toronto to work at the Canadian Defence Research Medical Laboratories. Before he left, he had completed most of the physical examinations, and he plans to return to the study area periodically to fill in the gaps in the data and to reassess the life circumstances of certain of the people.

A master file, which brings together all available information on the people we have chosen in our surveys, was constructed in an effort to relate all the various sources of information on the vital statistics of the population of the county on May 1, 1952. This task, done under the supervision of Mrs. Jane M. Hughes, Mrs. Ruth O. Kent, and Mr. David B. Macklin, was undertaken with the hope that any later epidemiological assessments of the county could be compared with an accurate baseline.

4. The Western Nova Scotia Clinic

The Western Nova Scotia Clinic, formerly the Psychiatric Clinic, is now operating under the terms of the Memorandum of Understanding, which provided for the transfer, in January 1957, of the Clinic administration from Cornell University to the Western Nova Scotia Mental Health Group, Inc.

The members of the Board of Directors are:

John R. McCleave, M.D., Chairman	President of the Nova Scotia Medical Assoc.
James R. Ayer	Superintendent of the Digby Co. Power Board
Victor G. Cardoza	Journalist
Harvey E. Chisholm	Municipal Warden (resigned March 1958)
Harry Lewis	Deputy Municipal Warden (appointed March 1958)
Edward J. Theriault	Queen's Counsel
G. Victor Turnbull, D.D.S.	Dental Surgeon
L. S. Mushkat	Personnel Manager of Cosmos Imperial Mills, Yarmouth
John Robbins	Supervisor of Schools, Yarmouth
Ernest O. Hodge	Administrator, Yarmouth General Hospital (resigned October 1957)
Ruth Gilpatrick	Supervisor of Elementary Schools, Yarmouth (appointed October 1957)
Rev. Cyril Gough	Minister of Religion, Annapolis (appointed March 1958)
Pat McCarthy	Bookkeeper, Lincoln Paper Co., Annapolis Co. (resigned January 1958)
Rev. William J. Scott	Minister of Religion, Bear River, Digby Co. (appointed March 1958)
E. S. Elliott	Merchant, Lawrencetown, Annapolis Co. (appointed March 1958)
V. K. Rideout, M.D.	Regional Public Health Officer
Alexander H. Leighton, M.D.	Professor of Psychiatry (Social Psychiatry), Cornell University Medical College
Clyde Marshall, M.D.	Director of the Division of Mental Health, Dept. of Public Health, Province of Nova Scotia
Robert O. Jones, M.D.	Chairman of the Dept. of Psychiatry, Dalhousie University Medical School

R. Kenneth Beckwith, Insurance Agent, acts as Treasurer. Dr. G. A. W. Angus, who succeeded Dr. Herbert Davy as Executive Director of the Clinic in March, is Secretary of the Board. Dr. Leighton, Director of the Stirling County Study, is a member of both the Research and Recruitment Committees and attended most of the Board meetings.

The Clinic now serves three counties--Digby, Yarmouth, and Annapolis--with a population totaling about 65,000 persons. In addition to the facilities at Digby, a Clinic is maintained at Yarmouth where service is available several days each week, and, with the support of the recently formed Annapolis County Mental Health Association, it is hoped soon to open a third Clinic in Annapolis County. All three counties are represented on the Board of Directors.

During the year much of the time of the professional staff has had to be given to service, and opportunity for research has been minimal. The research work that has been possible is as follows:

The Clinic-Clergy groups, both at Annapolis and at Digby, have continued to meet for the discussion of matters of common interest.

Dr. Angus has been making some general observations preliminary to investigating under what conditions it might be practicable to initiate a service of primary prevention.

Mr. Lionel Vallée, Social Worker at the Clinic, is carrying out the field work for an intensive study of the Negro communities of Digby County. He is particularly interested in the role of the family in the process of transmission of values.

Members of the Clinic staff have continued to cooperate with other professional groups concerned with the health, welfare and guidance of the people in the community. They have addressed a variety of meetings, have attended others, and have helped educate the public in matters pertaining to community mental health.

In October the staff of the Clinic visited Ithaca to meet the Cornell staff and to gain some familiarity with the work in progress. Dr. Davy and Dr. Angus then traveled to New York City to visit the Department of Social Psychiatry at the Cornell University Medical College and to attend the Annual Conference of the Milbank Memorial Fund. In June Dr. Angus joined the Stirling County Study staff at the research conference held at Many Farms, Arizona (see Part III).

5. Study of a Small Town Psychiatric Clinic

Under a Russell Sage Foundation Grant, described in last year's report, the study of a small town psychiatric clinic as a sociocultural innovation progressed along a number of lines. In the summer of 1957 a survey was administered to a systematic random sample of the community as part of the attempt to assess attitudes and knowledge about the Clinic. An additional purpose was to investigate attitudes about the community itself and about other social resources.

Eight interviewers were employed for the survey--seven of them residents or past residents of the province. In addition, some key informant interviewing of members of the community and past members of the Clinic staff was done by the two researchers, Dr. William D. Longaker and Mrs. Alice Longaker.

Analysis of the survey materials was begun in the fall and has continued through the year. Coding and preliminary tabulations were completed in the spring. It is expected that the analysis will be essentially completed by mid-September.

Several people assisted in the preparation and analysis of the survey material. The main assistant was Mrs. Louise G. Richards, a graduate student holding a research assistantship. Contributing as consultants were Dr. George E. Poucher, of the Mental Health Clinic of Ithaca, and Dr. Gordon F. Streib, of the Department of Sociology and Anthropology.

Some effort has also been made to compare material from this survey with that gathered in earlier Stirling County Study surveys of a similar sample from the same area. This comparison provides some data on social and economic changes occurring during the period under study.

During the year, initial assessment of the considerable amount of Stirling sociocultural data relevant to this study has been undertaken, and planning for the reporting of the study was begun. Further field interviewing will be done during the summer of 1958.

6. Review of Articles, Presentations, and Outside Activities

Two articles by former staff members, which are based on research aspects of the Stirling County Study, were published in professional journals this year. The first of these appeared in almost final form in the Sixth Annual Report. The second is included in Part II of this

report. Stirling staff, including a former member, participated in editing and writing a volume related to the work of the Study. These publications are listed below, followed by two articles which have been accepted for early publication.

a. Publications:

Marc-Adélard Tremblay, "The Key Informant Technique: A Non-Ethnographic Application," American Anthropologist, Vol. 59, No. 4, August, 1957.

John Collier, Jr., "Photography in Anthropology: A Report on Two Experiments," American Anthropologist, Vol. 59, No. 5, October, 1957.

Alexander H. Leighton, John A. Clausen, Robert N. Wilson, eds., Explorations in Social Psychiatry. New York: Basic Books, Inc., 1957.

E. J. Cleveland and W. D. Longaker, "Neurotic Patterns in the Family," in Explorations in Social Psychiatry, edited by Leighton, Clausen, and Wilson. New York: Basic Books, Inc., 1957.

Alexander H. Leighton and Alice Longaker, "The Psychiatric Clinic as a Community Innovation," in Explorations in Social Psychiatry, edited by Leighton, Clausen, and Wilson. New York: Basic Books, Inc., 1957.

b. Accepted for publication:

Alexander H. Leighton, "Mental Illness and Acculturation," New York Academy of Medicine Laity Lectures, 1955-1956, Series scheduled for publication by International Universities Press.

Allister M. Macmillan, "A Survey Technique for Estimating the Prevalence of Psychoneurotic and Related Types of Disorders in Communities," to appear in Symposium on the Epidemiology of Mental Disorder, American Association for the Advancement of Science, 1956, Benjamin Pasamanick, editor. (Presented at the Annual Meetings in New York City, December, 1956.)

Staff members, in addition to reporting the work of the project to several academic groups, have continued to collaborate with other

health studies and to participate in various conferences and committees not directly related to the Stirling County Study. The following describes activities of the staff this year.

Alexander H. Leighton held a fellowship at the Center for Advanced Study in the Behavioral Sciences at Stanford, California, 1957-58; served as Technical Consultant to the Milbank Memorial Fund; served as member of the Surgeon General's Advisory Committee on Indian Health and member of the Committee on International Anthropology of the National Research Council; participated in the World Health Organization's Study Group on Mental-Health Aspects of the Peaceful Uses of Atomic Energy, Geneva, Switzerland, October 20-26, 1957; attended the conference on "Methodology of Mental Health Studies" sponsored by the World Federation of Mental Health at Princeton, New Jersey, April 17-22, 1958; consulted by the Executive Director of the New York State Charities Aid Association.

Allister M. Macmillan served as member of the Surgeon General's Advisory Committee of the National Health Survey; consulted by the Survey Research Center, Michigan, National Survey for the Joint Commission on Mental Illness and Health; presented a review of the Stirling County Study to the Research Methods Seminar, Department of Clinical and Preventive Medicine, Cornell University, January, 1958; presented "Concept of Normality vs. Abnormality from a Psychologist's Point of View" to medical students at Dalhousie University, Halifax, Nova Scotia, February, 1958.

Charles C. Hughes presented "Stirling County Study Field Methods" to the Anthropology Theory Seminar, Department of Sociology and Anthropology, Cornell University, May, 1958.

Dorothea C. Leighton presented "Overview of the Stirling County Study" to the Anthropology Club, Stanford University, May, 1958.

Allister M. Macmillan, Charles C. Hughes, and Alice Longaker were consulted by the Warren County Mental Hygiene Association, Glens Falls, New York, July, 1957.

Alexander H. Leighton and Dorothea C. Leighton presented "An Outline of the Methods and Findings of the Stirling County Study" to the Bay Area Anthropological Association of California, March, 1958.

Part II - SUBSTANTIVE REPORTS

A variety of papers has been included as substantive reports in this annual report. The first is based on a study done in the early years of the project and appeared as a published article in October, 1957. The type of photographic experiment discussed in the article is presented by Mr. Collier at greater length in a book now in manuscript form.

The second paper is a draft taken from work currently being done for Volume III of the Study. It pertains to analysis of differential patterns of psychiatric disorder in the contrasting focus areas mentioned early in this report.

Problems of interpersonal relations between the research workers and the people being studied are the subject of the third paper included. This paper is reproduced from an article published in Bulletin of the Maritime Psychological Association, Vol. 6, No. 1, Spring, 1957.

Finally, another paper not previously reproduced in an annual report is also presented. It reports results of a study sponsored by the Canadian Government and using data taken from the Stirling County Study. The problem was that of the relationship between "organic" and "psychiatric" types of symptoms.

/As is our practice, in all publications pseudonyms are always used for people and places mentioned in the text. However, for the benefit of readers in Nova Scotia who may wish a more specific reference for geographic terms used in the substantive reports included here, the following list is appended./

KEY TO PSEUDONYMS

Code Name	Actual Name
The Bog	Barrens
Bristol	Digby Town
Charleston	Culloden
Fairhaven	Freeport
Great Cove	Broad Cove
Hilltop	Hillgrove
Jonestown	Marshalltown
Lavallee (L'Anse des Lavallee)	Belliveau's Cove
Loomervale	Weaver Settlement
Monkeytown	Chinatown
Northwest Jonesville	Northwest Marshalltown
Point Road	Lighthouse Road
Port Harmony	Plympton
Portsmouth	Weymouth
Pulp Creek	Weymouth Mills
Robertsville	Ashmore
Stirling County	Digby County

PHOTOGRAPHY IN ANTHROPOLOGY: A REPORT ON TWO EXPERIMENTS*

John Collier, Jr.

INTRODUCTION

Can photography be a source of direct research in anthropology? This query motivated a study of the properties and methods by which photography could support social science research. The experiment was the collaborative effort of the writer, a professional documentary photographer with no formal training in the sciences, and Alexander H. Leighton, an anthropologist.[1]

This research, extending over three years, examined practical ways in which photography could widen the field of scientific recognition and accelerate the processes of research. The areas examined included: photographic recording of direct observation; rapid surveying of ecological, technological, and sociocultural patterns; the use of photographs as an aid to interviewing; and the techniques and research possibilities of co-ordinating photographs with field notes. This report presents two instances, (1) an application of photographic surveying and (2) a more or less controlled experiment on the aid of photography in interviewing.

These studies were undertaken and completed while the author was a member of a group research team, Cornell's Stirling County Study, an interdisciplinary project on the relation of environment to mental health. Here the practical as well as the theoretical aspects could be developed and tested under field circumstances. Stirling is the research name for a county in the

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* Reproduced from article published in AMERICAN ANTHROPOLOGIST, Vol. 59, No. 5, October, 1957.

Maritimes of Canada; its population is of English and Acadian-French extraction, its economy a complex of farming, fishing, and lumbering. Aspects of this photographic experiment were later studied in an entirely different environment, among the Navaho of the American Southwest.[2]

Photography is a long-established tool in scientific research, and is widely used by anthropologists. Yet, there is a difference between its use in, let us say, astronomy, and in ethnography, for the physical scientist looks directly at photographs for research data, whereas anthropologists have generally used the camera solely to support their findings by illustration.[3]

From the outset of this study, two theoretical questions were encountered: "How can photographs function other than as illustration?" and "How can you apply photographic imagery to direct research?" We felt these queries struck at a major orientation of modern ethnography: its deep distrust of visual observation. For, perhaps in reaction to the earlier explorer-type ethnographers, the modern scholar considers that visual observation provides no more than an impression and affords no reliable measurement of the environment.

A factor that has discouraged the use of photography is the trend in anthropology away from the study of the shell of society inward to the emotional, psychic, and intellectual expressions of man. "We are not so much concerned with how a man looks as with what a man thinks." Can other material than the outer form of things be approached through graphic analysis?

Possibly methods could be developed within photography that could meet these requirements of research. The camera is an automative device which can permanently engrave the visual impression of an instant and can also

compensate in various ways for the shortcomings attributed to human impression. The mechanical eye of the lens and the automatic memory of film are the camera's assets for accurate reportage. This automatic documentation appears to go beyond the literal image of environment. Photographs also catch many elements of the emotional currents within situations that are involved in a man's reactions to his cultural circumstance. Here is significant material that might be abstracted if methods of analysis were developed that could give us clues to the meanings as well as the photographic outlines of things. The complex material and social relationships that can be found in photographic recordings present a host of data within the concerns of anthropology.

The two experiments reported in this paper followed an initial period of the photographic project which I will mention briefly. This initial phase was the development of a three-thousand negative file on the natural and human environment of Stirling, paralleling the other efforts of the Project in which field workers were describing the various technologies. As the Stirling County Study was concerned with community development as well as psychiatric behavior, the question was not so much whether I could gather a large body of photographic information, but whether I could do so without disturbing the goodwill of the whole project. My activities were critically observed; Leighton and the research team decided that the work was not disruptive, and in the next field period I began to apply photography to more sensitive areas.

EXPERIMENT IN EVALUATION

In the meantime, the Stirling County Study moved from its descriptive phase into one of objective examination. A series of variables had been hypothesized as being related to the mental health of the area, and this particular year's research dealt directly with problems of adjustment within these social, economic, and physical variables.

A large number of field workers were in Stirling in the summer of 1951, and all shared concern with one aspect of the study: the geographic distribution of the hypothesized variables to enable correlations between the incidence of each variable and the anticipated results of later psychiatric studies. In this connection a housing survey had been planned to learn about the distribution of affluence and poverty. The plan was for the field personnel, in teams of two, to drive along the roads of communities randomly selected and rate each house on a three point scale of good, average, and poor, for the following criteria: size of house; condition of roof and chimney; condition of walls; condition of yard; and condition of out-buildings. Ratings were then to be used as indices of the position of the various communities on the poverty-affluence continuu.

I was not at first involved in this activity. The teams set forth to make their first judgments, each team rating independently the same series of houses, in order to test the accuracy of their rating methods. When the teams returned and compared notes, the same house had too often been given different ratings. This had been expected, for the purpose of the trial run had been to co-ordinate bases for judgment. Models of the various typologies and standard scales had to be established to define a poor roof or an unkempt yard. The ensuing discussion revealed some significant points. Apparently the field workers were unconsciously judging houses in respect to

their personal backgrounds. A poor house for an urban dweller was not necessarily considered a poor house by a man who had grown up in the country. Regional influences also gave a different interpretation to phrases like "well-painted" or "poor repair." Order, neatness, and repair are states that reflect conflicting values. The group still had no positive understanding after comparing their findings and trying to describe to one another just what each meant by a well kept-up house, and the teams must needs drive around in a body and examine each house on the spot. Obviously this was neither advisable nor economical. At this point photography was considered. I suggested that I tour the county and photograph every level of housing I could find. Perhaps the photographic results could throw light on their problem.

Two aims were in my mind as I gathered a wide variety of housing types in communities of varying economic levels: to gather photographs that could be studied almost as completely as the houses themselves, and to do so as rapidly as possible without causing alarm in the communities through which I would pass. This was one circumstance in which we were forced to operate without permission, but we would be photographing from an area of public domain, the public right of way. Still, there was a chance that even this superficial survey might cause anxiety, so I operated as discreetly as possible. I took pictures from the window of my car in about the same perspective as the teams' judgments would be made, and from a considerable distance (with a long lens) so that bystanders and people within the houses would probably not be certain just what I was photographing. I traveled fast, barely stopping in my documentation, and in a day gathered over fifty housing samples of all classes. I developed

the film and made eight-by-ten enlargements of the range of housing represented. Forty-eight hours after the beginning of the housing survey, the photographic samples were in hand, and the teams gathered again to discuss the models for the typology.

The research workers sat in a circle and the photographs were passed around. Each member of the team studied the numbered photograph, wrote down his rating, and passed it on to the next, who in turn rated it independently, until everyone had rated the complete sample. The ratings were then compared. In general discussion, and by turning to the photographs as precise examples of what was meant, the group was able to define their criteria and co-ordinate their judgments. Not only was the photographic image sufficiently defined to permit critical examination of all characteristics visible from the road, but also the houses could in this way be compared critically with each other. The material could be analyzed with a comparability that was not otherwise feasible in such a rapid survey. When the prints were laid out together, each component could be viewed and measured simultaneously throughout the graphic sample. Further, the graphic evidence insured that quality would not be measured by some previously inculcated criteria, but in its own realistic environmental relationship.

Here was a working demonstration of the camera's ability to record visual impression so reliably that it could be carried into the laboratory for refined analysis. After the definition of values, the teams were able to complete their sampling without further confusion. In this instance, photography was used only to establish the typology, and the field workers completed the survey by direct observation of the houses. The whole study

could have been made photographically; such a technique would be especially valuable when the factors to be surveyed were too complex for reliable direct observation, or when a comparative analysis after a time interval was anticipated.

A host of other circumstances particularly adapted to photographic inspection are suggested by the housing survey. In theory, not only can studies of physical properties be supported by graphic records, but studies of social situations as well. Bateson and Mead (1942) did just this in Bali. Sociometric designs of many kinds might be understood further through photo-analysis.

EXPERIMENT IN INTERVIEWING

It was evident from this experiment that we could interpret the data in photographs of houses or other similar abstractions. But we would still be burdened by the initial limitations of human impression; a situation not understood at the time a photograph was made would not be any more understandable in the photographic print. To use photography in the analysis of complex sociometric situations we must find a way to read from photographs evidence that was not understandable to the camera-observer. One of the Stirling field workers had discovered the year before that local people could read a great deal of content into a map and that by interviewing with it he learned a lot without having to cover the ground himself. Why could we not consider the photograph a cultural map that could be read with equal clarity by the knowing informant? Could this process allow us to analyze the content of photographs--and therefore of situations--that we had not previously understood?

We decided to make controlled tests of this semiprojective technique. We chose a problem that overlapped almost everyone's research, the acculturation and migration of the French-Acadians to the English industrial town of Bristol, thereby adding to knowledge as well as testing technique. We hoped to gain clues to the Acadian acculturation process. How did the Acadian migrants adjust to the English environment? What were the goals of this movement? And were there intermediary "stepping stone" communities between the Acadian half of the county and the English town of Bristol?

Our research design involved four Acadian informants, two interviewed with photographs and two interviewed solely with verbal questions, to serve as a control. The interviewing was to be done by two Stirling field workers; one, William A. Magill, was studying the English social structure, and the other, Marc-Adélard Tremblay, was studying Acadian acculturation. The interviews were to be with matched informants--two living in what we presumed was a migratory belt midway between the Acadian half of the county and Bristol (the largest town and most important industrial and commercial center, predominantly English), and two living in Bristol itself. As a further control, the interviewers were to change position; the one who interviewed with photographs in the first set of tests would interview without them in the second, and vice versa. We planned three complete interviews with each informant, structured around three levels of personal experience: knowledge about wage work in Bristol, knowledge about their home communities, and sentiments about their personal lives. A "check" interview with photographs would be made with informants previously interviewed without photographs, to observe how the introduction of pictures might affect the character of their responses.

I gathered the pictures for the first interview, a rapid file on all the town's industries--lumber milling, clam packing, and fish processing plants. The documentation concentrated on three features: the nature of the individual industries, the conditions under which men worked, and the people who were employed in each plant. It was our goal to find out how our informants felt about their work and whom they could recognize in the various plants; this last information was valuable to our study, for if large numbers of people could be recognized we would have evidence on where Acadians worked and where they came from. A set of standard questions was composed around the photographs to structure the questioning and to allow for greater comparability between interviews made with photographs and control interviews made without them.

The Acadians chosen for the first interviews lived across the road from each other in the straggling community of Robertsville, midway between Bristol and the Acadian half of the county. The men's names were Plenn and Chiasson. Both worked in Bristol in the Morris sawmill and box factory; both were married to English girls; both had settled on land acquired through marriage. Both wives worked, Mrs. Plenn in the box factory, along with her daughter by a former marriage, and Mrs. Chiasson in the Post Office in Robertsville. Both Plenn and Chiasson maintained small farms on which they worked in their spare time.

Tremblay was to interview Chiasson without photographic aids, while Magill interviewed Plenn with photographs. It was important that I follow closely the development of the photographic interviewing in order to plan the content of the following tests and to write up the experiment. Therefore, despite the imbalance, it was decided that I should be present at all picture interviews.

As Magill and I drove into Robertsville on the evening of the first interview with George Plenn, we saw Tremblay's car parked in front of Chiasson's mail box. The Plenns met us at the door in their work clothes. Hospitable but tense, Mrs. Plenn led us through a darkened kitchen to a brightly lit dining room. We all sat down around the table, and there was a moment of embarrassed silence until we explained again the purpose of our investigation. Then, with Violet and George Plenn on one side of the table and Magill and myself on the other, the pictures were passed in a circle and Magill noted the comments carefully in his note book, conspicuously open on the table. Each print was numbered but uncaptioned. The interview was of a directive type and proceeded according to our previously formulated questions; however, there was flexibility, and new questions were expected to emerge as personal experiences and feelings were recounted.

The first photograph was a panoramic vista of the Morris mill, the second a close-up of the mill with more detail. On first inspection George had difficulty orienting himself, and finally turned the picture compass-wise (correctly), locating north, east, south, and west. Without hesitation he then named the main structures of the mill, including smaller units such as the bunkhouse and cookshed. He gave information on who lived in the bunkhouse and where they came from. These photographs were followed by detailed studies of all parts of the plant. Violet and George quickly identified all visable details and machines and talked at length about the different jobs shown in the pictures. They also exclaimed over the dangerous piles of scrap heaped about the machines, and admitted that the refuse was a source of annoyance and a danger to life. They identified most of the workers and told where they came from.

After the Morris pictures, we showed them the fish plant; they looked with interest but had nothing to say. We then showed them the clam-digging and clam-processing photographs; they were also unable to comment on these, and it became clear that they knew only the Morris mill and only the people who worked there. This suggested the possibility that each Bristol plant tended to draw its help from a distinct area. Then we showed them the streets of Bristol. Here also they knew no one, and it was evident that they rarely went into the town itself. They exclaimed, "When we are through work we are sick of Bristol and want to come right home . . . . When we want something during the week we buy it at the corner store, and on Saturday night we always do our shopping in Portsmouth" (a town on the edge of the Acadian half of the county, in the opposite direction from Bristol). The interview lasted more than an hour. The session was probably tiring for the Plenns, as they had to get up very early to do their farm chores and arrive for work in Bristol at seven, but they studied each picture with great interest throughout the interview.

Across the road Tremblay had been interviewing Chiasson on the same subjects. A shrewder and more independent man than Plenn, Chiasson spoke with fluency and offered many intelligent observations on his life. Plenn seemed a limited person, unimpressed by circumstances beyond his immediate pattern; he seemed anxious to oblige but expressed himself poorly.

The quality of data gleaned from each interview was excellent, though quite different in character. Each covered the same material, but with different perspective and depth. The photographic interview got considerably more concrete information on the structure and processes of the Morris mill, more emphatic expressions of dislike for certain aspects

of this industrial work, and much more specific information on the other workers. The non-photo interviews strayed from the course of the research to include more distantly related associations and data; the informant talked more about himself and much of the interview was semi-autobiographical. In this the two interviews differed, for the Plenn interview stayed on the track of the picture probes--which no doubt cut down on the introspective observations we might have obtained by allowing our informant to choose his way.

Another difference was that the note book caused no concern with Plenn, while it apparently upset Chiasson's composure. We feel that this was because the pictures served as a second subject; both interviewer and informant fastened their attention on the pictures, which relieved the strain of being questioned directly. In spirit, both parties were questioning the photographs.

The first test revealed still another function of the photo-interview. In non-photo interviewing, statements of ignorance are difficult to appraise: has the informant forgotten, has he misunderstood the question, or is he holding back? We feel that the photographic probes sharpened Plenn's memory, reduced the area of misunderstanding, and compelled him to stick to the truth. We could be confident that Violet and George really had no contact with the other industries or the people who worked there, for reviewing the pictures with the Plenns to some extent approximated visiting the plants with them in person. The negativity of their response to the Bristol photos suggested the group isolation of Acadian wage workers, who form islands with little or no interaction with

the culture of this English town. Wage work in Bristol seemed less influential in the acculturation process than we had presumed, for Plenn gave evidence of retaining his roots in the Acadian area and even in the backwoods village from which he came. The non-photo interviews did not present this picture clearly; it left us with a hazy picture of Chiasson's knowledge of and interaction in Bristol. Many of his verbalizations were impressionistic, and we had no way of checking whether he really knew much about the information he offered.

The second interviews were structured around the home values of the Acadians, to appraise the depth of satisfaction they felt toward their traditional mode of life and the sense of independence gained by a diversified pattern of farming and lumbering or fishing. This variety and independence would normally be lost upon migration to urban Bristol. If their sentiments were deeply rooted, could the frustration be a cause of serious disturbance?

We made a complete document of the Plenns' farm and family interaction to serve as a graphic basis for the study of home values. Gathering this material was different from photographing house types for it required the utmost co-operation of the family. They had either to enjoy it or to feel that their sacrifice of privacy was being made for a worthy cause. I presented the proposal to Plenn by stating that we were interested in life on a farm as compared with life in Bristol, and that we hoped photographs of their home might help us understand these two ways of life more clearly. This explanation was accepted.

The depth of this photographic study must have made great demands on the Plenns. In return for the privilege, we knew we must conduct

ourselves sincerely and openly, and operate on as equal a plane as possible, so I decided to make the photographic occasion a family visit. The first photograph was made of George and Violet and Violet's daughter pulling up to the door in their truck on the return from their morning's work. Violet seemed pleased that I had brought along my wife and three-year-old son. They all grinned broadly as they climbed out of the truck, and the women went into the kitchen while George and I set off to see the farm.

It was evident that George was enjoying himself and that the farm was the center of his life. When we returned to the kitchen for a late lunch, he told Violet that I had photographed everything, and sat down to lunch as if he were very pleased. I continued my photography, explaining that food made the most important picture of all. Violet's face lit with a smile when I made a close-up of some home-grown vegetables. She laughed and said, "Sure, those people in Bristol never get such food. We get everything from our farm but sugar--flour, sugar, and tea, and such things."

The afternoon passed in much the same mood, as I depicted the family working the garden, milking the cows, rounding up the geese, etc. My own family had a very good time, and I believe the Plenns did too. They remained jovial and asked us to stay on to supper.

Magill and I returned for the second interview in which we would be showing the Plenns pictures of themselves, sitting in the very location where the pictures had been made. This was an entirely different sort of test than identifying pictures of the Morris mill. In effect, we would be holding up a mirror to the Plenns and asking them what they saw. When

we arrived, Violet ushered us rapidly into the dining room, and all eyes were on the folio of pictures. The atmosphere had changed from our first interview. Everyone knew what to expect, and the family was impatient to get started. The first picture of the series was of the Plenns arriving home.

> Magill: Here you are returning home on Saturday. How do you feel as you drive home?
> Violet: I'm just tickled!
> Plenn, with a broad grin: I feel good.
> Magill: Are you tired after work?
> Plenn: Yes, I'm tired.
> Violet: Ennis (the foreman) just drives you till you're crawling. Yes, it's very tiring.[4]

At this point we observed a curious development: while George, Violet, and Violet's daughter looked through the pictures of home scenes with great intensity and amusement, their comments were just as often about the work of the mill. They seemed more anxious to express their feelings about their work than they had been in the first interview. One had the impression that as they looked at the homely scenes, the harshness of the mill environment was brought to mind. It was as if they were unconsciously comparing their farm, which gave them great satisfaction, with this industrial environment which appeared more and more as the interview progressed to be distasteful to them all. As they looked at the pictures of the farm they spoke of quitting work, saying that they could make a good living on their farm if they were fired. They talked about other workers who had no farms. In answer to a query as to what they thought about as they worked at the mill, Plenn said, "I plan all day." Violet said, "We think about the farm and what we are going to do when we get home." When asked what the others thought about, George added, "They go home to sleep, that's all." Violet: "No, there is nothing to do but sleep."

When we questioned them about the central topic of our investigation--would they move to town--they gave us a highly charged picture of their concept of life in Bristol, adding, "What would we think about all day at the Morris mill if we didn't have the animals on our minds?"

This was a fairly short interview, but completely to the point. There was little doubt that the Plenns strongly resisted moving to Bristol under any circumstances.

The third and last interview with the Plenns was structured around their home community and their contact with Portsmouth, where they did their weekly shopping. We particularly wanted to know whether Robertsville, midway between the Acadian half of the county and the town of Bristol, was a stepping-stone in migration. The method we developed was to make a complete housing survey of Robertsville, to identify all its inhabitants, and to track the Plenns' social contact throughout the community. I photographed every house in sequence, so that we could obtain the complete sociometric picture. Though Robertsville is two miles long, all the houses are visible from the road, and I completed the survey in an hour, snapping pictures from the car window. I added pictures of a picnic at the Catholic Church at Port Harmony, to which parish Robertsville belongs, and a set of photographs of Saturday evening in Portsmouth when the Plenns were doing their weekly shopping.

On the evening of our third interview we found George very tired after his day at the mill, but Violet was brisk and cheerful. She seemed anxious to see the pictures and made room for us around the table, calling in George and her daughter. We started the interview with the

housing survey. Houses were identified slowly and with great care, moving in an orderly fashion down the road. The names of the occupants were always given, where the head of the household worked (if he worked), and then usually something about the family's history.

> Picture 23: That's Willy Chiasson's house (owns). He's the one with the whiskers. He rents it to a widow woman. (How much?) $5.00 a month. She gets a pension from the States.
> Picture 24: Willy Chiasson's house. He owns half of Robertsville. He bought it from Capt. Campeau. He's got lots of money. He has a wife and four children. He owns a big farm at Pulp Creek. He has a store and woodland. He has lots of cows. One man looks after the place, and one man drives the truck. Russell Hawley works in the woods with him.
> Picture 25: Richard Mouser's house. He stays home. He has cows. Sells cream. He has some hens. He works on the road on and off.

House by house, we learned about Robertsville. They told us of its farming economy, and its position between the Acadian area and Bristol. It was not a transient or migratory community; its population was fairly stationary, with family names in some cases going back three generations. It was a marginal community that had once been predominantly English, whereas today it was predominantly French; though many men worked in Bristol, many others were farmers or retired people. It had little influence on the migration of the Acadians. We had hoped to find points of social interaction between the Plenns and the community, but to our surprise there were almost none. Most of their social relationships were with George's family in the backwoods of the Acadian region.

We next showed the Catholic Church picnic in Port Harmony, a few miles from the Plenns' home. Was this crowded gathering evidence of a growing Acadian population in this area? The Plenns knew many people at the picnic, but their identification told us that the majority of the crowd came from other parts of the county.

In the pictures of Saturday evening in Portsmouth, our informants were able to recognize a great many people and tell where they came from. Two points emerged. From the way Violet could recognize people--giving their names, occupations, and origins--we could assume that with several informants we could construct a rough index of who came to Portsmouth on Saturday night and arrive at a pattern of French-English participation. At the same time, the number of people that could not be recognized offered clues to the extent of the Plenns' interaction in the area. Both the positive and negative responses served to show the pattern more closely.

This interview was the longest; though George became very tired, Violet's attention never flagged. Scores of personalities had been identified and added to our file, and very revealing insights given on the Robertsville, Port Harmony, and Portsmouth ethnic structure.

Meanwhile, Tremblay returned for a second interview with Chiasson. While Tremblay considered the first had been "an above-average interview," he felt the second was "quite a different matter." His notes state, "I realized he was not too talkative . . . . The answers to my questions were short and vague." Though the interview ranged over wide territory, from Chiasson's war experiences to proposed improvements on his home, Tremblay could not hold Chiasson within the planned structure of the tests, the subject of home values and his reasons for living in Robertsville rather than Bristol. In these areas his responses fell far below those of the Plenns; it must be borne in mind that we were testing for information on precise subjects.

This "diminishing return" in interviewing is not uncommon; where the first interview may be good, the second may be indifferent, and a third repetitive or impossible. An interview can empty the outer mind of its more absorbing preoccupations, so that it may require time for new feelings to gather. It appeared that photographs stimulated a restoration of expression. The imagery opened doors of memory and released emotions about forgotten circumstances, which allowed a second interview to be as rewarding as the first.

At this point an unforeseen element entered the experiment and upset the balance of our tests. Photographs can be objects of consuming curiosity. This attraction had helped us throughout the Plenn interviews, but it operated in reverse in the Chiasson test. Robertsville was a tiny settlement; Chiasson had seen me taking pictures and had heard that the Plenns had seen them. At the end of the second interview he made it clear that he wanted to see the pictures too, and that Tremblay had better bring them if he wanted to come again. This sudden demand, plus the less satisfactory response to the second interview, made Tremblay feel the third non-photo interview should be cancelled, and that he should proceed at once to the "check" interview with photographs. This was agreed upon, and henceforth we limited the test to two visits, with and without pictures, with a "check" interview following the latter.

When Tremblay returned for the "check" interview, using the Bristol industry and street scenes, the tone of the interaction changed. Chiasson greeted Tremblay with spirit and called his wife from her ironing to see the photographs. She looked at the pictures with interest and listened

to the interview (which she had not done in the first two visits), though she made few verbal contributions. The interview proceeded in a fashion similar to the Plenn tests. Chiasson identified people and processes as George had done, and like the Plenns he could identify almost nothing in pictures of other plants or on the streets of Bristol. Like the Plenns, the Chiassons evidently had little contact with the English town. This interview was significant for its similarity to the first Plenn interview; the number of responses on the industrial picture was comparable, and the quality of the information much the same. Both Plenn and Chiasson had identified all that they recognized, and the nature of the photographs dictated both the depth and scope of the information.

The second pair of informants for our tests were drawn from workers employed in the Scotia fish processing plant in Bristol. Both men had migrated to a suburb of Bristol from the area between Robertsville and Bristol, and both had married English girls whom they had met in the fish plant. One, Lawrence Dumas, had worked many years in the plant, owned his home, and had six children. The second, John Campeau, was a younger man with a wife and small baby, lived in a rented flat, and worked on and off at the plant.

According to our design, at this point we switched interviewers. Magill, who had worked with the photographic probes, interviewed Campeau without pictures, while Tremblay interviewed Dumas with pictures.

Our reception at the Dumas home was markedly different from the open reception of the Plenn household. Dumas was a silent man whose face was marked with strain, and though his wife was less reserved, she

too seemed to be feeling unexpressed anxiety. As previously, we started the interview with photographs of the plant in which the informant worked, in this case the Scotia fish plant. When we laid a photograph before Dumas, he would talk; when we took it away, he sat silent. Despite his reticence, he and his wife were able to identify most of the workers and to describe clearly what was happening in each picture.

When we showed pictures of the other plants, they revealed that they knew little about them. Only when we showed the Morris mill did Dumas come alert, explaining that he had worked there eighteen years ago. Then we showed the street scenes of Bristol on a Saturday night. In five of these pictures they recognized no one, commenting that most of them must be from out of town. In the sixth picture they named three people.

Because we had to compress our material into two sessions, we had added to the first photo-interview a set of pictures of a Bristol Catholic Church bazaar, comparable to those on the Port Harmony Church picnic used in the third Plenn interview. We had assumed from Dumas' Acadian background that they were Catholics, but when we showed these pictures, Lawrence "manifested a sense of acute embarrassment." Tremblay, himself a Catholic, was conscious that we had moved in on very emotional ground. The couple finally told us with some agitation that they had been married in the United Church, of which Mrs. Dumas had been a member. This information was given in an emotional manner, as if it were something they would have preferred not to discuss.

This demonstrated another potential. Photographs can trigger responses that might lie submerged in verbal interviewing. Visual reminders can shatter the composure of a guarded reply and cause the

informant to blurt out submerged feelings or to reveal his emotional state by embarrassed silence, either of which can be eloquent to the sensitive field worker.

Although the situation thus uncovered was more delicate than anything we had encountered in Robertsville, we decided to go ahead with the home photographs in the same way we had with the Plenn family. In taking the pictures, we discovered that the children's major interest was working at the Golf Club nearby, so we covered these activities too. We also repeated the survey of houses in the community in the hope of tracing the pattern of the family's social interaction, which we felt important to understanding Dumas' adjustment as a recent migrant to an English community. And we added pictures of a supper at the United Church, suspecting that Mrs. Dumas might also have gone through strain and adjustment in marrying an Acadian, and hoping she would tell us of her social relations and contact within the church group. Thus the pictures for the second Dumas interview were comparable to those of the second and third Plenn interviews combined.

We returned for the second interview prepared to find a continuation of tension accumulated from the first session. Instead, we were met cordially with an air of expectation. It was evident that the family was excited to see the photographs of their home life, that this had gratified them, and that whatever reluctance they had about further questioning was overcome by curiosity about the pictures.

As at the Plenns', the photographs were shown one at a time and the family's elation made us wonder whether they had ever before seen pictures of themselves except in fuzzy snapshots. They could hardly contain themselves as they waited for the next picture.

These home studies revealed the process of urbanization. Dumas loved his garden, but only he and his wife worked it. Mrs. Dumas was an accomplished seamstress and cook, but her teen-age daughter scarcely knew the rudiments of these arts--she was learning to sew in school, her mother said.

We showed the pictures of the Bristol Golf Club, and the children quickly identified all the caddies and talked with warmth about the various personalities. There was no reference to other children coming to the house to play. The children's interaction seemed outside the family and centered about the links where they played and caddied whenever they had free time.

> Tremblay: Do you think the links are a good place for the children to work?
> Dumas: It gets them out of doors and makes them walk around.
> Mrs. Dumas: If they are on the links we know they are not in trouble somewhere else.

The church supper pictures gave us only negative insight. Mrs. Dumas could recognize few people and couldn't even pick out the new preacher who had arrived a month or two before. It was clear the family rarely participated in United Church activities, and this intensified the picture of loneliness in which the parents lived.

We showed photographs of some seventy-five houses in the Dumas' neighborhood, taken in precise sequence. They moved from picture to picture, giving names, occupation, and length of residence in the area. As house after house passed with only a handful identified as either friends or relatives, we arrived at a fair understanding of the isolation associated with migration. Stepping from one social grouping to another is not an easy process. Mrs. Dumas shared the loneliness of her husband in his move from the semirural Acadian to an urban English environment.

Meanwhile, Magill was interviewing John Campeau with verbal questions. The first interview gathered reasonably good data about the life of the informant. Campeau was more outgoing than Dumas and welcomed the interview as a relief from boredom, whereas Dumas had not enjoyed the experience at first. Campeau gave fairly adequate answers, but the first Dumas interview was considerably more specific.

When Magill arrived for the second interview, Campeau and his wife were sitting in the same position in which he first found them. As in the first session, Campeau gave a good interview in the field of general information about his life, but in the area of our interest--the movement of people, where they lived and where they worked--his statements were generalized. Campeau had worked around Bristol in various jobs over the past few years, and should have been able to give more explicit information. The surface of knowledge had apparently been skimmed, and the second interview leaned toward repetition.

The "check" interview with Campeau was the last in the experiment. Again the Campeaus were in the same chairs, as if they had never altered their preoccupations or positions throughout the week. But when the pictures were introduced, Mrs. Campeau for the first time laid down her True Romance and drew her chair up to the table to watch.

The interview lasted three hours, whereas the non-picture interviews had lasted scarcely two. Its content was compact and in direct response to the photographs and questions; it was primarily factual. Let us listen in on it:

Campeau: (Picture 11) (Laughter) Alfred Strong. It's not a good picture of him. Henry Hagen. He lives upstairs. Billy Steen of Jonestown.
Magill: Do you visit upstairs?
Campeau: We go up every night, play cards, 500, rummy. The women talk. They're cooking fish there. There are nine racks. (Picture 12) They're cleaning fish out. Messy job. Men pick out big bones. Girls pick out small black stuff. Everything is on a belt that moves along slowly. That's in the cannery. Jim Short, the Point Road. Jane Cawkins, Jonestown. Yvonne Currey, Jonestown. Her husband works there too. Hyacinthe Blanchet (Mrs. Reuben Blanchet) a Campeau from Jonestown. She has five children. She has a house-keeper. Reuben Blanchet, Jonestown. Stella Eisenhower, Hilltop. Marie Campeau, Jonestown (a sister to John). Sophie Campeau (not related).

Responses to the photographs were simple and in keeping with the type of evidence presented; it might be called "shorthand" on photographic content. When the data were assembled they offered a clear explanation of the research content of the pictures. The phenomenon which interested us most was the specificity of the information as compared to Campeau's response in the non-photo interviews to the question of where people in his plant came from: "Oh, my God, Jonestown, different places. Some from Bristol, some from the Point, Charleston, Great Cove!"

CONCLUSION AND FURTHER OBSERVATIONS

The characteristics of the two methods of interviewing can be simply stated. The material obtained with photographs was precise and at times even encyclopedic; the control interviews were less structured, rambling, and freer in association. Statements in the photo-interviews were in direct response to the graphic probes and differed in character as the content of the pictures differed, whereas the character of the control interviews seemed rather to be governed by the mood of the informants.

In an attempt to compare the interviews objectively, we devised a system of coding each statement according to content in one of ten categories: Work, People, Community, Family, Migration, Religion, Pleasure, Ethnic Relations, Interaction, and Associations. This supplied us with an approximate statistical picture of our results. The two techniques did not differ significantly in total number of responses, but in distribution of responses they were very different indeed. The analysis showed clearly that the pictures dictated the content of the interview, and more effectively than did the verbal probes. Pictures of Bristol industries elicited technological information, details about fellow workers, and values in relation to work; home pictures centered the discussion on family patterns and values; and housing pictures brought information about the community. In these areas, the picture interviews were impressively longer and more complete than the non-picture interviews. Whenever the pictures covered a distinct area, we consistently obtained compact the definitive answers. This was especially true in the study of the home community; no information in the non-picture interviews could be compared to the flood of material obtained from the housing surveys.

Another point borne out by the analysis was that, while both of the second non-picture interviews had been less full than the first and showed a decline in the informants' responsiveness, the "check" interviews reversed the trend and produced more material. This can be interpreted as an indication that photographs can be stimulating and can help to overcome the fatique and repetition often encountered in verbal interviews. It is also safe to assume that the photographs were an aid to

rapport in opening the field of discussion, whereas in the control interviews we sometimes had to press against resistance and apathy.

The manner in which we carried out these photo-interviews created a situation that could at one point limit an intensive investigation and at another greatly accelerate it. The presence of the pictures invited group participation. This was a spontaneous circumstance, unanticipated, and beyond our control. All the photo-interviews involved the complete family, and this resulted in two things: first, it dictated the plane of response, and statements were of a more public nature than they might have been had we interviewed one adult alone; and second, group participation accelerated the pace of the interview. At times it took on the character of a quiz program, with one vying against another to give the clearest and most correct identification of the pictures. The result was to supply us with precise encyclopedic information; at the same time, it cut down on more submerged and more freely associated material, which we found later we could obtain from confidential photo-interviews with one informant. The photographs proved to be a useful tool for guiding group discussion when such a study might be necessary, but we realized that this group participation weighed heavily against the strict comparability of our results.

Of course, our project was more in the nature of an exploration than a conclusive test. The qualities we found in photo-interviewing do not necessarily make it a better method than the other, but they do show that photographs can influence the color and structure of an interview. In our limited experience, the photograph as an interview aid seems well

adapted to the rapid gathering of precise information on method and on identification of geography and personality. A second potentially significant application lies in the area of psychological response where the graphic image can stimulate expression of values or release submerged reactions. This element seems to be unpredictable, and it would require more refined testing before its true value could be appraised.

The method of photo-interviewing described was only the first we tried; it merely touched on possibilities. We feel that photographs used at a different pace or with other subject matter might gather broader and freer expression. Photo-interviews at other points of the Stirling County Study indicated that photos were capable of reaching deeper centers of reaction, triggering spontaneous revelations of a highly-charged emotional nature. The number of photographs, the speed with which they are presented, the size of enlargement, the quantity of detail, the familiarity and intimacy of the subject, and even the photographic quality, are variables which can affect the quality of an interview. We look upon the use of photographs as an interview aid, rather than as an infallible technique. Pictures may be vitally useful at one point of an interview, and impeding at another. To use graphics skillfully, we feel they should not be pressed upon the informant but should be used judiciously to control the drift of thought, to stimulate memory, or to recover some precise fact.

One of the foremost services of photographs as a research aid was their function as a language bridge. The graphic image can assist an informant who lacks fluency of words to make clear statements about

complex processes and situations. It was also demonstrated that by means of photographs a field worker can rapidly appraise and explore a technology with which he has had no experience, for the photographic record can supply detail that could otherwise be obtained only by lengthy first-hand experience. For example, photographs of commercial fishing could supply the imagery to enable a fisherman to explain his processes by following them directly through a series of pictures, without the impasse of technical reference.

A second element observed was a more subtle function of graphic imagery. This was its compelling effect upon the informant, its ability to prod latent memory, to stimulate and release emotional statements about the informant's life. Rural dwellers and small town folk may be especially responsive to this kind of stimulus--people such as the farmer who has lived his life in one valley, plowed the same fields, walked the same road, and met the same people year after year, so that time is marked for him only by disasters and breath-taking circumstances. Such a person might well find difficulty in remembering smaller events or the sequence of history; one interview may appear to tap all he knows and a second interview may be a frustration and an embarrassment. Or the reverse may be true. Similar environmental circumstances may have left the informant so starved for verbal expression that it is difficult to get him to stop talking, difficult to deflect his winding course through events that are of no concern to the investigation. Often, the informant won't be fazed by a question intended to steer the conversation; he will give a brief answer and then return to his whirlpool of recitation. In this situation also, we found that a photograph commands interest, deflects digression, and helps the interview to proceed on its meaningful way.

We feel that the stimulation of a photograph stems from its very nature. A photograph is an abstraction. No matter how familiar the object or situation portrayed may be, a photograph is a restatement of reality; it presents life around us in new, objective, and arresting dimensions, and can stimulate the informant to discuss the world about him as if observing it for the first time. Erskine Caldwell, in Journeyman, sums up the character of this experience. Country folk are gathered against a barn wall, eagerly waiting their turn to look through a crack in the boards. However, their excitement is not at looking in at some forbidden thing, but rather at looking out at everyday life. The crack in the boards creates for the viewers a new arrangement of reality, an abstraction of the part from the whole, so that the most familiar landmark is viewed with a sense of discovery.

NOTES

[1]I would like to express my gratitude to the Wenner-Gren Foundation and the Carnegie Corporation of New York for the grants that made this photographic study possible; to Lauriston Sharp, Chairman of Cornell's Department of Sociology and Anthropology, under which the projects were carried out; to Dr. Leighton, Director of the Stirling County Study and the Fruitland Project, for guidance and close collaboration; and to the field staffs of both projects for their cooperation, interest, and stimulation. I am particularly indebted to William A. Magill and Marc-Adélard Tremblay for their work as interviewers in the tests described in this paper, and to Robert N. Rapoport, Field Director of the Stirling County Study, for his advice and support.

[2]See Tremblay et al (1954). This aspect is not reported in the present paper.

[3]Bateson and Mead's work in Bali (1942) is an obvious exception. An example of the exhaustive use of photographs by a psychologist is Gesell's monumental work (1934), which reproduces 3200 action stills from movies, so arranged that selected children can be followed individually or compared with their peers as they go through a number of specific normal and test routines.

[4]Direct quotations are from the field notes of Magill and Tremblay.

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ANALYSIS OF THE PSYCHIATRIC EVALUATIONS FROM THE FOCUS AREAS *

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This paper reports on the work done on Focus Area (FA) records in California during the spring and early summer of 1958, including some of the apparent findings--"apparent" since no attempt was made to introduce corrections for age, sex, etc.

1. Methods

After the evaluations were finished and punched and the definitions of terms* had been completed, attention was turned to what the FA's showed. First the proper numbers were identified and sorted into communities (6).

Then distributions were made by community for the various ratings and impairments.* The Balance of the County (BC) records plus duplicates were totalled by machine for comparison with FA numbers. See Table 3.

Next a distribution of Symptom Pattern (SP) main categories was worked out by community and for the BC, divided further according to rating and impairment. See Table 2.

Following this, individuals were selected who were A+ and all their SP's were charted. They were separated into communities, and charts were made both of main SP's and of detailed SP's. In the latter case the degree of confidence* in the SP was indicated. See Tables 5, 6 and 6a.

The same steps were taken for individuals from Bristol and Portsmouth as has been described in the preceding paragraphs. See Tables 3 and 4.

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* For Method of Evaluation and terminology see Fourth Annual Report, pp. 151-175.

Finally, A- and B+ individuals were charted for SP's also. See Tables 7 and 8.

All these tables were examined and considered, and an attempt was made to construct metroglyphs from them. At this point, Edgar Anderson, the proponent of the metroglyph, arrived and worked with us to show the most relationships possible with the glyphs. While it is not at all sure that the glyphs contributed anything entirely new, they certainly served the purpose of highlighting some relationships which were only dimly perceived before, and to make these relationships easier to grasp and, thus, to think about. They will be discussed in greater detail later.

A few special studies were also begun, though most of them need further work for completion: mental deficients, sociopaths, personality disorders, sex differences, monosymptomatics. A plan was made to sort out a sample of "pure Acadian" and "pure English" for contrast, but aside from determining the criteria for selection this has not been completed as yet.

The rest of the paper will be devoted to discussion of what the tables and glyphs showed.

2. Findings

From the point of view of ratings and impairment (Table 1), it is clear that there is a big contrast between the Depressed Areas (DA's) and every other group. They have the most A+'s, the most B+'s, the most A's and B's, the least C's and by far the least D's. The other three groups (Lavallee, Fairhaven and BC)* are much more alike, though there is some tendency for BC to be intermediate between Lavallee and Fairhaven, with Lavallee on the "healthier" side and Fairhaven on the "sicker."

* BC left out of Table 1 here for lack of space. See Table 3 for BC figures.

RATINGS AND IMPAIRMENT BY COMMUNITY - TABLE 1

N=	Laval-lee 33	Fairhvn. 49	Camb. DA's 86	Monkey-town 25	NW Jones-ville 23	The Bog 23	Loomer-vale 15
Rating & Impairment	%	%	%	%	%	%	%
A	18	36	50	60	48	48	40
A+	15	20	40	52	35	39	27
A-	3	16	10	8	13	9	13
B	27	20	29	20	31	34	33
B+	12	8	19	4	22	30	20
B-	15	12	10	16	9	4	13
A+ and B+	27	29	59	56	57	70	47
A+, B+ and A-	30	45	69	64	70	79	60
All A's and B's	45	57	79	80	79	83	73
C	31	27	16	12	13	17	13
A, B, & C	76	84	93	92	92	100	86
D	24	16	4 (2 NA)	(8 NA)	9	-	13

Fairhaven, in fact, shows twice as large a percentage of A's as does Lavallee, and is comparatively deficient in B's, C's and D's. This difference is due mostly to the A- group (3% for Lavallee to 16% for Fairhaven), which one might surmise to be unimpaired neurotics in large part.

Of the DA groups, Loomervale shows considerably fewer A's and more D's than the other three. It tends more towards county norms than the other three groups, though it has a much higher total per cent of A's and B's. Monkeytown and The Bog share honors as "sickest," while Northwest Jonesville stands between them and Loomervale. Without Loomervale to temper the picture, the other three would appear as much "sicker" if they alone represented the DA's. A third of Loomervale's A's are A- and more than half of its B's are B-. In the case of Monkeytown, The Bog and Northwest Jonesville the proportion of A- to A is 1/8, 1/5, and 1/4, respectively; while B-/B is 4/5, 1/9, and 1/3 respectively. It must be borne in mind, however, that the small total number of respondents in Loomervale may skew the figures for that group.

Symptom Patterns

Turning now to Table 2 which gives percentages of people who show various main categories of symptoms, by communities, a few major points are at once apparent:

1. Brain Syndrome (BS) and Psychosis (Ps) are rare conditions everywhere and do not seem related to type of community. With such small numbers one would hardly expect to find detectable variation with social settings, in any case.

2. Psychophysiologic (Pp) and Psychoneurotic (Pn) symptoms are extremely common--commonest in the DA's, but only slightly less so in the BC and in Lavallee and Fairhaven.

DISTRIBUTION OF SYMPTOM PATTERNS - TABLE 2
(% based in all cases on total N for community)

	Lav.	Frhvn.	DA's	BC plus duplicates	Monkey-town	NW Jones-ville	The Bog	Loomer-vale
Total N:	33	49	86	632	25	23	23	15
All ratings & imp.	%	%	%	%	%	%	%	%
Brain Synd.	0	4	2	3	0	0	4	7
Ment. Def.	0	0	33	5	24	26	52	27
Psychosis	0	0	1	2	0	4	0	0
Psychophysiol.	76	74	61	74	84	70	87	74
Psychoneurotic	45	55	70	52	80	70	69	53
Pers. Dis.	12	10	8	5	8	4	9	13
Sociopathic	15	4	20	5	12	17	35	13
N=	9	14	50	155	14	13	16	7
A+ and B+	%	%	%	%	%	%	%	%
BS	0	2	2	0.3	0	0	4	7
MD	0	0	26	3	16	22	48	13
Ps	0	0	1	2	0	4	0	0
Pp	24	20	51	23	56	48	61	33
Pn	18	18	45	19	52	43	52	27
PD	6	2	8	2	8	4	9	13
Socio	9	0	15	3	12	9	26	13
N=	15	28	68	343	20	18	19	11
All A and B	%	%	%	%	%	%	%	%
BS	0	4	2	2	0	0	4	7
MD	0	0	27	4	16	26	48	13
Ps	0	0	1	2	0	4	0	0
Pp	42	45	69	49	76	65	74	53
Pn	33	45	64	42	76	65	61	47
PD	9	8	8	4	8	4	9	13
Socio	12	4	19	4	12	13	35	13
N=	10	13	12	173	3	3	4	2
All C	%	%	%	%	%	%	%	%
BS	0	0	0	0.3	0	0	0	0
MD	0	0	6	1	8	0	4	13
Ps	0	0	0	0.3	0	0	0	0
Pp	27	22	9	20	8	4	13	13
Pn	12	10	6	9	4	4	8	7
PD	3	2	0	2	0	0	0	0
Socio	3	0	1	1	0	4	0	0
N=	8	8	4	112	0	2	0	2
All D	%	%	%	%	%	%	%	%
Pp (only SP)	6	6	3	5	0	9	0	7

3. A big contrast is seen in the percentages of Mental Deficiency (MD)--none in Lavallee and Fairhaven, 5% in the BC and 33% in the DA's. The Bog seems to contribute more than its share to the DA figure.

4. As to Sociopathic (Socio) symptoms, the division is between the BC and Fairhaven on the one hand and the DA's and Lavallee on the other. One might suppose this was a Protestant-Catholic division, but the BC contains a good proportion of Acadians as well as other Catholics. As with Mental Deficiency, The Bog seems to contribute more than its share to the DA percentage for Sociopathic.

The second part of the Table shows percentages of people with various SP's who are rated A or B impaired. Here one can see that the DA's not only surpass other localities in percentages of people with symptoms, but that many of these people are impaired by their symptoms--59% of the DA's as compared with approximately 25% of other areas. The types for which they are outstanding--Mental Deficiency and Sociopathic--are chiefly in this A and B plus group. With Lavallee, Fairhaven and the BC, approximately the same proportion fall into the C rating as into the A+B+ rating, while the DA's show very few indeed to be rated C.

Bristol and Portsmouth

These two communities are taken to represent "urban" or "complex" environments, as near as we can approach this in a rural area, in any case. The shortcomings of the figures discussed here are chiefly that Bristol is represented by only 41 cases, redone with one of the medium-information runs of evaluations. When results of the additional 100 Bristol cases are added, the figures may well be somewhat different from those given here.

Table 3 shows that the two towns resemble the BC closely in total A's and in A+ and B+. Bristol shows a relative deficiency of B's and D's and an excess of C's. Generally speaking, Portsmouth resembles Lavallee and Bristol resembles Fairhaven as compared to the BC and to each other. Since there is a considerable Acadian population in Portsmouth and a much smaller proportion in Bristol, this is perhaps what one should expect. It will be interesting to see what the additional evaluations do to the Bristol figures.

Table 4 shows that here, too, Portsmouth tends towards the Lavallee picture and Bristol towards the Fairhaven picture in the per cent with Pp, Pn and Sociopathic. If they were treated as a single "complex" unit and compared to the county, they would show a slightly lower per cent of all symptom categories except Sociopathic, and this would be only very slightly higher than the county average.

Individual Symptom Patterns

Table 5 shows the first attempt to study individual SP's, here listed by main categories. In these A+ individuals, a glance suffices to show that Pp and Pn are practically universal, other categories much more scattered. It is obvious that little can be learned from such a table.

In Tables 6 and 6a, 7 and 8 we tabulated the detail Symptom Patterns for A+, B+ and A- individuals to see if anything could be learned of over-all patterning, and whether this varied from community to community. Although it is not too easy to pick out the figures from the zeros, a careful perusal shows that the different communities seem to have some tendency for 2's and 3's in columns that are mostly zeros in other communities.

RATINGS AND IMPAIRMENT, PORTSMOUTH AND BRISTOL - TABLE 3

	Portsmouth Ecol. Area	Bristol Ecol. Area	BC
N=	114	41	632
Rating and Impairment	%	%	%
A	24	28	29
A+	15	24	16
A-	9	4	13
B	29	13	24
B+	11	2	8
B-	18	11	16
A+ and B+	26	26	25
A+, B+ and A-	35	30	38
All A's and B's	53	41	54
C	24	44	28
A, B and C	77	85	82
D	23	15	18

DISTRIBUTION OF SYMPTOM PATTERNS - PORTSMOUTH AND BRISTOL - TABLE 4

		Portsmouth	Bristol
Total N=		114	41
All ratings		%	%
BS		0	0
MD		2	2
Ps		1	4
Pp		70	59
Pn		46	52
PD		4	9
Socio		11	2
A+ and B+	N=	30	12
		%	%
BS		0	0
MD		0	2
Ps		1	4
Pp		24	20
Pn		19	22
Pp		3	2
Socio		7	2
All A and B	N=	61	19
		%	%
BS		0	0
MD		1	2
Ps		1	4
Pp		48	30
Pn		39	37
PD		4	2
Socio		10	2
All C	N=	27	20
		%	%
BS		0	0
MD		1	0
Ps		0	0
Pp		16	24
Pn		7	15
PD		0	7
Socio		1	0
All D	N=	26	7
		%	%
Pp (only SP)		6	4

Of the Pp symptoms, Gastro-intestinal (GI) seems nearly (but not quite) as universal as Pp itself. On the other hand, look at the Musculo-skeletal (MS) column: Only 1 in Lavallee, almost all in Fairhaven, a fairly heavy scatter through the DA's. One can also see that there is more tendency for A+ individuals to have marks in several columns (Pp, that is) than for A- or B+ individuals. There is a hint that there are community modes for Pn symptoms--Anxiety (Ax) for Lavallee, Other (O) for Fairhaven and DA's. The difficulty of grasping with the eye the significance of these tables made us all the more eager to see what could be learned by using some more graphic means of showing the same facts without losing the individuality of the patterning. Metroglyphs seemed like a convenient tool.

KEY TO TABLES 5, 6, 6a, 7 AND 8

10 -- Glyph number
F52 -- Sex, age
) Tables 6, 6a, 7 and 8

073 -- FA serial number

BS -- Brain Syndrome
MD -- Mental Deficiency
Ps -- Psychosis
Pp -- Psychophysiological

Sk -- Skin
MS -- Musculo-skeletal
Rs -- Respiratory
CV -- Cardiovascular
GI -- Gastro-intestinal
GU -- Genitourinary
Hd -- Headaches Often
En -- Endocrine
) Tables 6, 6a, 7 and 8

Pn -- Psychoneurotic

Ax -- Anxiety
Dp -- Depressive
Hy -- Hypochondriacal
O -- Other
) Tables 6, 6a, 7 and 8

PD -- Personality Disorder
Socio -- Sociopathic

Alc -- Alcohol Addiction
Dys -- Dyssocial
Ant -- Antisocial
) Tables 6, 6a, 7 and 8

0 -- No symptoms
1 -- Low confidence symptoms
2 -- Medium confidence symptoms
3 -- High confidence symptoms

SYMPTOM PATTERNS IN A+ INDIVIDUALS BY COMMUNITIES - TABLE 5

(0 = not present; 1 = low confidence; 2 = medium confidence; 3 = high confidence)

	BS	MD	Ps	Pp	Pn	PD	Socio.
Lavallee							
FA 044	0	0	0	3	2	0	0
047	0	0	0	3	3	0	0
208	0	0	0	3	3	3	0
229	0	0	0	3	3	0	2
322	0	0	0	3	3	0	0
Fairhaven							
FA 011	3	0	0	2	3	0	0
040	0	0	0	3	3	0	0
048	0	0	0	3	3	0	0
076	0	0	0	3	2	0	0
098	0	0	0	3	0	2	0
113	0	0	0	3	3	0	0
157	0	0	0	3	2	0	0
178	0	0	0	3	3	0	0
295	0	0	0	3	3	0	0
368	0	0	0	3	3	0	0
Monkeytown							
FA 003	0	2	0	3	2	1	0
009	0	1	0	3	2	0	0
017	0	0	0	3	2	0	0
021	0	0	0	3	2	0	3
043	0	0	0	3	2	0	3
054	0	0	0	3	3	0	0
075	0	2	0	3	3	0	0
105	2	0	0	3	3	0	0
158	0	0	0	3	3	0	0
167	0	0	0	3	3	0	0
211	0	0	0	3	2	0	0
283	0	2	0	3	3	0	0
339	0	0	0	3	3	0	0
NW Jonesville							
FA 014	0	0	2	3	2	0	0
112	0	1	0	3	3	0	3
120							
200	0	0	0	3	3	0	0
205	0	0	0	3	3	0	0
285	0	0	0	3	3	0	0
297	0	0	0	3	3	0	0
336	0	0	0	3	3	0	0

TABLE 5 - cont.

	BS	MD	Ps	Pp	Pn	PD	Socio.
The Bog							
FA 022	0	0	0	3	3	0	0
045	0	1	0	3	2	0	0
093	0	3	0	3	0	0	3
129	0	0	0	3	2	0	2
164	0	1	0	2	2	0	3
198	0	2	0	3	3	0	0
234	1	0	0	3	3	2	3
265	0	1	0	3	3	3	0
300	0	2	0	3	3	0	0
Loomervale							
FA 024	1	2	0	0	0	0	0
139	0	0	0	3	3	0	3
195	0	0	0	3	3	3	0
355	0	0	0	3	3	0	0

DISTRIBUTION OF DETAIL SYMPTOM PATTERNS IN A+ INDIVIDUALS BY COMMUNITIES - TABLE 6

(0 = not present; 1 = low confidence; 2 = medium confidence; 3 = high confidence)

	BS	MD	Ps	Sk	MS	Rs	CV	GI	GU	Hd	En	Ax	Dp	Hy	O	PD	Alc	Dys	Ant
							Pp						Pn				Socio		
Lavallee																			
1 F32 044	0	0	0	0	0	0	0	3	0	3	0	0	0	0	2	0	0	0	0
2 M61 047	0	0	0	0	0	3	1	3	0	0	0	0	0	3	0	0	0	0	0
3 F46 208	0	0	0	0	0	0	0	3	0	0	0	3	0	0	0	3	0	0	0
4 M32 229	0	0	0	0	0	0	1	3	0	0	0	3	3	0	0	0	3	0	0
5 F58 322	0	0	0	0	3	0	1	3	0	0	0	3	0	0	0	0	0	0	0
Fairhaven																			
1 M59 011	3	0	0	0	2	0	0	0	0	0	0	0	3	0	0	0	0	0	0
2 F60 040	0	0	0	0	2	0	2	3	0	0	0	3	0	0	0	0	0	0	0
3 F74 048	0	0	0	0	2	3	1	3	0	0	0	0	0	0	3	0	0	0	0
4 F61 076	0	0	0	0	3	0	3	3	0	0	0	0	0	0	2	0	0	0	0
5 M36 098	0	0	0	0	3	0	0	3	0	0	0	0	0	0	0	2	0	0	0
6 F65 113	0	0	0	0	3	0	3	0	0	0	0	0	0	0	3	0	0	0	0
7 M57 157	0	0	0	0	3	0	3	3	0	0	0	0	0	0	2	0	0	0	0
8 F62 178	0	0	0	0	0	0	3	3	0	0	0	0	0	0	3	0	0	0	0
9 F37 295	0	0	0	3	0	0	1	3	0	0	0	3	3	0	0	0	0	0	0
10 F76 368	0	0	0	0	3	0	3	0	0	0	3	0	3	0	0	0	0	0	0
Monkeytown																			
1 M72 003	0	2	0	0	3	0	3	3	1	0	0	0	0	0	2	1	0	0	0
2 F55 009	0	1	0	0	0	0	2	3	0	0	0	2	2	0	0	0	0	0	0
3 M45 017	0	0	0	0	3	0	2	3	0	0	0	0	2	0	0	0	0	0	0
4 M60 021	0	0	0	0	2	0	3	3	0	0	0	0	0	0	2	0	3	0	0
5 M67 043	0	0	0	0	0	0	3	3	0	0	0	0	0	0	2	0	3	3	0
6 M55 054	0	0	0	0	3	0	1	3	3	3	0	0	3	3	0	0	0	0	0
7 F38 075	0	2	0	0	0	0	0	3	1	0	0	0	0	0	3	0	0	0	0
8 F41 105	2	0	0	0	3	0	1	3	0	3	0	3	3	3	0	0	0	0	0
9 F20 158	0	0	0	0	3	0	0	3	0	0	0	0	3	0	3	0	0	0	0
10 M42 167	0	0	0	0	3	0	2	2	0	0	0	0	3	0	0	0	0	0	0
11 M55 211	0	0	0	0	3	0	3	0	0	3	0	0	2	0	0	0	0	0	0
12 F55 283	0	2	0	0	3	0	0	3	0	3	0	0	3	0	0	0	0	0	0
13 M56 339	0	0	0	0	3	3	2	3	0	0	0	0	3	0	0	0	0	0	0
NW Jonesville																			
1 F38 014	0	0	2	0	2	0	0	3	0	0	0	0	2	0	2	0	0	0	0
2 M32 112	0	1	0	0	3	3	0	3	1	0	0	0	0	0	3	0	3	0	3
13 F32 120	0	2	0	0	0	0	0	0	0	0	0	0	0	0	0	3	0	2	0
3 F59 200	0	0	0	0	0	0	0	0	0	0	0	3	0	0	3	0	0	0	0
4 M75 205	0	0	0	3	0	0	2	3	1	0	0	3	3	3	0	0	0	0	0
5 F57 285	0	0	0	0	3	0	1	3	0	0	1	3	0	0	3	0	0	0	0
6 F61 297	0	0	0	0	0	0	3	0	0	0	0	0	0	0	3	0	0	0	0
7 M66 336	0	0	0	0	3	0	2	0	1	0	0	0	3	0	3	0	0	0	0

TABLE 6a

			Pp								Pn				Socio			
	BS	MD	Ps	Sk	MS	Rs	CV	GI	GU	Hd	En	Ax	Dp	Hy	O	PD	Alc	Dys	Ant
The Bog																			
1 F36 022	0	0	0	0	2	0	0	3	0	0	3	0	0	0	3	0	0	0	0
2 F44 045	0	1	0	0	0	0	1	3	0	0	0	0	0	0	2	0	0	0	0
3 F68 093	0	3	0	0	0	0	3	3	0	0	0	0	0	0	0	0	0	2	0
4 M50 129	0	0	0	0	3	0	0	3	0	0	0	0	0	0	2	0	3	0	3
5 M45 164	0	1	0	0	0	0	2	0	0	0	0	0	2	0	0	0	3	0	0
6 F28 198	0	2	0	0	3	0	1	3	0	0	0	3	3	0	0	0	0	0	0
7 M46 234	1	0	0	0	3	0	3	3	0	3	0	3	3	0	0	2	3	0	0
8 M24 265	0	1	0	3	0	0	3	3	0	3	0	3	3	0	0	3	0	0	0
9 M38 300	0	2	0	0	3	3	1	0	0	0	0	0	0	0	3	0	0	0	0
17 M55 039	0	0	0	0	3	0	0	3	0	0	0	0	0	0	2	0	0	3	0
Loomervale																			
1 M55 024	1	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
2 M33 139	0	0	0	0	3	0	0	3	0	3	0	0	0	3	0	0	3	3	0
3 F38 195	0	0	0	0	3	0	0	3	3	0	0	0	3	0	0	3	0	0	0
4 F66 355	0	0	0	0	3	0	3	3	3	3	3	3	0	0	3	0	0	0	0

DISTRIBUTION OF DETAIL SYMPTOM PATTERNS IN B+ INDIVIDUALS BY COMMUNITY - TABLE 7

	Pp										Pn					Socio			
	BS	MD	Ps	Sk	MS	Rs	CV	GI	GU	Hd	En	Ax	Dp	Hy	O	PD	Alc & Drug	Dys	Ant
Lavallee																			
6 F53 028	0	0	0	0	0	0	3	0	3	0	0	0	0	0	0	0	0	0	0
7 M60 221	0	0	0	0	0	3	3	0	0	0	0	0	0	0	0	0	3	0	0
8 F60 228	0	0	0	0	0	0	3	0	0	0	3	0	0	0	0	0	0	0	0
9 M32 278	0	0	0	0	0	0	0	0	0	0	0	0	0	0	2	2	2	0	0
Fairhaven																			
11 M55 060	0	0	0	0	3	0	0	3	0	0	0	0	0	0	2	0	0	0	0
12 F29 078	0	0	0	0	0	0	0	3	3	0	0	2	0	0	0	2	0	0	0
13 M73 251	1	0	0	0	0	0	2	0	0	0	0	0	0	0	2	0	0	0	0
14 M77 275	0	0	0	0	0	0	2	0	0	0	0	0	0	0	0	0	0	0	0
Monkeytown																			
14 M42 305	0	0	0	0	0	0	0	3	0	0	0	0	0	0	0	2	3	3	0
NW Jonesville																			
8 M47 059	0	0	0	0	3	3	0	3	0	0	0	0	0	0	2	0	0	0	0
9 M22 115	0	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
10 M52 151	0	2	0	0	3	0	1	0	0	0	0	0	0	0	0	0	0	0	0
11 M56 323	0	1	0	0	3	0	1	3	0	0	1	0	0	0	2	0	0	0	0
12 F59 347	0	0	0	0	3	0	3	3	0	0	0	0	0	0	2	0	0	0	0
The Bog																			
10 M29 064	0	2	0	0	3	0	0	0	0	0	0	0	0	0	0	0	0	0	0
11 F44 080	0	2	0	0	3	0	0	3	1	0	0	0	0	0	0	0	0	0	0
12 M21 125	0	2	0	0	0	0	1	3	0	0	0	0	0	0	0	0	0	0	0
13 F20 177	0	2	0	0	0	0	1	0	0	0	0	0	0	0	2	0	0	0	0
14 M33 238	0	2	0	0	0	0	0	0	0	0	0	0	0	0	2	0	0	0	0
15 M77 266	0	0	0	0	2	0	2	3	0	0	3	0	0	0	1	0	3	0	0
16 M82 331	0	0	0	0	0	0	3	0	0	0	0	0	0	0	1	0	0	1	0
Loomervale																			
5 F65 015	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	2	0
6 M67 062	1	0	0	0	3	0	2	3	0	0	0	0	0	0	0	0	0	0	0
7 M57 201	0	2	0	0	3	1	0	0	1	0	0	0	0	0	0	3	0	0	0

A- INDIVIDUALS BY COMMUNITIES - TABLE 8

			Pp								Pn				Socio			
	BS	MD	Ps	Sk	MS	Rs	CV	GI	GU	Hd	En	Ax	Dp	Hy	O	PD	Alc	Dys	Ant
Lavallee																			
10 F52 073	0	0	0	0	3	0	0	3	3	3	0	0	0	0	2	1	0	0	0
Fairhaven																			
15 M34 145	0	0	0	0	3	0	0	3	0	0	0	0	0	3	0	0	0	0	0
16 F42 166	0	0	0	0	0	0	0	0	0	0	0	0	0	0	2	0	0	0	0
17 F43 173	0	0	0	0	3	0	0	3	3	0	0	0	0	0	3	0	0	0	0
18 F45 209	0	0	0	0	0	0	0	0	0	0	0	0	0	0	3	0	0	0	0
19 M68 276	0	0	0	0	3	0	0	3	0	0	0	0	0	0	3	0	0	0	0
20 F54 329	0	0	0	0	0	0	0	0	0	3	3	0	0	0	3	0	0	0	0
21 F34 350	0	0	0	0	0	0	0	3	0	3	0	3	0	0	0	0	0	0	0
22 F61 359	0	0	0	0	3	0	3	0	0	0	0	0	0	0	3	0	0	0	0
Monkeytown																			
15 M31 072	0	0	0	0	0	0	3	3	0	0	3	0	0	0	2	0	0	0	0
16 F36 081	0	0	0	0	3	0	0	3	0	0	0	3	0	0	0	0	0	0	0
NW Jonesville																			
14 M38 110	0	0	0	0	3	0	0	0	0	0	0	0	0	0	2	0	0	0	0
15 F42 192	0	0	0	0	0	0	1	0	2	0	0	0	0	3	0	0	0	0	0
16 F34 352	0	0	0	0	3	0	1	0	0	0	0	0	0	0	3	0	0	0	0
The Bog																			
17 F50 269	0	0	0	0	3	0	0	3	0	0	0	0	3	0	0	0	0	3	0
Loomervale																			
8 M67 361	0	0	0	0	3	3	0	3	0	0	0	0	0	0	3	0	0	0	0
9 F41 207	0	0	0	0	0	0	0	3	0	0	3	0	0	0	3	0	0	0	0

Metroglyphs

After preliminary orientation discussions with Anderson, explaining the research, the rating methods, the difference between communities, etc., he took Tables 6 and 6a, 7 and 8 to study. His intention was to make the glyphs as multi-dimensional as possible in order to show the maximum in patterning.* However, in order for the glyphs to be readily appreciated by the eye, they must not be too complicated in themselves; as much as possible must be made into indices which can be used as the sides of a Cartesian diagram. The eye can grasp as many as 5 rays around the central circle, and shading can be utilized in addition.

Since Pn and Pp were by all odds the commonest SP's, they seemed the likliest subjects to make into indices, using the rays for less common SP's, and shading for sex. Initial work was all done on the A+ and B+ groups on the assumption that SP's would be maximum with these individuals and provide larger numbers to work with.

A frequency study convinced Anderson that of the Pp SP's, GI was at once the commonest and also a sort of dividing principle. As a trial he made a Pp index of: None; any 1, 2, or 3 Pp except GI; GI; GI plus 1 Pp; GI plus 2, 3, or 4 Pp. This seemed to provide an acceptable sort of scale, and was retained throughout the experiment.

Pn was more difficult to scale, because not only quantitative but also qualitative aspects had to be considered, and the latter were not so easy to arrange on a meaningful scale. The first one used was: None; Depressive; Hypochondriasis; Other; Anxiety; Any 2; Any 3. A later step was to move Other next above None. Finally the scale was revised to: None; Other; Anxiety or Depressive or Hypochondriasis; Any 2; Any 3.

* See "A Semigraphical Method for the Analysis of Complex Problems," Proceedings of the National Academy of Sciences, Vol. 43, No. 10, pp. 923-927, October, 1957.

This seemed to give more adequate recognition to the difference between Other and the 3 more specific SP's, though it is hard to be sure that this is justified as a measure of greater quantitative significance. The change also tended to make the diagrams easier to read since it made the two sides of equal length.

After a number of trials, the rays were reduced to 4: on the left for Mental Deficiency; at the top for Personality Disorder (PD); and on the right 2 for Sociopathic, 1 indicating Alcoholism (Alc) or Drug Addiction, and other Dysoccial (Dys) Reaction. Shading was used to indicate sex, black for men and white for women. Other characteristics were shown by limiting a diagram to one community or another, e.g., and to certain rating and impairment categories.

Many diagrams were made: each Focus Community, the two Organized Pooled, and the 4 Disorganized Pooled, all 6 Focus Communities Pooled; the Men, and Women; the Mental Deficients, the Sociopaths, the Alcoholics. Others we would like to try are: the A's, the B's, the C's; the Impaired, the Unimpaired; pure Acadian, pure English; Bristol and Portsmouth. No doubt others will come up in the course of the analysis. They are useful, as already remarked, not so much to produce altogether new ideas, as to emphasize ideas in the direction of relationships which are not so easily perceived from tables of figures. Some of the diagrams are hereby appended, with a few notes on what they show.

Look first at Figures I and II, which include all A+, A-, and B+ individuals from Lavallee and Fairhaven, respectively. As we saw in Table 1 there are more people of these ratings in the Fairhaven diagram than in the Lavallee one. It is clear also that there are more women

FIGURE I

Lavallee

A+ A- B+

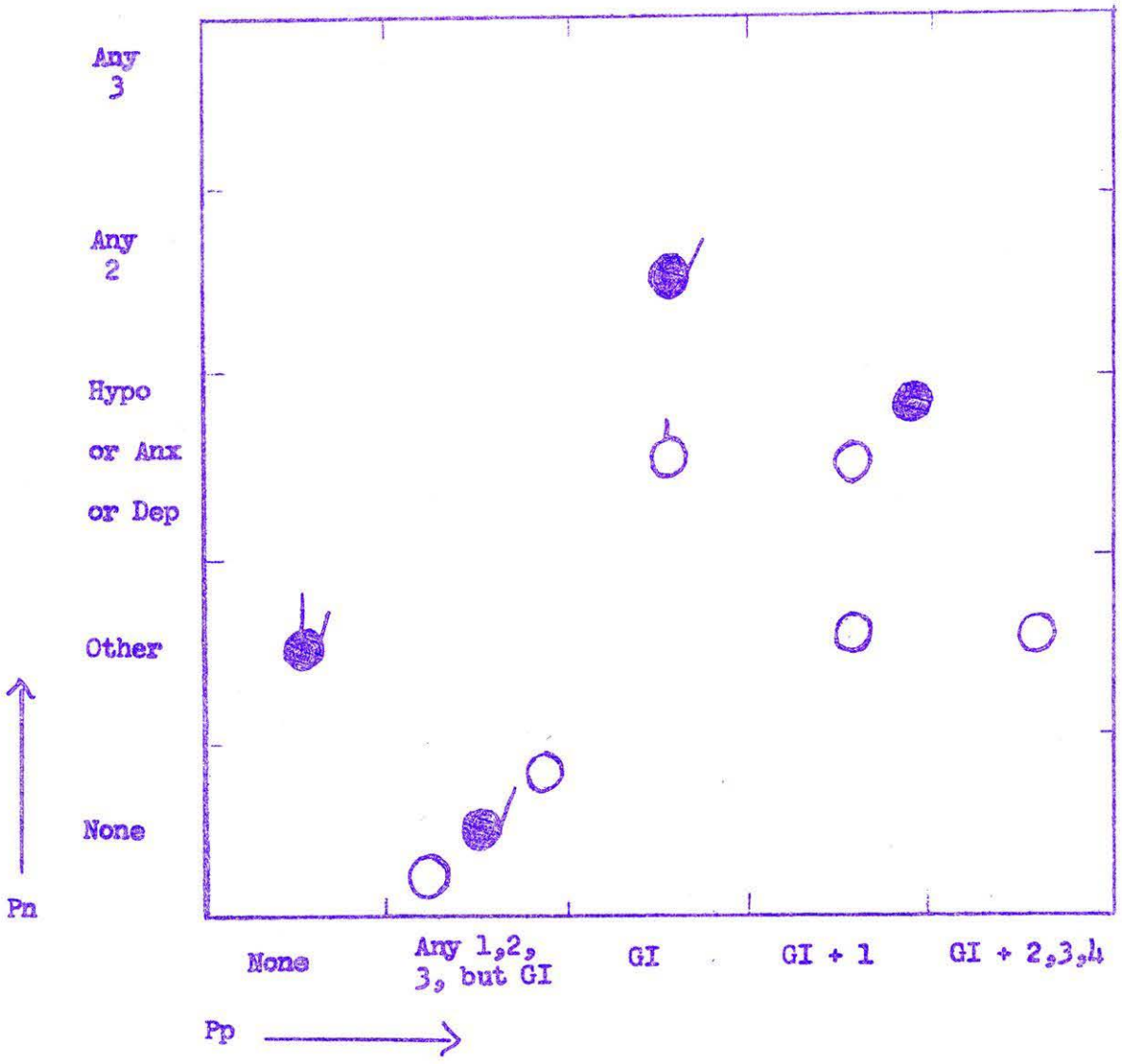
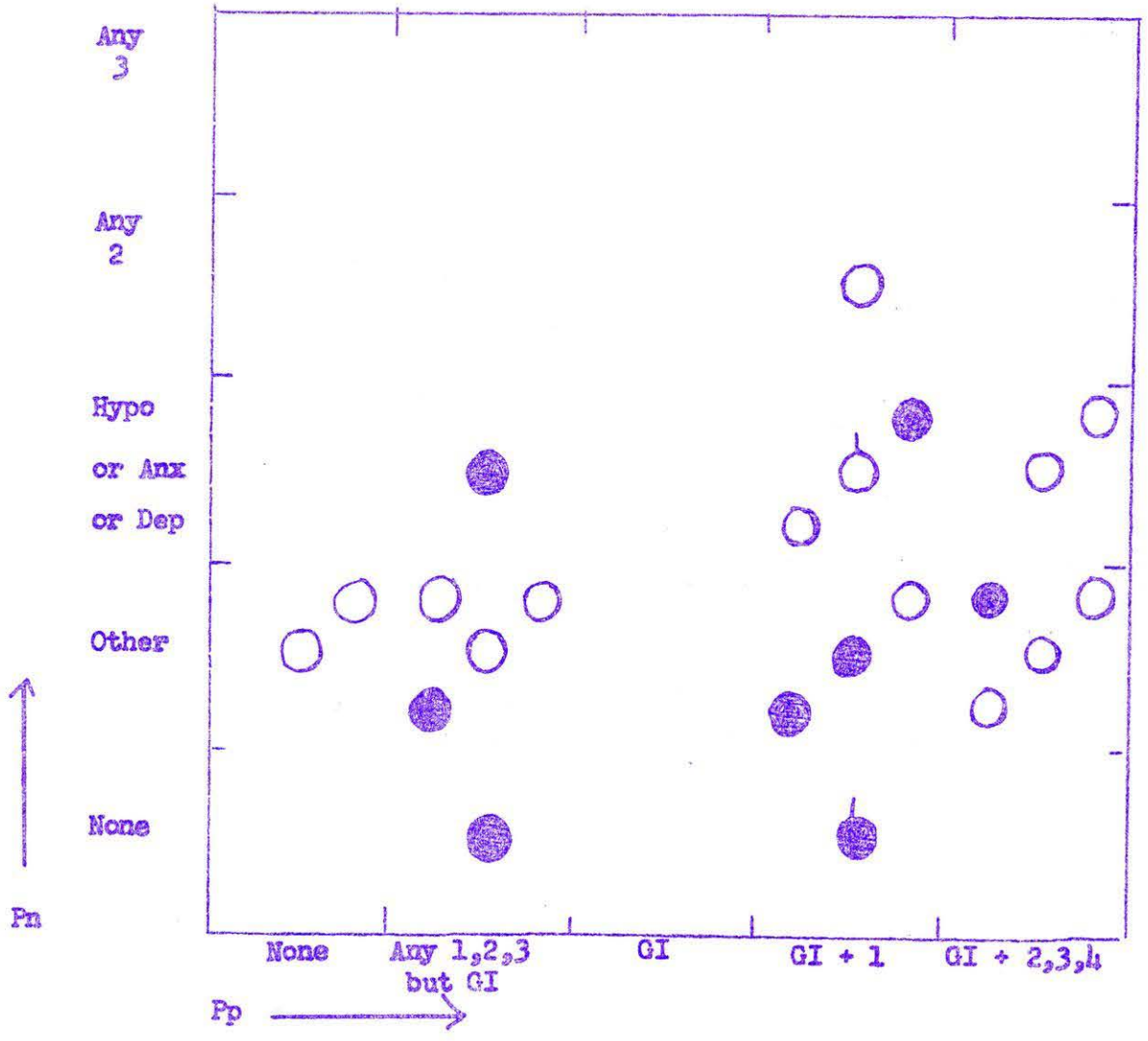


FIGURE II

Fairhaven

A+ A- B+



than men in both places--Fairhaven 14/8, Lavallee 6/4. Another characteristic quickly apparent is that there are few rays in either diagram--only 2 for Fairhaven's 22 and 5 for Lavallee's 10. Two of these are for Personality Disorder, in each, one man and one woman. In addition, Lavallee shows 3 rays for Alcoholism, all men, while Fairhaven shows none. All 4 corners of each diagram are empty: Lavallee shows 3 without Pn, 1 without Pp, the rest scattered; Fairhaven shows 2 without Pn, 2 without Pp, and a considerable concentration of glyphs in the Pn Other zone. The empty corners indicate the absence of extremes in the Pp-Pn axis--none of these AB individuals lacks both Pp and Pn, none has the one in large amount without the other, none has maximal amounts of both (the assumption being that for our purposes such symptoms are somewhat quantifiable). Combined into one diagram, Fig. IV results, which shows (all A) and B+ individuals from the total of 82 interviewed in these two communities.

Now compare Fig. IV with Fig. III, the latter representing in the same way (all A) and B+ individuals from the disorganized neighborhoods, of a total of 86 interviewed. Three points are at once evident: there are many more glyphs; more of them are black; there are many more (and more varied) rays. Moreover, all corners except the upper left (high Pn, no Pp) are utilized. The sex-reversal raises the question of the sex ratio in organized and disorganized areas; are there more females in Lavallee and Fairhaven and more males in the DA's? Or is it just that the females in the organized and the males in the DA's are more symptomatic?

Taking Fig. III by itself, now, the largest number of glyphs seems to lie between the top and bottom zones vertically and within the two right-hand zones horizontally. That is, this group have Pn Other, Pn

FIGURE III

Pooled Disorganized

A+ A- B+

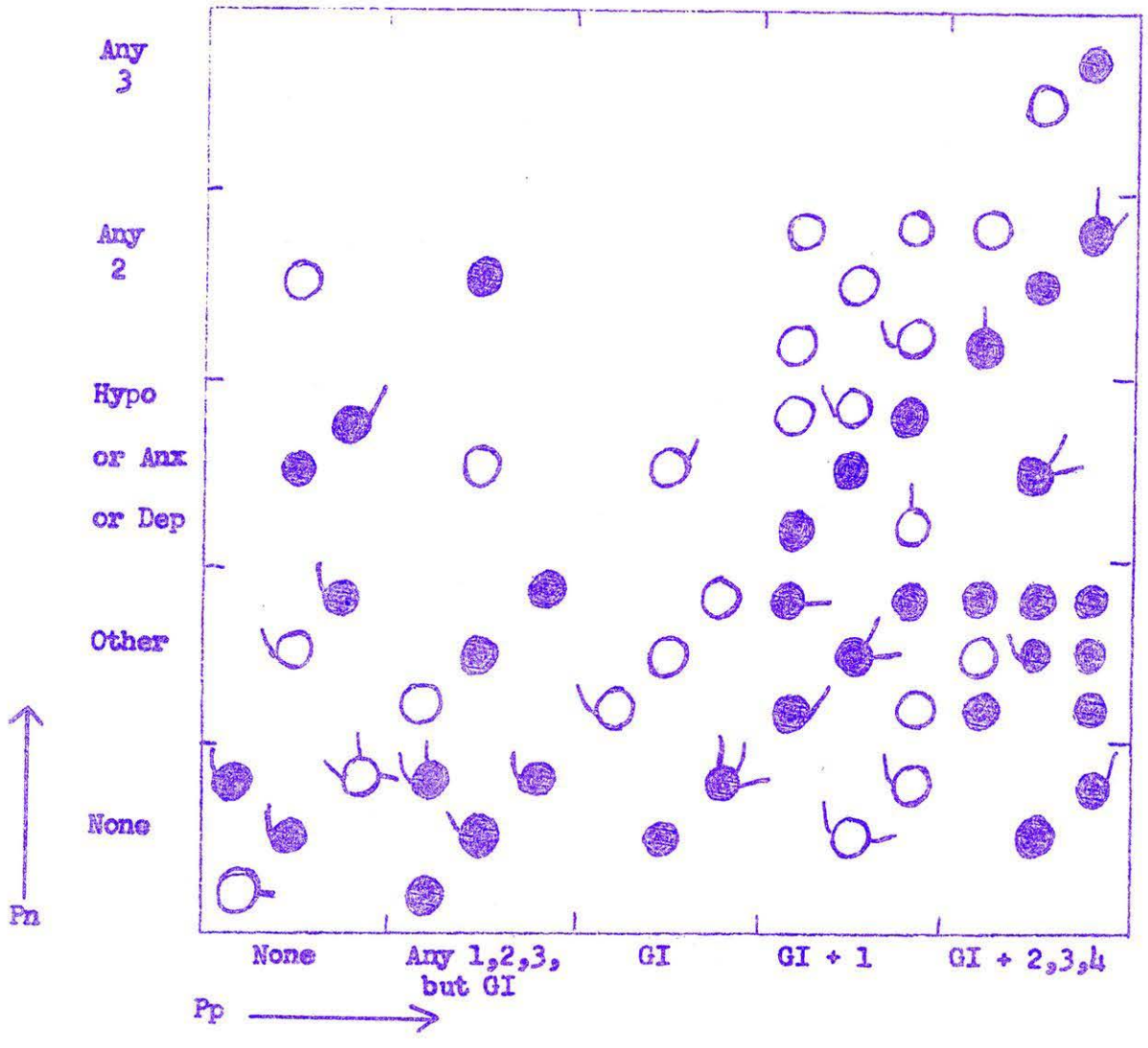
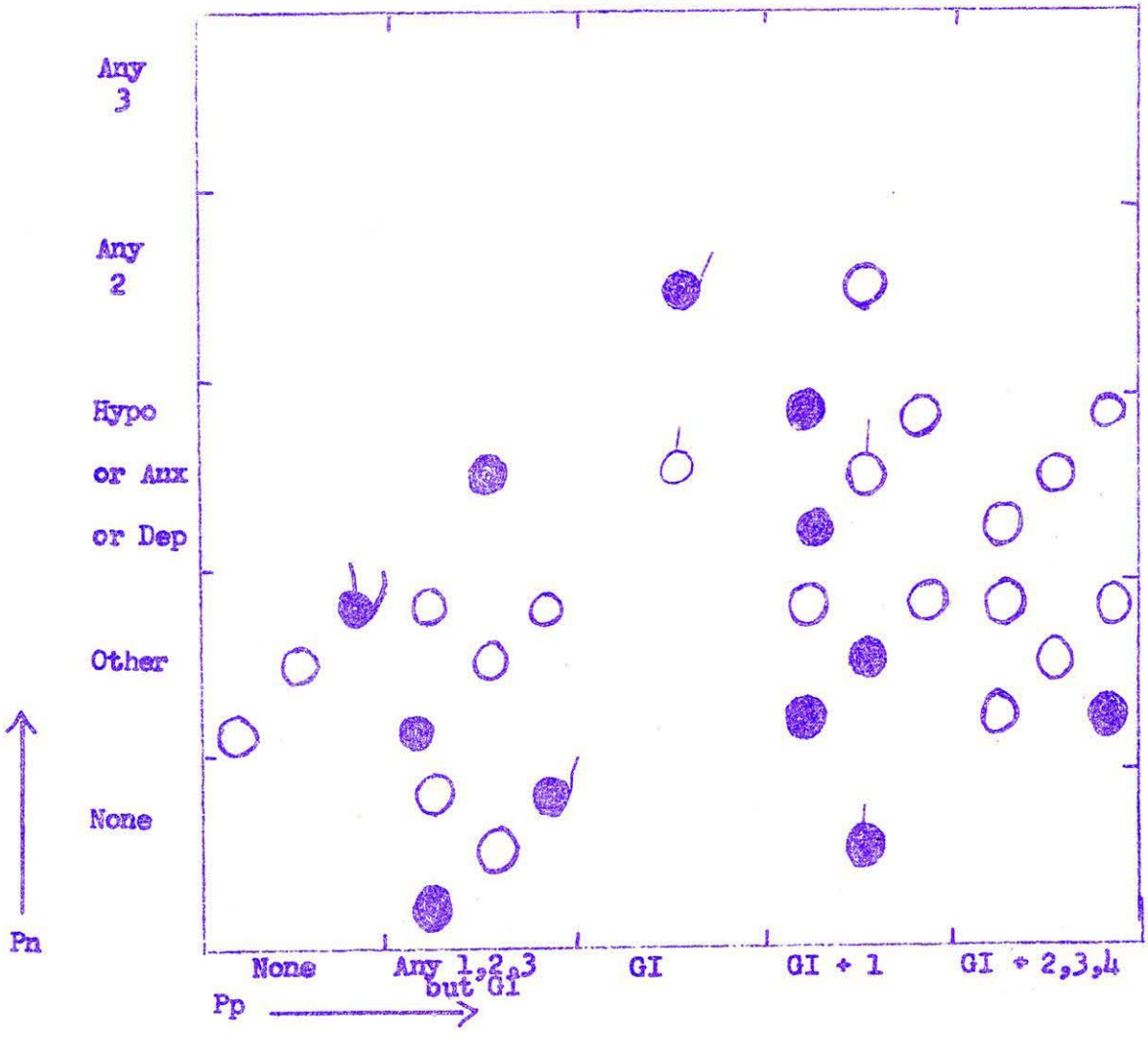


FIGURE IV

Pooled Organized

A+ A- B+



specific, or 2 Pn; and also GI plus 1 Pp, or GI plus 2, 3, 4 Pp. They also show a number of rays: 3 to the left for Mental Deficiency, 3 on top for Personality Disorder, and 7 to the right for Sociopathic. Eleven are women, 18 men.

Another interesting group on this diagram are those in the bottom zone who have no Pn. Of the 14 glyphs here, only 4 are women, only 3 show no rays (all men), 8 show Mental Deficiency, 3 show Personality Disorder, 5 show Sociopathic, either alone or combined with other rays (only 1 is Sociopathic alone).

Looking next at the extreme left zone, the no-Pp's, we find 9 glyphs, 4 women, 5 men. Of these 5 show Mental Deficiancy, 3 Sociopathic, 1 Personality Disorder, 2 no rays (i.e., pure Pn's). It is of interest to note that the lower left no-Pp-no-Pn corner contains only glyphs with rays--4 of them--as one would expect.

The above inspection of the scattergrams led on to a few others. Figures V and VI show the results of graphing the men and the women of the 6 focus communities separately. The men show somewhat more clumping and more in the no-Pn zone than the women. There are 46 men, 43 women. The men show more rays--26 altogether as compared to 14 for the women. Mental Deficient rays are on 7 women's glyphs, 8 men's: Personality Disorder, 3 women, 6 men; Alcoholism, 0 women, 9 men; Dyssocial, 4 women, 3 men. Only 2 women show multiple rays, 5 men.

Finally an effort was made to see if the lack of association of Sociopathic symptoms with Pn could be seen in the county-wide data. Mental Deficients were also examined for the whole county. In each case, every individual who was rated Sociopathic (or Mental Deficient) with high or medium confidence was included in the scattergram. Figures VII

FIGURE V

Men Only - Six Focus Communities

A+ A- B+

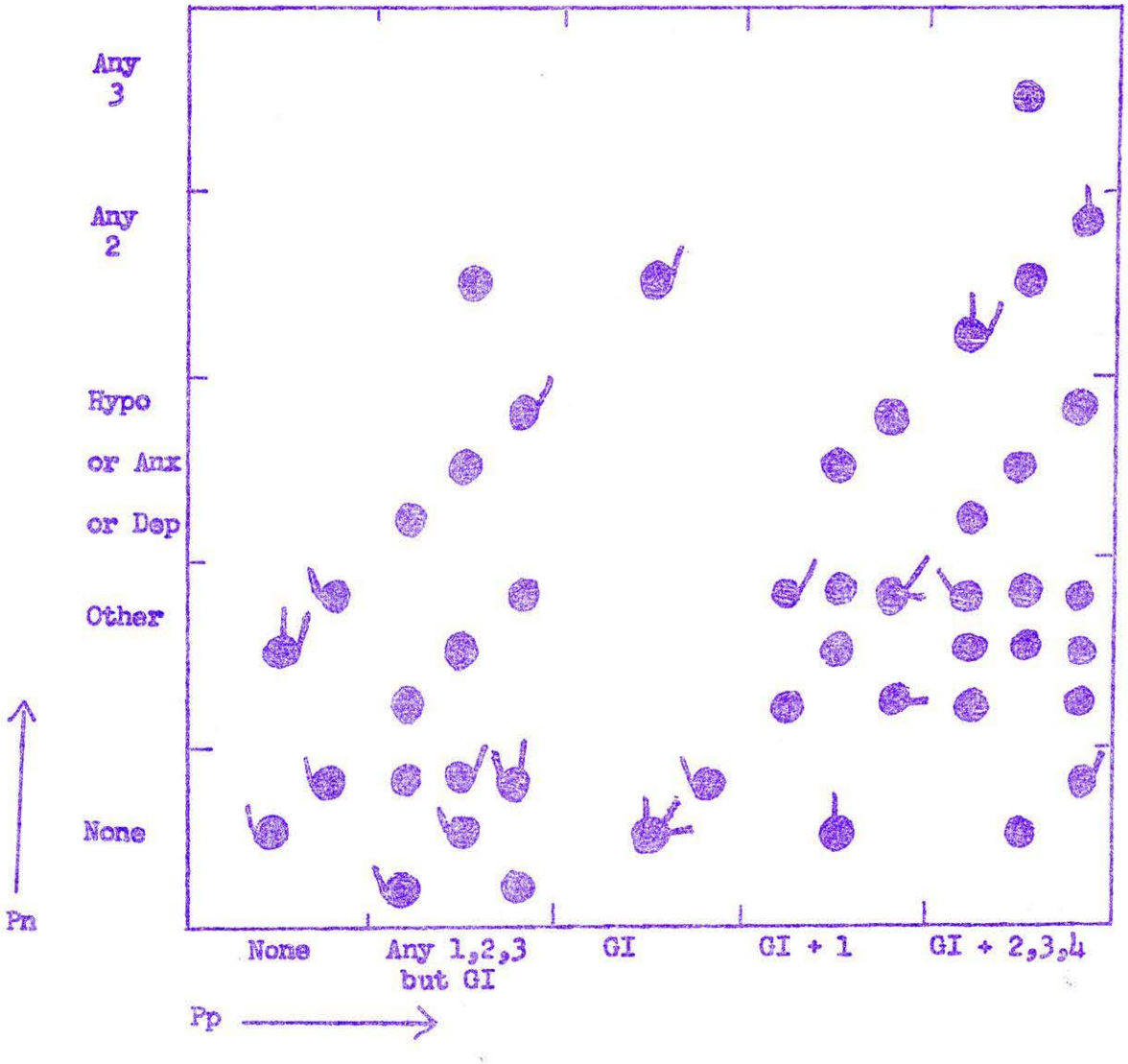
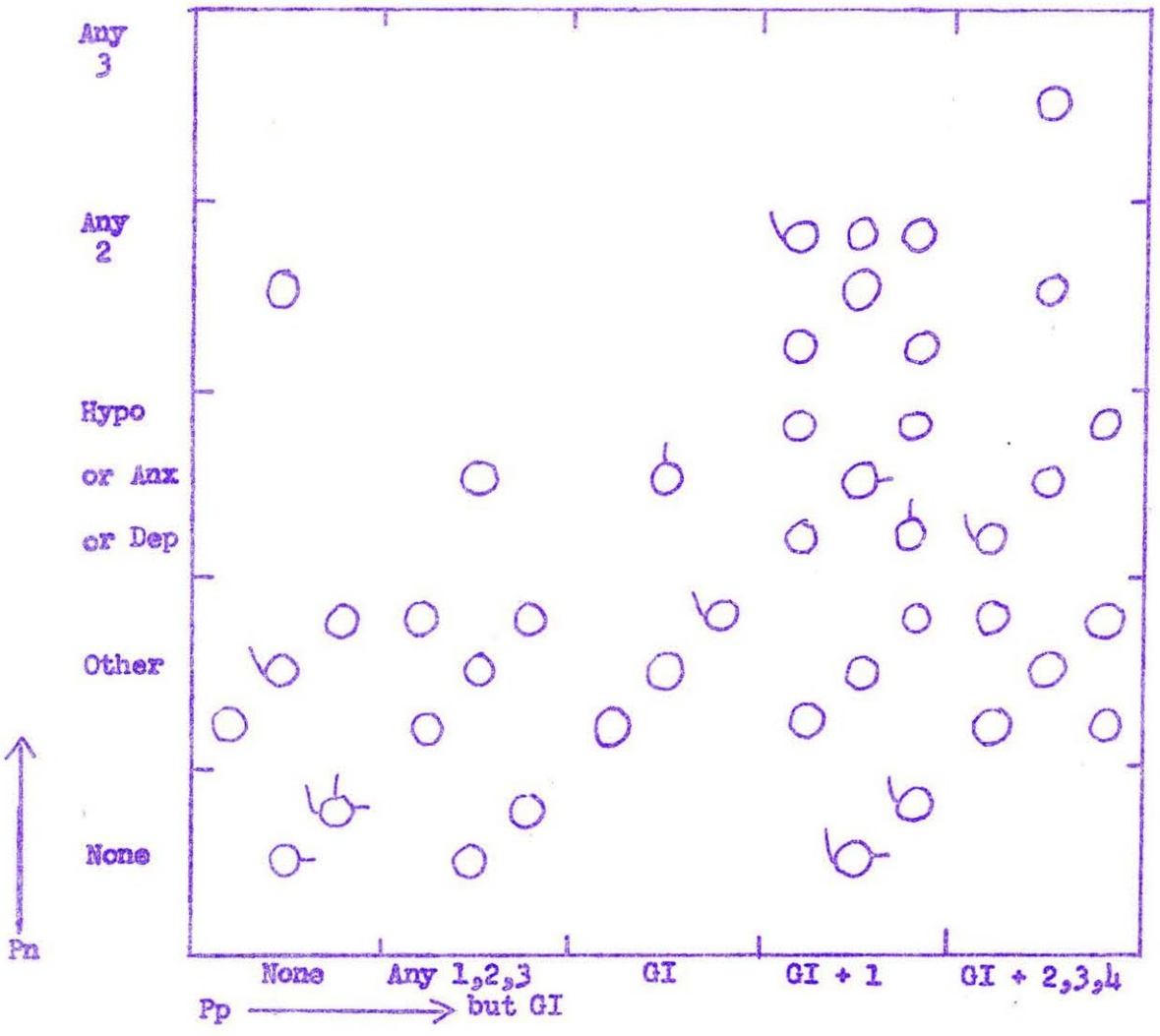


FIGURE VI

Women Only - Six Focus Communities

A+ A- B+



and VIII resulted. The negative relationship is most striking in the case of Sociopathic symptoms, where there is a heavy concentration of glyphs in the no-Pn zone. In the case of the Mental Defectives, half show no Pn, the rest are scattered; with the Sociopathic almost twice as many show no Pn as are scattered over the rest of the diagram. There is, likewise, a negative association between Sociopathic and Mental Deficiency, with only 3 individuals showing both, two of them from the DA's.

On the whole this is what one would have expected--on the hypothesis that Sociopathic symptoms represent an alternative mode of reacting to life's vicissitudes from the one which shows as Pn symptoms. There is no very clear relationship in either diagram with any particular Pp patterns, though the no-Pp-no-Pn box is comparatively crowded in both diagrams, and there may be a slight negative association with GI for the Sociopathics.

In summary, the following points have been noted, and might well be tried out on the whole-county material to see if they hold there also:

1. More women than men in AB ratings for Organized, opposite for Disorganized.
2. No Sociopathic for Fairhaven, Alcoholism for Lavallee, both Alcoholism and Dyssocial for DA's.
3. DA's extreme in every way--more AB's, more rays, more in diagram corners.
4. More acting-out type symptoms in men than women.
5. Negative association between:

   Sociopathic and Pn (strong)

   Sociopathic and Mental Deficiency (strong)

   Mental Deficiency and Pn (moderate)

FIGURE VII

All Mental Defectives
(High and Medium Confidence Only)

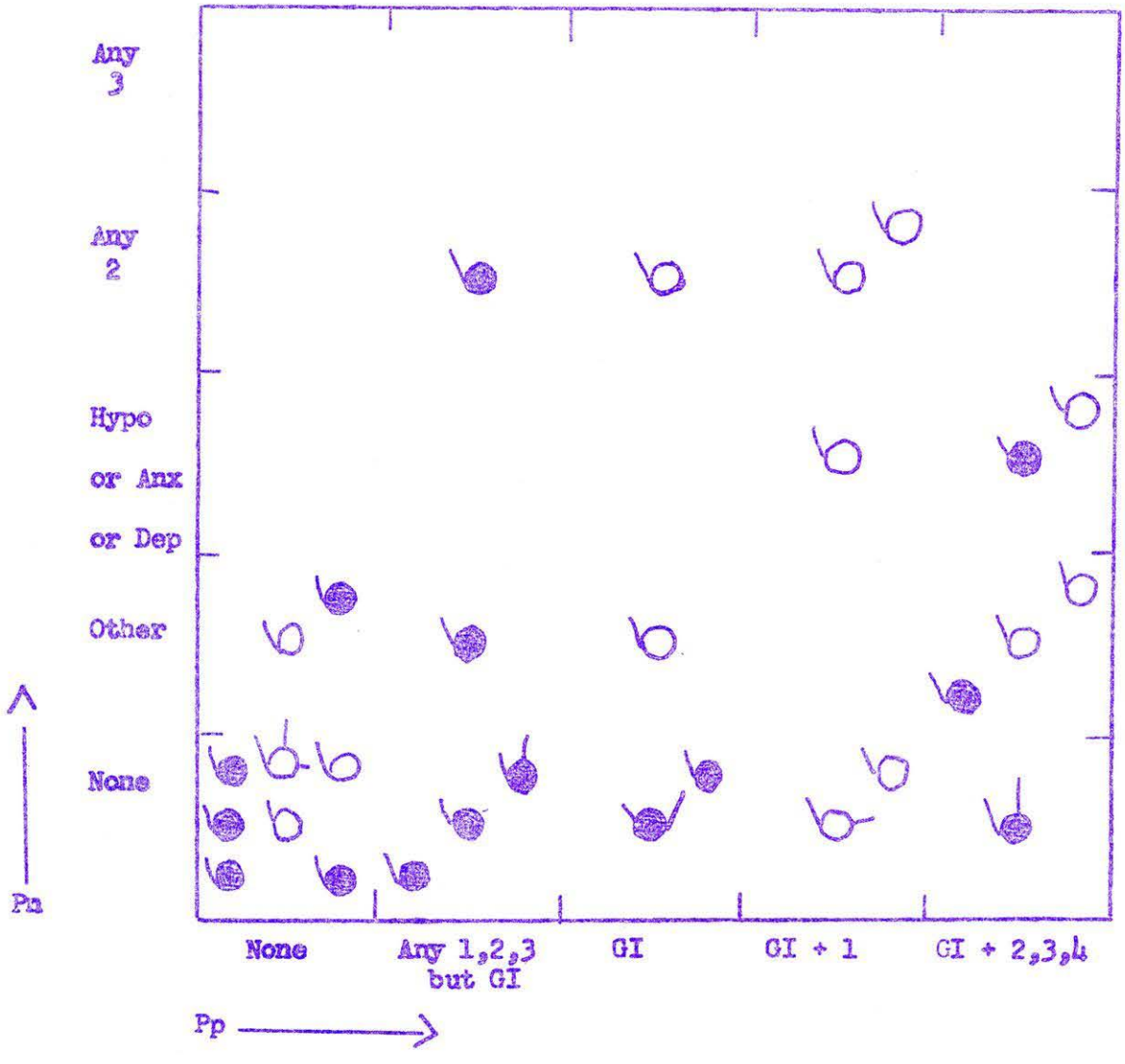
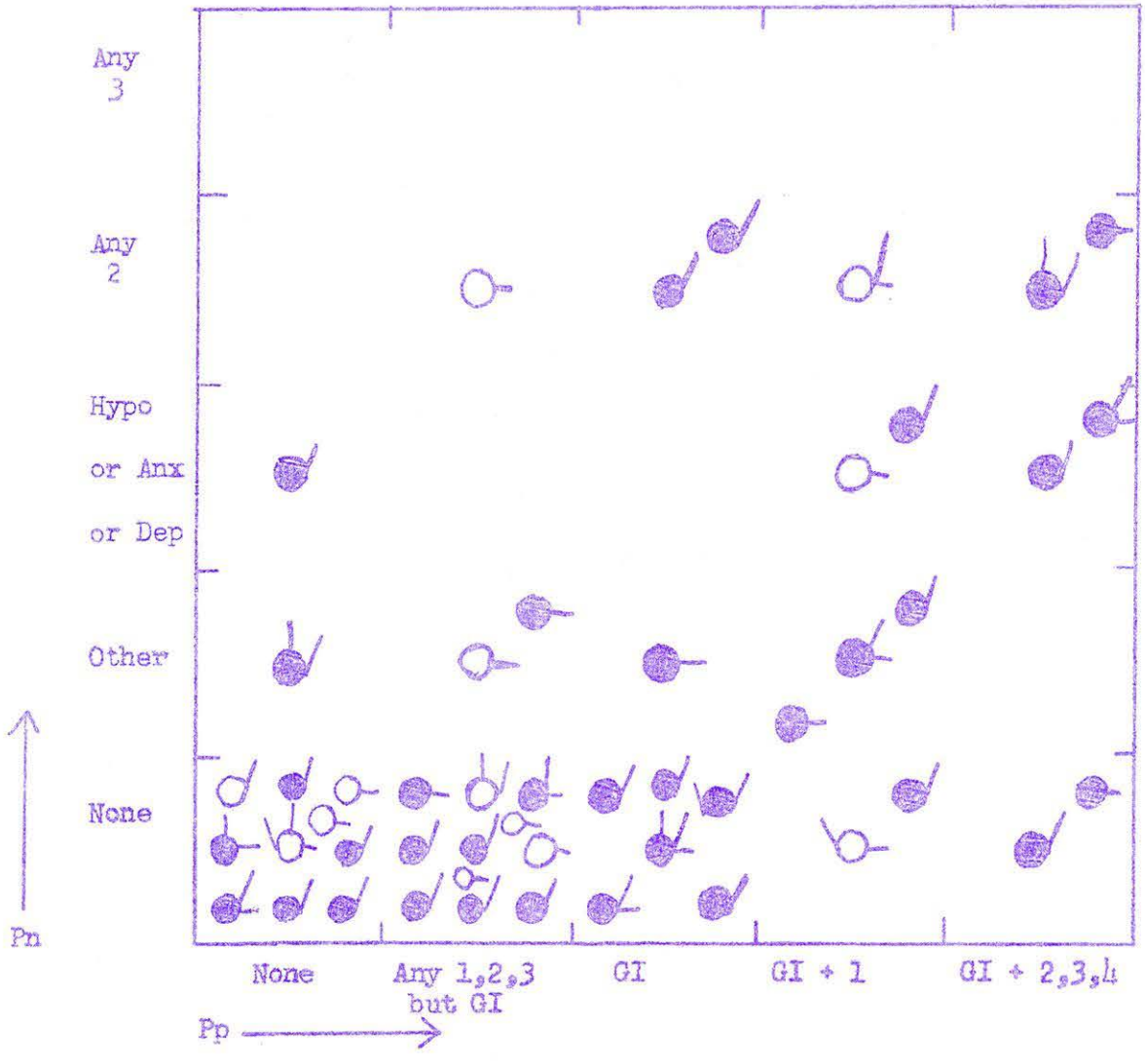


FIGURE VIII

All Sociopathic
(High and Medium Confidence Only)



"Field Relations in Community Research"[1]

Allister M. Macmillan

The purpose in exchanging a few ideas with you on the topic of field relations is two-fold: First, our programs, over the years, have tended to be largely clinical in orientation so introduction of a social topic brings some variety to the program. The experience of working with people in the clinical or educational setting is quite different, in several respects, from that of working within the framework of an ongoing community. One might say with some exaggeration, that in the clinic or the school we deal with a "captive" population. Whereas in the community research we deal with the "wildlife" conditions. It seems appropriate to emphasize this particular difference. Having had experience in all those fields, this contrast merely explains my own feeling on the matter.

Second, large increases in studies of society following World War II are reflected in the growing literature on various aspects of field relations. However, very few of the published works inform us in any detail of how the researchers went about gathering data and motivating

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[1] Read at the 9th Annual Meeting of the Maritime Psychological Association, University of New Brunswick, Fredericton, Friday 14th September, 1956. Reproduced from article published in Bulletin of the Maritime Psychological Association, Vol. 6, No. 1, Spring, 1957. The paper is abstracted from a larger one par prepared by the author for the Core Training Program Committee of the Cornell Social Science Research Center 1953-1954.

people to give information. Here, at home in the Atlantic provinces, social studies have been few so that our people are not as accustomed to the pollster type of approach as are those in some of the American towns and villages. (It has been said that one small town in New Jersey has been a field-study laboratory for such a multitude of social research projects based at nearby universities that the city council has issued an edict forbidding any more "peddlers of social science research.") We, in the Atlantic provinces, are a long way from that as yet. Offering some general comments from the rather long-term Cornell social research experience here may be useful for others who are embarking on various social research programs, and it is hoped that others may profit by our mistakes and successes.

There are several good discussions in the literature of particular aspects of field relations by such authors as Chris Argyris, John Dean, Joseph Eaton, Florence Kluckhohn, Ben Paul, Steve Richardson, and others, covering a wide variety of field situations, from studies in our own western communities and industries to work in entirely non-western cultures. But, as yet, there has been no full scale attempt to systematize all available data on the subject. Most syntheses have been with respect to specific field situations and this is pretty much the case of the present paper, being based on several years' field experience here in the Maritimes. In addition, though, I have had the benefit of personal discussions with several other colleagues at Cornell and will try to emphasize the more general aspects of field relations which cut across several quite different kinds of cultures which seem to have, at the impressionistic level, a degree of general appropriateness.

A useful paradigm to keep in mind during these remarks is that of introducing an innovation into an ongoing social process - a foreign body, if you will, into an organism. By the very fact of our trying to do social research within a community we are really introducing new social phenomena and changing aspects of the social interactive matrix. We have found, again at the impressionistic level, that such social innovation progresses through a series of stages which at each phase involves a learning process by both fieldworkers and groups in the community. I will not dwell on this but simply state the stages:

(1) The project appears to the community as an innovation meeting favorable and unfavorable attitudes which initially define the function and roles of the researchers. The staff needs to accept this situation and to learn from the community what its expectations are.

(2) Next comes a change in community expectations - some people become actively concerned with assisting the work. Earlier friends may now become enemies and former enemies may become friends.

(3) This process repeats itself with progressively more and more insight by both staff and community members. This is the general picture of the social process in trying to integrate oneself with an ongoing community.

It is difficult for the worker with some field experience "under his belt" to avoid a tendency to list a long series of "dangers" and "don'ts." Past mistakes in field work are so vivid that one rather naturally would

like to see other workers avoid these errors. On reflection, however, such an approach undoubtedly has the danger of emphasizing the negative aspects and neglects to stress the actual fact that development of social science during this past half century has been possible only through the cooperation of community people in all walks of life and in many cultures.

Our work is always premised on the assumption that people in the society we undertake to study will help us with the research, and we can generally depend on this "universal characteristic" of human beings. Our task as social scientists is to understand some of the general principles involved in gaining this human cooperation and by the use of appropriate field methods to foster this tendency to the optimum degree possible, while at the same time, being fully aware that no aspect of human relations is devoid of thorns and disappointments.

Information Gathering Before Entering the Field

Generally, after the research area has been selected, the worker should gather as much basic information about that area as he can, before he attempts to enter the field. This may be done from a variety of sources - the study of newspapers, official records kept outside the area, personal documents, published histories, board of trade publications, scientific literature, tourist and travel publications, etc., and if at all possible, by interviewing someone who has spent some time in the area previously - preferably a native who emigrated from the proposed research area.[1]

From this information, he will try to form a picture of the essential social skeleton of the society in terms of demography, economy, power and

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(1) See Lamb, R.K., "Suggestions for the Study of Your Hometown," Human Organization 11, 2, 1952.

authority structures, major institutions, other formal and informal organizations, the variety of value orientations, with customs, expectations, and taboos, any current social issues, any sensitiveness to outsiders, and the formal leadership patterns as well as the informal structure, cliques and pressure groups. One may get hints as to the usual channels of communication and to the sources of information, too. At the same time, it is desirable to consider what appear to be the major felt-needs of the community since, later on, the researcher will be trying to relate his needs as an investigator to the needs of the community in order to achieve the maximum measure of cooperation. All these topics, even in rudimentary or crude form, are essential as a foundation for formulating meaningful field relationships.

General Plan of the Research

Because of the possibility of initially tenuous field relations within the research community, it is a wise device to have a relatively flexible plan of operations for your initial research in an area where you have not worked before. In fact, nearly all initial field appearances involve the necessity of altering the plan of approach to a problem and consequently some alteration of role. The researcher must be quite flexible and not feel insecure by forced changes in his plan or in his role - he must be expecting to improvise on the spot since what seems to be an ideal plan at the university many times is really impossible to fulfill in the field situation.

Various workers have indicated the importance of prior preparation

before entering the field. Richardson[2] stresses the value of increased confidence accruing to the worker who has done careful preparation and, with Dean[3], emphasizes the importance of the initial contacts one makes. Indeed, preparation, planning ahead, and timing one's moves are just as important in a research study as they are in agriculture, warfare, or other major social action programs. In doing this planning, we must remember that the way we make the initial contacts is a major determinant in the quality of subsequent field relations.

We must not forget in our enthusiasm (or insecurity), that the social science fieldworker has a moral obligation not to upset people, particularly if he is using queries that have psychological implications, and furthermore, he has an obligation to his profession not to build up public hostility toward the procedures of social science. There was a certain worker who was wont to state that every study was like a surgical operation - "get into the community fast - rip them open with questions - then get out fast." I have had the unfortunate experience of entering a Negro community research area just after another researcher had left it, having been among the people for a month or so. He had not explained his presence to the inhabitants, nor his "peculiar" (to them) behavior in photographing, querying, and examining children and adults. They "suffered" him because he was "big, white, and elderly," but afterwards when we tried to gain the cooperation of the people, using every device to gain their support we could muster, we found that we could only get about 50% response and that

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(2) Richardson, Stephen A., "Field Methods and Techniques" in Human Organization 12, 3, Fall 1953, p. 31. This article gives several valuable references to the literature.

(3) Dean, John P., "Participant Observation and Interviewing" in An Introduction to Social Research, John T. Doby, ed., Harrisburg, Pennsylvania, The Stackpole Company, 1954, Ch. 9, pp. 225-252.

was given grudgingly. All, I am sure, a direct result of the previous worker's very limited perceptions of what constitutes good field relations and of his own obligations not only toward the natives but to other researchers who may follow him. (While his scholarship is unquestioned, his field relations were very poor.)

Preparing the community in advance of the actual data-gathering phase, or field work proper, is important, because the initial field work may arouse questions or misinterpretations of motive. So one needs to take time beforehand for laying the groundwork for the operation. This preparation involves an understanding of the leadership, communication, and factorial divisions of the community, and making sure that some people in each area of the social structure are informed of our research proposals and are well-disposed toward it. (Unfortunately, in many cases we do not have adequate information beforehand about the informal leaders so that our initial contacts usually must be with the formal leaders.)

Dean points out that all field situations vary so that rigid principles are not universally applicable, but "generally, field contacts should move from the persons in highest status and authority down to the actual participants in the field situation one wants to study." In special situations, such as an industrial plant, the fieldworker may need to contact both labor and management. As a matter of fact, Gullahorn and Strauss[4] each studied the same plant, one under the auspices of management, the other sponsored by the labor union. Their combined results were very interesting.

An illustration of initial contacts in rural community research can be given from our own study. The task involved drawing up a publicity

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(4) Gullahorn, John and Strauss, George, "The Field Worker in Union Research," (Research MSS).

program. This involved, first, writing letters to all the County Councillors in a two-county research area making a general statement about our proposals and asking for an appointment; in the face-to-face situation later, I was able to explain the project in simple terms and answer the immediate questions of the Councillors and their wives. Then they were asked to cooperate in revising local large-scale maps which showed all family residences. Thus, in each locale they were actually participating in the research by assisting us in laying an adequate groundwork for our cross-county sampling procedure. This technique was carried out with the county Sheriffs and the Town and Village Mayors and the Town Clerks over the whole two-county area.

In addition, an explanatory letter was sent to every clergyman in the area, including those who were not officially ordained - the lay preachers - and to every physician and agricultural agent, asking for their cooperation. Similarly, a letter was sent to the officers of each local branch of the Farmers Organization, The Women's Institute, and the veterans group. Offers were made to speak to any group which required a fuller explanation, and several such talks were given in a friendly atmosphere. Newspaper publicity was neglected in this phase until after the initial contacts by letter and the personal interviews for the specific purpose of first informing the key people - the opinion setters - in each community what our research project would try to do. After this was done, the newspaper presentation was made and the local leaders all knew about it and could inform others - "Oh yes, so and so was here and saw me." This increased his status in his own local niche and he served as the project's sponsor to his neighbors.

An Advisory Committee was formed from the ranks of the local physicians which gave added status to our project and did not hinder the research operations one iota. Thus before the fieldworkers actually began to gather data in the two counties, at least one or several of our staff had contacted by letter and/or in person about a hundred of the formal and informal leaders all over the research area. There are many qualifications necessary in this kind of operation, apart from the degree of actual communication achieved, but there is no time to discuss these here.

Selection of a suitable role and a suitable explanation of your research purposes are closely related. Two principles are:

(1) Try to represent yourself, your sponsor, and the study as realistically as possible. Bluffing, pretending, misrepresentation, making the study more or less important than it is, are all dangerous tactics. Later lines of communication may reveal the real situation and damage your relations so as to make further research absolutely impossible. It will "blow up in your face."

(2) You need to have a plausible explanation of the study which makes some sense to the people whose cooperation you seek. This is usually not easy to do. If people get the idea that they are being scrutinized and evaluated in everything they do, then you will find resistence.

Hence it is better to indicate interest in understanding the legitimate activities of a person or group, rather than understanding or evaluating the persons themselves or their behavior.

It is rare that one can communicate the whole truth of a scientific endeavor to any non-scientific audience, and you will find that some people are bored with such a detailed explanation. Unfortunately, well-meaning researchers often make the mistake of feeling that they can communicate their general research aims to laymen very adequately, and in some cases these attempts to do so only complicate the whole field relations of the project. For now, we need to stress that the aim is not to tell a "half-truth," but to tell as much as it is possible to make meaningful to the people being studied, without invalidating the study by producing self-conscious behavior rather than the normal behavior one usually finds in that community or organization. We have tried to do this in Stirling County, but with key informants and leaders in various walks of life instead of with every individual. For instance, most of the community members of our local Advisory Committees know in a general way what we are up to, and some know it in considerable detail and with some sophistication.

If you haven't made your research purposes and your role clear to people, they are bound to resolve their uncertainty by fitting you into some category which is logical to them in relation to what they "know and can see about you." [5, 6] In other words, people will invest you with an "attitude-role" which can very adversely affect your field relations. Ben Paul gives some very interesting illustrations, and I would like to point

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(5) Kluckhohn, Florence, "The Participant Observer Technique in Small Communities," Amer. Jour. Socio., 46, 1940, pp. 331-343.

(6) Paul, Ben D., "Interviewing Techniques and Field Relations," Anthropology Today, A. L. Kroeber, et al, University of Chicago Press, 1953.

out that at various times in the field I have been identified by people as an Air Force plain-clothes investigator, a Royal Canadian Mounted Policeman in mufti, an Internal Revenue undercover man, and a Communist Spy! For example, once I was gathering information in a rural French-Acadian area about families, migration, and the like, in order to build up genealogies of certain family surnames common in the area; a partly acculturated elderly French-Acadian man and his wife were the informants. He was quite happy to give information, but his wife was very suspicious of what my "real reason" was in gathering data about the people and where they lived. She accused me of being a Communist spy!

Gaining Entry to a Community

There is no "right" or "correct" way to do this since the method will vary depending on the problem you are pursuing, the community, the degree of sophistication and your advance information about it.

The usual way is through introductions and sponsorship "from the top down" through the hierarchy and it presents fewer difficulties, but, in any case, whatever way of entry you do happen to select is bound to affect your rapport at other levels - so all sponsored approaches should be evaluated and weighed very carefully beforehand. It may seem "natural" to enter, for instance, sponsored by the president of a company if you want to study a plant but it may prejudice any information you may get (and may not get) later on in your work.

"Getting an in" may involve a number of planned devices but in some cases it may be purely a matter of good luck rather than of good management.[7, 8] A personal experience may illustrate this. The problem was to gain acceptance by a rural minority group which was "looked down on" by the surrounding community and which, in turn, was highly suspicious of outsiders. There were no formal social hierarchies or leader-sponsors valued by both groups through which to make an entrance. One morning the writer chanced to be talking to a few of the higher status men who were loading wood on the beach, and the conversation turned to "ancestors" One of the higher status men, a storekeeper, turned to a nearby worker and laughingly said, "You don't know who your grandfather was, do you Isaac?" And there was considerable laughter from his cronies and the writer realized from the set look on Isaac's face that he must be one of the despised minority group. The writer did not smile at the sally.

A few minutes later the storekeeper looked up and spotted a rather raggedly dressed elderly man approaching the wharf on a dilapidated bicycle - he said, "Oh, here comes Andy, now we'll have some fun!" Andy was one of the minority group. He was called over and the writer was introduced as "the man who's writing up the history around here and we told him you have the best memory of anybody in these parts." Andy greeted me soberly and in

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(7) Roy, Donald F., "Quota Restriction and Gold-Bricking in a Machine Shop," American Journal of Sociology, Vol. LVII, March 1952, pp. 427-442.

(8) Macmillan, A. M., Burma Road Study, Stirling County Project, mimeo 1950.

a minute or two began to recount past events, preceding each anecdote with a precise statement of the year, month, day and hour of the event - thus emphasizing his great memory.

After some minutes he began to "run-down" but then noted that his thumb was hooked into the strip of rubber inner-tube which he was using in lieu of braces to hold up his trousers. Andy began to flex the rubber outwards vigorously and recounting to the effect that - "I made these braces on the 13th day of October, nineteen hundred and thirty-seven and you can see they're just as strong as ever they was" - at exactly that point in his harangue the rubber broke with a loud snap.

The effect on the higher status men was electric - they roared with laughter at Andy's discomfiture and the storekeeper literally rolled on the beach, laughing uproariously meanwhile. Andy stood stock still with absolutely no sign of expression on his face. The writer took his cue from the previous "Isaac" incident and didn't even begin to smile. Instead he reached into his leather jacket pocket and produced a board-nail, offering it to Andy for repairs to his braces. Andy accepted the nail at once and effected the repairs - with the other people's laughter still going on. The writer thanked Andy for telling him the stories and Andy in turn invited the writer to "come around" and call on him anytime. Thus, the researcher was able to "get in" purely by chance.

Initial Field Contacts

Dean feels that one becomes accepted in a community more from personality characteristics than from what the research means to people. It certainly pays to get acquainted slowly giving people ample time to appraise the worker as being a "right guy" - "harmless" or "someone to be watched" -

and to give the worker time to become sensitive to local reactions and locally approved behavior, as well as to locate the best informants.

It is probably better to begin on a "neutral" plane rather than become typed as an "investigator or a reformer." I believe that "reformer" is the worst tag you can have attributed to you, in some communities, as far as doing regular field work is concerned. Generally, people are fairly well satisfied with the status quo of their community and resent any appearance of a desire to "make changes," particularly if sponsored by brash outsiders. Quiet participation in the community or organization, to the extent indicated by the role selected, is the goal to aim for initially, rather than trying to "act a part" or something like that.

The presence of any "stranger" no matter how unobtrusive, will change the social situation you wish to observe and will modify to a greater or lesser extent the spontaneity of the interactions among people. People need to become accustomed to the worker before his observations will have much value. The better he "fits" into the local social milieu, the faster will this acceptance be, and the more he tries to indicate how "different" he is, the longer the process will take. This has to do with clothes, the equipment he uses, and everything else.

One of the major determinants of acceptance includes having a sense of social appropriateness, which we have observed as being an extremely difficult fact for some workers to grasp after entry to a field situation - particularly a situation that is different from the worker's normal milieu. There seems to be, with some people, a "blindness" or psychological blockage, probably inspired in part by feelings of relative insecurity,

and he is unable to see that certain behaviors, perfectly acceptable in another social environment, are wildly out of place in certain field situations. The researcher working with colleagues is fortunate, here, since he may rely on their experience and their own observations to set him right on such points as these, but the lone worker very definitely has the need of some confidential local informant to help set him right on these points.

Some other methods of gaining cooperation will only be mentioned.[9]

(1) Not being reticent about answering direct questions concerning your work.

(2) Using the "passing technique" - introductions from one informant to another.

(3) Not trying to get much from the informant in initial interviews - especially if he is very willing to talk. (That's the time to stop and come back again, perhaps three or four times.)

(4) Pressuring people to give information invariably backfires.

(5) The impossibility of continuing a neutral role over a long period - especially where the community is divided on some issue. You have to take sides, otherwise you're not human.

(6) The researcher must always remember he is an "outsider," even when he is of the same ethnic and religious background. Because of this he is an obvious target for local scape-goating. This he must accept as part of his lot from the start. Not even the local politician can please all of his constituents and for a fieldworker to attempt to do so is only inviting trouble.

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(9) See Argyris, C., "Diagnosing Defenses Against the Outsider," Journal of Social Issues, VIII, 3, 1952, pp. 24-34.

If there appear to be no signs of petty animosities and jealousies in your community relations, then you can be quite sure that it is simply a matter of your own perceptions not being acute enough to see what really is there. In other words, no discord at all is a sure sign of poor field relations. People can't help being human. If they don't appear to be, you know you are wrong. These kinds of petty animosities and jealousies, plus inadequate comprehension of the worker's true research aims, are the situations from which whispering campaigns and rumors may arise. They may range from surreptitious gossip and innuendo to complete opposition to the worker and the project. Unfortunately, in many cases, the field-worker is the last one to hear such things about himself or the project.

One season I was driving a jeep station wagon painted a light grey color and I pulled over on a dirt road to let a load of hay go by. A youngster on top of the load called out to me, "When are you going to bring your airplanes?" This was very puzzling to me, but I'd heard the youngster clearly enough. Then, with further inquiry around the community, I began to see that the natives had associated the grey-blue station wagon with the Air Force and the clothes I was wearing, khaki trousers and shirt, were similar to the Air Force summer dress. They thought of me as being an Air Force person, and presumed that I was going around surveying this territory for an airfield and that "all of the people were going to be moved or sent out west somewhere." This is not at all uncommon. It occurred to me, after this experience, that one way to get a periodic "rumor-temperature-guide" and "role-status-indicator" would be to query youngsters every so often as to "what I was doing around here." Of course you couldn't get away with that too much because the parents would soon instruct the youngsters in what to say.

There are a hundred ways of maintaining your equilibrium in the field. The obvious ones, to mention a few, are: (1) the many personal neighborly services you can do for people; (2) natural participation in local organizations; and (3) feed-back of certain interesting aspects of the research results to the community. Even leaving the field imposes obligations on the worker. It is important to thank the informants in person, if you can, and if there are too many of them, then by letter. The adage of "out of sight, out of mind" holds quite as well in field relations as between separated lovers. The principle to remember here is: Most humans appreciate the appropriate behaviors, even if they think their help had been minimal. In conforming, you are thus laying a good foundation for adequate field relations for other social science field-workers and possibly for yourself as well.

Again, we have time here only to mention briefly the important topic of the fieldworkers' personal problems. This deserves a paper in itself. The fieldworker must be very wary of informants' queries to him about his co-workers and colleagues, or of other people who have been in the field before him. Many times, informants will make derogatory remarks about another fieldworker, or praise him. You have to be very, very careful in such a situation because in many of such cases the community person is trying you out. Again, an informant may say something which is quite incorrect just to see if you will correct him or not, or just to test you to see if your role is a façade or not. Lastly, the fieldworker must be a rather rugged person and I don't mean this physically. He will have to take a lot of punishment as an individual, psychologically. He must have a fundamental liking and respect for people; for individual

humanness without regard to their "manners," status, skills or education. If a person does not have this characteristic in his own personality, he should go into some other type of work; sooner or later, no matter what kind of an "act" he tries to put on, his lack of real value for others will show through and he will be regarded by the community people as an exploiter - as someone who is trying to work some kind of racket. Even more important, he will certainly not be happy in the work, after the initial stimulus has worn off.

"Liking" and "respect" have to do with the enjoyment and appreciation of an old farmer, laborer, or any person, each for himself, and not because he is a temporary curiosity or a means to the worker's academic achievement. This is a personality characteristic in the worker which he cannot acquire if it is absent, though he may develop it if it is present to a small degree. Many excellent people do not have it and will be among the first to "discount" this factor. They are too deeply involved in the values of their own culture and cannot ever really appreciate another human being who is not a member of the "right" group and who does not exhibit the correct symbols and respond adequately to the appropriate cues. A worker can do structured interviewing; he can administer psychological tests; and he can do psychotherapy without this quality, but he cannot do adequate community work without it.

Eaton's discussion of some of the intra-group processes in professional teamwork are very appropriate here.[10] In addition, the fieldworker's ethical problems are most important but we have no time for

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(10) Eaton, J. W., "Social Processes of Professional Teamwork," American Sociological Review, 1951, pp. 707-713.

them this afternoon. I will only say that a common phenomenon among workers who have not fully solved their own personal ethical problems before coming into the field, is that of an almost complete psychological blocking - particularly with those workers whose training and experience have been in a more secure work situation. Allied to this is a great activity and "business" about getting other kinds of information which they rationalize is very important to the study (and they spend hours and hours on this) instead of getting right down to the core and center of the work. It is a very traumatic experience not to have solved your own problems before entry to the field. Here you are gathering confidential information from people, putting it down surreptitiously in notes, and reporting it in your field journal. "Is this right?" Such problems can only be solved by yourself. We have seen plenty of good workers "go on the rocks" just because they hadn't resolved such conflicts beforehand.

Conclusion

It will be well to try to end this discussion in a positive key. The beginning fieldworker is urged to remember that the vast majority of field operations have been carried through quite successfully without untoward events to mar the researcher's public relations. He will do well to recall, also, that when he enters the field situation - feeling very much "the stranger" - he will not be regarded by the "natives" as another Gilliat of the But de la Rue depicted in Victor Hugo's "Toilers of the Sea" and subject to all manner of trials, scrutinies, and suspicions. On the contrary, he will find that he will quite easily "fit in" if he takes things easily and "goes along" with events, customs, and mannerisms without "aping" people and otherwise remaining normally inconspicuous.

In general we can say that probably the best way to gain adequate field relationships is to be friendly in a quiet way and to do our level best to relate our research aims and activities to the felt-needs of the human group with which we are working. The purpose of the project may very legitimately have various levels, but we must be sure our various informants are offered the one they can understand, accept, and appreciate as ultimately good.

A COMPARISON OF ORGANIC AND PSYCHIATRIC SYMPTOMS IN A SMALL TOWN

by

William D. Longaker, M.D., and John O. Godden, M.D.*

Introduction

The study to be presented here is a sequel to one reported in 1956 by Dorothea C. Leighton on "The Distribution of Psychiatric Symptoms in a Small Town."[1] In that study, four psychiatrists (one of whom was the senior author of this paper) acting as a panel made estimates regarding the prevalence of psychiatric symptoms in a probability sample. This consisted of approximately 20 per cent of the adults in a town, "Bristol," that had a total population of about 3,000. An adult was defined as anyone eighteen or over. One of the conclusions reached was that 37 per cent of the sample number had symptoms of psychiatric significance and were impaired by them to an important degree. This estimate includes psychoneurotic types of symptoms but excludes those which are purely or mainly of a psychophysiological (psychosomatic) type.

The data upon which the psychiatric judgments were based consisted in a protocol on each of the 283 respondents in the sample. In addition to certain questions of psychiatric import, this covered a review of health history (including a check list of ailments and symptoms) obtained

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* Pauline Mahar, Ph.D. (Sociology), contributed substantially to the planning and carrying out of the analysis of the evaluation data.

1 The American Journal of Psychiatry, Vol. 112, No. 9, March, 1956, pp. 716-723.

from the respondent in a survey interview, the impressions of two general practitioners, and the results of searching the records of five hospitals serving the area. In a few cases (12) there was no interview with the respondent but with a spouse or other person close to him.

The estimate of psychiatric symptoms being on hand, it seemed of interest to go back to the same data and reanalyze in order to obtain an impression regarding the prevalence of organic symptoms. It is realized, of course, that people get ill as whole beings and that organic illness may often involve concomitant mental and emotional disturbance. Despite this it appeared worth-while attempting estimates as to per cent of people with predominantly organic types of symptoms. The hope was to get some insight regarding the relative load of organic symptoms as compared to psychiatric symptoms in a small town.

Evaluation Procedure

The evaluation procedure for the "organic" estimate was developed by the senior author, a psychiatrist. However, it seemed wise to have the evaluations made by two physicians, with the second one a specialist in internal medicine. The two physicians together modified the method Longaker had developed and then independently rated every symptom found in the protocols of the sample.[2]

In the present study the word "symptom" is used in a much broader sense than is customary in medicine, namely to mean a reported fact or

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[2] Some protocols were eliminated from the original sample for reasons that will be discussed later. The psychiatric evaluations were, of course, retabulated with these changes in order to carry out the comparative analysis. It may be noted here that these alterations made no significant differences in the percentages previously reported.

statement about illness as perceived by the respondent, by the doctor interviewed, or reported in the hospital records. It includes complaints, symptoms in the usual sense, diagnoses, and partial diagnoses.

Each symptom was rated for three qualities: 1) its organic nature (as distinguished from its psychological nature), whether it was considered to be "probably organic," "possibly organic," or "probably not organic;" 2) its seriousness from a doctor's point of view--whether the symptom was major or minor; and 3) its onset, duration, and whether acute, intermittent, or chronic. The selection of these three qualities was determined partly by the data available, partly by an assessment of the categories of illness which it would be useful for those concerned with morbidity studies to have, and partly by the previous study of the prevalence of psychiatric symptoms.

More precise definitions of these qualities are quoted below from the evaluation instructions:

1.) Organicity

> Probably organic--"Symptoms or complaints (including mention of specific illnesses) which readily suggest the strong possibility of a pathological process directly productive of (or concomitant with) structural change (past or present, acute or chronic, reversible or irreversible) in an organ or system of the body."
>
> Possibly organic--"Symptoms or complaints which indicate the possibility of a pathological process which could be productive of structural change in an organ or system of the body but where there is some doubt as to the presence of such a process. Certain responses will be considered to be automatically "possibly organic" because they are vague terms or because of the possibility that they are primarily an expression of emotional stress. Those which will be possibly organic because of vagueness are: arthritis, neuralgia, sinus trouble, pleurisy, stomach trouble, bronchitis, rheumatism, sciatica. Those which will

be possibly organic because of the likelihood of psychological components are: migraine, obesity, skin trouble, asthma, and hay fever." (Symptoms which fell into these categories might be upgraded or downgraded on the basis of other evidence.)

Probably not organic: The following symptoms: "low Blood," bowel trouble, and low blood pressure. (Again these could be upgraded depending on modifying evidence.)

2.) Seriousness

Major--"Symptoms or diagnoses which indicate a threat to the overall health and well-being of the individual . . . . This is really a medical judgment as to the seriousness of a symptom." (Major symptoms were all "probably organic.")

Minor--"Symptoms which do not fall into the major category will be considered to be minor."

3.) Acute, Chronic, Intermittent

Acute--"Symptoms or illnesses which tend to run a self-limited course of less than two months' duration."

Chronic--"Symptoms or illnesses which tend to run a prolonged and often progressive course over a period of months or years."

Intermittent--"Those (symptoms) which tend to recur again and again over an extended period of time. Symptoms which are described by the respondent as occurring 'once in a while,' 'off and on,' or 'from time to time.' . . ."

"Onset" and "duration" are sufficiently obvious to require no comment. These, however, were used as a basis for classifying symptoms as past and current. A symptom was considered current when there was clear evidence in the record that symptom was present at the time of interview; and past when the symptom was dated definitely in the past or was not clearly current.

The evaluations were made in two parts. Each physician evaluated all symptoms independently, and then the two compared their results in consultation. There was a high degree of agreement on the final evaluations.

A symptom was counted as such even though there may have been disagreement as to whether it fell into a certain category such as probably organic or possibly organic, major or minor, etc., but such disagreement was recorded.

Findings

These are presented in the order of increasing need for medical attention.

1.) 88% of the sample have now, or have had in the past, at least one probably organic symptom significant enough to be remembered and reported. It is more or less an outside figure and may be compared to the 86% with some kind of symptom of psychiatric significance noted in the previous report. Many of these symptoms are obviously of very little consequence.

2.) 60% have at least one current probably or possibly organic symptom when tooth trouble and the need for eyeglasses are disregarded. 21% (287) of all symptoms reported were in this category. This is not counting the current probably not organic and symptoms where agreement regarding organicity was not reached.

3.) 32% have at least one current probably organic symptom. These current probably organic symptoms were placed in some 33 code categories of which only seven contained as much as 5% of the total reported symptoms. These are:

Hemorrhoids	14% (15)
Deafness	10% (10)
Inguinal Hernia	6% (7)
Unclassified Medical	6% (7)
High Blood Pressure	5% (6)
Bodily Injury	5% (6)
Alcohol or other addiction	5% (6)

The rest of the symptoms may be listed as follows: cancer, diabetes, other metabolic disease, asthma, hay fever, anemia, neurologic disorders, eye injury, cataracts, ear trouble, heart trouble, varicose veins, bowel trouble, tuberculosis, throat trouble, bronchitis, pleurisy, peptic ulcer, stomach trouble, gall bladder, kidney trouble, boils and abcesses, skin trouble, arthritis, and rheumatism.

When body systems are considered, we find that a plurality of the symptoms reported may be grouped mainly in terms of four systems: circulatory, respiratory, digestive organs, and organs of movement.

Most of these current probably organic symptoms were minor (76 or 67%), but there were 34 (33%) which were major and therefore could be considered serious.

4.) 11% of the population have current symptoms evaluated as serious, that is to say, a threat to the overall health and well-being of the individual. These symptoms consist in the following: pleurisy, tuberculosis, anemia, heart disease, high blood pressure, stomach trouble, bowel trouble, gall bladder disease, kidney disease, rheumatism, arthritis, neurological disorder, other medical (hemophilia and esophageal constriction), bodily injury, diabetes, cancer, and alcohol or other addiction.

The psychiatric symptoms in the original study were not evaluated in terms of seriousness but in terms of impairment. The different criteria here reflect differences in our conceptions of organic and

psychiatric disorder and also of some difficulty in making a cross-comparison with regard to prevalence of these disorders. In very general terms, however, it is probably appropriate to consider this 11% figure in relation to the 37% figure given in the previous paper. This figure of 37% is on a base N of 283 used in the psychiatric evaluation. It becomes 41% when the organic evaluation N of 258 is used. However, when only current psychiatric impairment is considered, the figure 41% is reduced to 38%.

Serious or major symptoms, as used here, fall roughly into two categories: acute serious illnesses and chronic serious illnesses. Only the chronic ones would be current since an individual would not be interviewed during an acute serious illness. Chronic serious illnesses can be either current or past. All persons with major current illnesses fall into the former category and could be thought of as cases which require more or less regular medical care. In many respects these individuals can be compared to the A impaired cases in the psychiatric rating. Their illnesses are usually of long duration, are frequently impairing in the same long-term manner that the A impaired psychiatric cases are, and they hold the prognosis of continued long-term impairment. These are the only organic cases that are clearly comparable to the A psychiatric ones. There were few chronic major illnesses which were limited to the past since it was considered that such cases continued to be a potential threat to the individual currently; e.g., arrested tuberculosis, an osteosarcoma treated by amputation five years previously, or a person with a history of childhood rheumatic fever.

Most of the major past illnesses were acute rather than chronic and there were several categories of symptoms which were routinely judged to be major. These were: major operations requiring general anesthesia, abortions or miscarriages, pneumonia occurring before age twelve years, epidemic diseases (typhoid, diptheria, malaria, etc.), and bodily injury requiring extended hospitalization (multiple skeletal fractures, head injuries, etc.). There are 298 such symptoms reported, 22 per cent of the total symptoms reported. Childhood pneumonia and major operations made up the large majority.

The psychiatric rating of impairment was made differently from the organic rating of seriousness. Psychiatric disorders tend to be chronic with an ill-defined beginning and end. Impairment was estimated for the lifetime of the individual, with a focus on the effect the symptom pattern had on the individuals functioning as a citizen, family head, and bread-winner. On the other hand, organic illnesses tend to be limited to a certain brief period during which the individual may be impaired anywhere from 100 per cent to nothing at all. Lifetime impairment cannot be estimated as in the psychiatric cases except, perhaps, as mentioned above, in the major chronic organic illnesses.

In drawing conclusions with regard to the comparison of organic and psychiatric symptoms it is important to keep in mind that it is the same material that is being examined from two points of view. That is, two different judgments were made on an identical datum. This does not mean, of course, that the same parts of the datum got equal weight and attention from the two groups of evaluators as they scanned it and made up their

minds with regard to significance. Rather, it means that the same protocols were available to the raters for the two different purposes. In general it can be said that, due to the nature of most of the psychiatric symptom categories, the psychiatric evaluations were more holistic while the organic evaluations were limited to specific items of information with regard to organic disease processes. Thus to a large extent the organic evaluations were easier than the psychiatric. This is evidenced in the fact that there was close agreement on the categorization of the same organic symptoms for different respondents. This tendency to unanimity in handling any particular type of symptom (e.g., heart trouble) probably resulted from the fact that many of the items were on a check list in the survey as disease entities rather than as signs or symptoms. Such entities were fairly easy for two organic evaluators to rate and to agree upon. Hence, setting aside for the moment the question of the accuracy of such data, it was easier with the organic evaluation to consider that this person had such and such specific disorders, whereas with the psychiatric evaluations, while it was not necessarily difficult to feel a fairly high level of confidence that a person had psychiatric symptoms, it was often exceedingly difficult to have a high level of confidence as to just which particular category of psychiatric symptoms should be used. Consider, for instance, the borderline between psychoneurosis on the one hand and personality disorder on the other.

Having now some idea of the prevalence of serious organic symptoms as compared to impairing psychiatric symptoms, the question remains as to the amount of overlap between the two. Are the organic symptoms and the psychiatric symptoms in different individuals for the most part, or do they tend to occur together?

In general there is an association of organic symptoms and psychiatric symptoms in the same individuals. This can be seen in the following table.

Table #1

Psychiatric rating	Organic rating of major, current and probably organic	
A impaired	70%	(20)
C + D	10%	(3)
Other (A + B non-impaired and B impaired)	20%	(6)
	100%	N = 29

We also find that 20% of the A impaired group on psychiatric rating also were rated as having major, current probably organic illnesses. In the C + D group only 4% were so rated in the organic evaluation. 8% of the group "other" (Table #1) were found to have serious current organic illnesses.

The numbers here are, of course, too small for the application of tests of significance. However, a great many other combinations of the data were attempted and all show the tendency. To some of these tests of significance can be applied, as for instance in the following.

Table #2

# of probably organic symptoms	A + B impaired %	C + D %	A + B non-impaired %
0	5	45	7
1	15	28	14
2	14	18	27
3 or more	66	9	52
Totals	100 (120)	100 (79)	100 (59)

This table is significant at the 1 per cent level. However, it must be borne in mind that such a test of significance refers only to the probability that if all the rest of the adults in the town were studied in the same manner, the distribution would be essentially the same as in the sample. The results may represent the phenomena, they may represent the way the phenomena were perceived by respondents and general practitioners, and they certainly represent the perceptions and habits of mind of the evaluators. It is obvious that, given the nature of these phenomena, any set of evaluations such as these cannot be done by means of independent judgment. That is, it is impossible for the evaluator who makes the organic judgment not to see data of psychiatric significance, and conversely, for the evaluator who makes the psychiatric judgment not to see and weigh carefully all the information with regard to organic symptoms. Since this is inevitable in diagnosis, it is inevitable in the evaluation which attempts to simulate diagnosis as much as the data will allow in designating the categories of symptoms. The conclusions that one might draw reflect the perceptions of the evaluators as much as they do the relationships which may be in nature. What we are actually dealing with here is that, given certain limited kinds of data about the symptoms which a probability sample of individuals has, here is how medical evaluators sort these symptoms into those with mainly organic and those with mainly psychiatric significance. More data, including actual examination by the physicians of the respondents, would result in more accurate definitions and assignments to categories. But the process of sorting and the need to scan both organic and psychiatric types of symptoms would remain the same. Hence it should be clear that no causal relations are implied by this kind of association.

Some Cautions

A number of cautionary considerations have been indicated in both the first (psychiatric) article and in the preceding pages of the present (organic) article. It seems appropriate in concluding to reaffirm and expand these somewhat.

The first point to be kept in mind is that the findings presented are estimates based on data that is both approximate and uneven. Thus the physicians did not conduct firsthand studies of the respondents but utilized secondary information of three main sorts: impressions of two general practitioners, responses secured by interviews with a questionnaire, and hospital records. The general practitioners were not equally well acquainted with all the respondents and hence gave more accurate and more detailed information on some individuals as compared to others. There is no reason to suppose that such variation is random, but on the contrary is likely affected by socioeconomic factors and also by disease type. One must also allow for differential effectiveness on the part of the interviewers and for socioeconomic and educational factors affecting the way different respondents answered the questionnaire. Finally, the same and similar factors no doubt affect who goes to the hospital and who does not.

There are also problems inherent in the sampling and in the questionnaire instrument itself. A good deal of time and effort has been expended in the analysis of these problems and it is expected that this will be published at a later date since it has bearing on all the work done in the town of Bristol. It may be noted here, however, that in fact two 10 per cent samples were drawn and then amalgamated. One was a probability sample of household heads and the other of adults(eighteen or over). In addition to

sample difference, there were differences in the interviewing teams and some differences between the questionnaires. In some instances the two surveys differ from each other. For the most part they show the same trends, though one may have a higher level of confidence than the other. In approaching the organic analysis, the evaluators sought to narrow differences by eliminating all non-heads of households (N = 13) and also those individuals who had not actually been interviewed (N = 12). This accounts for the reduced size of sample in this as compared to that used in the previous report.

However, in the figures given here, both samples point in the same direction and no serious questions are involved. Where findings might be equivocal they have been omitted. From the experience of making the evaluation and analyzing the results we are led to the conclusion that the figures given are on the conservative side--both psychiatric and organic. That is to say, most of the sources of error are such as to remove symptoms and cases from consideration rather than give "false positive." Hence we feel these estimates are, if anything, on the small side. More accurate data would, if anything, probably increase, not decrease, their size.

Part III - RELATED RESEARCH SPONSORED BY ADDITIONAL GRANTS

1. Extension of Current Research

One of the first activities under the Cornell Program in Social Psychiatry was an informal seminar held among several psychiatrists and anthropologists at the Center for Advanced Study, at which the topic of discussion was, "Indicators of Psychiatric Disorder Which Might be Valid Cross-Culturally." The attempt was therefore to separate those aspects of psychiatric symptoms which are heavily influenced by specific European-American culture from those which presumably are generic indicators of psychiatric disorder. A report of this seminar has been prepared by Drs. David Hamburg, Alexander Leighton, Dorothea Leighton, and Charles Savage.

In June, through the kindness of the Director of the Navajo-Cornell Field Health Research Project, Dr. Walsh McDermott, the staff of the Stirling County Study held a series of meetings in Many Farms, Arizona, at the Project's clinic to discuss problems of mutual interest. This included the research which is being conducted by the Cornell Department of Public Health and Preventive Medicine on environmental aspects of disease among the Navajo, as well as socio-cultural aspects of the research. A basis for discussion of some of the psychiatrically-relevant cases with which the clinic has dealt was provided by the Hamburg, Leighton, Leighton, and Savage paper on the

cross-cultural aspects of society. A report on the Many Farms conference has been prepared, summarizing many points of discussion.

One of the people in attendance at the Many Farms conference was Mr. Donald A. Kennedy who is completing a doctoral dissertation dealing with problems of cross-cultural psychiatric research among the Navajo. His thesis will be included as one of the early working papers of the Cornell Program in Social Psychiatry and has been based both on library research and on field work among the Navajo. Navajo belief systems regarding psychiatric disorder and behavior pathology have been examined for their relevance to conceptions of what is deviant behavior and the etiological factors involved in that behavior. Also, on the basis of his experience in conducting such exploratory research, Mr. Kennedy has suggested a number of considerations that might be kept in mind in undertaking research of this nature.

Another exploratory study was begun by Mrs. Jane M. Hughes. It is analysis of data furnished by a key informant regarding the people of an Eskimo village on St. Lawrence Island, Alaska. The village has been under anthropological investigation both by the Leightons in 1940 and by the Hugheses in 1954-55. The key informant provided thumb-nail sketches of all the village-members from the 1940 period to the end of the Hugheses' field study. Background information concerning age, sex, marriage, children, English-fluency and contact with the white world is available for the 495 individuals who constitute the population of interest. The main data, however, are the descriptions of general

health, deviant behavior, serious mental disorders, and the informant's spontaneous comments on causal factors. These are being studied along with corollary information from health records, interviews with local administrative representatives, knowledge of village-members based on personal acquaintance, and the results of administering the Health Opinion Survey (from the Stirling County Study) to a 20 per cent random sample.

By bringing together data from these various sources, it is hoped that a working baseline can be established regarding the amount of mental disorder, distribution, and its characteristics in this non-western culture used as an example. It is planned that the results of the Eskimo study will be compared with a similar analysis of key informant data from the Stirling County Study. With regard to the latter, comments by local doctors and leaders provided one aspect of the total psychiatric evaluation. Thus comparing data gathered by similar methods, we hope to gain some impressions about cultural variation at the most obvious level. It is anticipated that this kind of approach--using local informants rather than psychiatrists--will be of value in planning a study of the less accessible aspects of mental illness in various settings.

2. Canadian National Health Grant for the Comparison of Organic and Psychiatric Symptoms in a Small Town

Research concerning the Canadian National Health Grant, awarded jointly to Dr. Alexander Leighton of Cornell and Dr. Chester Stewart,

Dean of the Medical School, Dalhousie University, has been carried out under the direction of Dr. William D. Longaker. The general outline and progress of this study were given in the Sixth and Seventh Annual Reports.

Most of the coding and analysis was completed by the first of this year and subsequent work has been directed toward producing a brief and succinct report to be submitted to the Canadian Government. Preparation of this report has been somewhat delayed by coding errors which had to be corrected before final tabulations could be made. The report will be forwarded to the Canadian Government early in 1959.

The final results are included in Part II of this report.