ILLUSTRATIVE STRATEGIES FOR RESEARCH ON PSYCHOPATHOLOGY IN MENTAL HEALTH

Group for the Advancement of Psychiatry

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Dr. Ginsburg: I have the great pleasure and privilege to turn the meeting over to Dr. James G. Miller.

Dr. James G. Miller: Psychiatrists are being asked, more and more especially in the last two or three years, to make recommendations concerning strategies of research in the field of mental health and in the study of psychopathology. Our Committee on Psychopathology has been concerning itself for approximately three years with a study of various cases of research design of different sorts which we think might have some application to our further understanding, not only of psychopathology as such but of diagnosis and treatment of pathological conditions. Certainly there is a rapid acceleration of interest in mental health research at the national level.

A number of bills concerned with the support of such activities have been before the last Congress and as you all know, a program is being developed for an assessment of the whole mental health problem in this country, as well as of the contribution which can be made by research.

Dr. Leo Bartemeier had the unique opportunity to represent psychiatry before most of the State Governors a few weeks ago when they met in their annual conference. I believe he was one of the first non-governors who ever spoke to all the Governors at once as a representative of the field of mental health.

The question of what research could contribute to the solution of one of the major problems of all of the states was obviously in the minds of many of the governors. This sort of interest means that recommendations will be asked at the local level concerning effective strategies for furthering our knowledge in psychiatry and related disciplines.

Dr. Raymond Waggoner has done a magnificent job in making possible a program in basic mental health research in the state of Michigan. He has energetically and effectively convinced our Legislature of the importance of our new Mental Health Research Institute and they have given solid financial support for the multi-level research program which Dr. Gerard, Dr. Rapoport, myself and others are now in the process of establishing at Ann Arbor. Comparable programs of similar or different patterns are now being set up or planned in a number of other states.

I am informed that in June the Cabinet of the United States for the first time devoted a major portion of one of its sessions to the
problem of mental health, which seems to represent a significant indication of the same trend. This did not appear in the newspapers.

Today we would like to present to you a series of case histories in research design, and method in the field of psychopathology. We are quite aware that there are innumerable equally promising forms of organization of research in this field and that there are other categorizations of research, all of which make sense.

While the categorization of such research is an arbitrary matter, today we have selected an emphasis on the notion of levels of behaving units, first the level of cells, then the organ, then the individual, then the group and the society, the cells being made up of subunits of molecules. On the other side, of course, the society exists in its environment, the larger environment which is relevant to the behavior of the large social unit. It is clear also that roughly these distinctions also permit the possibility of considering other units like species, etc.

It also appears that these units, while to some extent arbitrary, have tended more or less traditionally, often implicitly rather than explicitly, to be the levels at which research relevant to psychiatry has been carried on. There has been a tradition of biochemists and neurologists in neurological research which deals with cells and organs, the tradition of individual clinical research and experimental laboratory research with animals and human beings at the level of the individual, and then more recent developments in face to face groups like families, communities and so forth at the level of the group dynamics, and finally a study of social process by cultural anthropologists, sociologists, as well as those who are interested in the various survey methods employed for quantifying social process.

This, then, is roughly the organization of the presentations that we are going to bring to you today, not with any feeling that this is a sacrosanct organization but just that it is a convenient one.

I would also like to point out to you the distinct possibility that while research can be carried on and a perfectly legitimate science can be established independently at each of these levels, it is quite possible also to recognize what I might call vertical associations between the levels; that is, certain individual phenomena obviously can be understood perhaps more completely by going to the level of the cell and organ. Also obviously, individual phenomena can be under-}

stood more completely by going to the level of the group, the family in which the child is reared or the stratum of the community from which he comes. So I would suggest that, as each of the speakers presents his case history of research, you might devote some attention to these vertical associations—to similarities of processes or conceptions which might profitably be pointed out from one level to another. It is even possible, it seems to me, that the advance of science at each of the levels can be facilitated by the recognition of similarities or identities of process from one level to another. After these few introductory suggestions I would like to introduce to you our first speaker, Dr. Ralph Gerard, who is going to talk about some specific studies in which he has been associated at the level of the cell and organ. Dr. Ralph Gerard of the University of Michigan.

Dr. Ralph W. Gerard: Since the program at the Mental Health Research Institute at Michigan is not yet under way at the cellular and organ level, I shall have to report to you work that had been done during the last years at the Psychiatric Division of the Neuropsychiatric Institute at the University of Illinois, work done primarily by my former colleagues and continuing associates, Ruth Geiger, Ivan Boszeremny-Nagy, Leo Abood, Ernst Sigg, Harold Goldman, and others.

The examples that I have chosen to mention illustrate experimental work at the cellular and at the organ levels, and from the latter it is pretty hard not to get into the total individual to some extent. Some show an immediate relation to the problems of mental disease, some do not; but it will be obvious enough that also the latter experiments are bound to contribute to understanding and are necessary.

At the cellular level, consider first some work on single cells, isolated neurones obtained in tissue culture. It is not a difficult job with modern techniques to scoop out a bit of brain—motor cortex or any other part one wishes from rabbits or humans, normal or schizophrenic—supply the cells under aseptic conditions with known nutrients, the appropriate salts and, unfortunately, still one or two unknown substances presented in the form of tissue extracts, and achieve a living, growing culture. As this implies, adult neurones can be made to reproduce, to undergo mitosis, in tissue culture, especially with the aid of cortisone to suppress glial overgrowth. The dogma that adult neurones, once formed, can never again divide is thus not true—with some very hopeful implications as to central regeneration.
Neurones, then, can be kept alive and under observation, and appropriate measurements can be made on them — cytological, biochemical and electrical, as the development of adequate techniques permits. With the microelectrodes we developed some years ago, having a tip diameter of 0.1 to 0.5 micron, it is possible to penetrate a single neurone without any permanent damage. The cells remain intact indefinitely, although poked repeatedly, and one can measure resting potentials across the membrane, somatic potentials from end to end, action potentials in response to stimulation, and so on. Chemical studies, which ordinarily take larger quantities of cells, are only starting, but cytological observations, recorded visually and with the aid of films, using phase contrast microscopy and all of the other aids to bring out visible changes in an unstained, unfixed, moving cell, are well advanced. Most interesting are the actions of some excitant and depressant drugs and of electrical excitation. If one stimulates these neurones with appropriate electric shocks a striking change occurs at once and reverses over some minutes, following cessation of stimulation. Some granules disappear, others cluster around the nucleus and form a dark ring. Exposing the culture to metrazol, in doses about adequate to cause convulsions in the intact individual, yields essentially the same picture. Conversely, doses of barbiturates, again sufficient to produce light narcotic action, cause quite a different cytological picture, a thinning out and loss of granules in the cells. Incidentally, biochemical changes that have been established on stimulation include breakdown of lipoproteins and other nitrogenous substances, so the dogma that carbohydrate is the only fuel for the activity of the nervous system is definitely of the past. These findings, quickly referred to, have no immediate bearing on mental disease but do illustrate some of the technological problems and advances. Until one can get cells to function with, under conditions where they can be appropriately observed, these changes could not be detected. As you know, a battle has raged for decades as to whether the narcotizing drugs have any measurable effect on the metabolism of neurones. With the newer techniques, changes are found.

Other work in the laboratory does have a very direct and immediate bearing on mental disease. Again at the cellular level, there is work by Ivan Boszerményi-Nagy, who was led, by experiences that I need not recount, to examine the metabolism of red blood corpuscles from schizophrenic patients. These offer a sample of cells, easily obtained with a hypodermic needle and a centrifuge, and able to reveal a cellular defect in metabolism, if not limited to neurones. His experiments, some of which have been published, have been most encouraging; they indicate an abnormal glycolytic process in schizophrenic cells.

The chemical sequence of events through which a glucose molecule passes, on the way to lactic acid, is a complicated one. It involves at several stages the combination of the carbohydrate molecule with phosphate ions, to form hexose phosphate, triose phosphate, etc.

One way of following this metabolic sequence is to determine the fraction of phosphate in the system which has become combined; so the change from free phosphate ion to bound phosphate gives a reasonably direct measure of these processes. Schizophrenic erythrocytes differed from normal ones in the amount of phosphate bound, in the time course of binding and, particularly, in the effect of insulin on the phosphate binding. The insulin action was in opposite directions in the normal and the schizophrenic, in one case leading to greater phosphate binding, in the other to less — seemingly a real difference at the cellular level.

While concerned with biochemical changes, let us move to the organ level and examine one or two things of interest. We found a couple of years ago (the work is continuing with additional agents and more precise identification of brain structures) that adrenalectomized rats showed certain changes in metabolism of the brain; epinephrine-dosed ones comparable but opposite changes. The whole brain shows nothing; only with a small appropriate part, as the hypothalamic region, a few milligrams out of a gram or so, can a specific metabolic alteration be identified. The changes are again primarily in the phosphate compounds, the combinations of inorganic phosphate with creatine, with carbohydrates, with other of the molecules that are intimately involved in energy liberation and as a supply of fuel for the cells. Chlorpromazine, incidentally, blocks the formation of such phosphate compounds in this same region. To pick up these changes fairly sensitive biochemical tests are available — in the case of phosphate combining it with an other radicle to form phosphomolybdic acid, which on reduction develops an intense blue color easily measured colorimetrically. Different phosphate compounds behave in particular ways which, if worked out in the laboratory previously, permit use of this test in distinguishing and measuring them. But a steady source of advancement in this field...
is the discovery of new compounds, not yet identified and studied. To pick up changes in these one must have methods that are more permissive, so to speak, and at the same time precise. These comments lead to mention of the chromatographic methods. Some of you are not so familiar with it that a few words about its usefulness may not be wasted.

A small drop of fluid, with an extremely small amount of a mixture of substances dissolved in it, is put on filter paper. Under the influence of gravity, or evaporation, or electric fields, a large additional volume of solvent is caused to flow over the spot and so carry along the substances. But a sort of competition occurs between solution in the solvent and adsorption on the filter paper. There is a steady adsorption and elution, a holding and releasing; and the fraction of time during which a particular kind of molecule is held to the paper or during which it is free to move in the flow will depend upon the electrical properties and the adsorption properties and a great many other properties of that particular molecule. I am simplifying this a bit, incorrectly, but the effect is that different kinds of molecules move along the filter paper or other adsorbent at different rates. The process continues for a day or two and the different substances are then strung out in space, separated far enough on the paper so that they can be cut apart with scissors. Further, the substances can be spread in two dimensions by following gravity with an electric current at right angles and with a different solvent, so it is a tremendously powerful technique for separating substances. They must be identified when they get there, but with some idea of the class of substances a reagent can be found that will develop a color and reveal spots of previously unidentified substances. This is the technique used to work out details of the phosphate changes in rat brains, as indicated.

With this same technique, Dr. Moskowitz isolated a new porphyrin in the laboratory last year. He had been interested in porphyria patients, with excessive and abnormal pigments in the urine, which gives a striking color reaction. He used urine from patients around the Institute more or less at random for controls, including some epileptics, and by virtue of the chromatographic method, found a hitherto unidentified porphyrin. So far it has appeared only in the urines of epileptics, an extremely exciting lead in this disease.

I find most of the time allotted to me gone before coming to what is perhaps the most directly relevant research, on stress and some of its neurological and hormonal complications. Let me put this before you very briefly.

A project we pushed for some years, at the behest of the Army, was directed to the incapacity that afflicts soldiers under battle stress. You remember reports of uninjured soldiers, during the Normandy invasion, collapsing on the beach and drowning in the rising tide. They had only to crawl a few feet above the high water mark but did not. What kind of paralysis was this? What physiological mechanism was involved? We considered as a possible hypothesis that a massive liberation of excessive endogenous adrenal medullary hormones, of epinephrine or nor-epinephrine, might cause a straightforward pharmacological C.N.S. paralysis. This hypothesis did not work out because, although we could show that indeed large amounts of epinephrine were liberated under comparable conditions of stress in animals, and that sufficient epinephrine acting upon the nervous system can indeed cause paralysis, there was still a gap of perhaps one magnitude between the amount the adrenals can discharge and that required for paralysis. The negative conclusion may not be final—we may not have gotten the maximum output or have found optimal conditions for paralysis—but in any event, this was the setting for the work.

Large doses of epinephrine given to rats will make it quite impossible for them to hang from a bar for the normal length of time. Normal rats, placed on the bar every five minutes, will hang for two or three minutes of the five, and do so over a half hour period of testing. After large epinephrine injections, the time of hanging rapidly goes down; and by the end of 20-25 minutes the rats cannot hang on at all. Animals conditioned with a shock avoidance technique and given large epinephrine doses still move when shocked but make no effort to escape when the warning light or sound appears. This may mean only that they feel so sick that only actual pain can provoke movement.

Moderate doses of epinephrine, something like 5 or 10 gamma per animal, can give considerable depression or inhibition or suppression of motor responses in the cat, as tested by direct stimulation of the motor cortex or by the knee jerk. This was pretty close to what we were looking for—liberation of epinephrine decreasing the response of the motor cortex to activation—but paralysis was never complete. Still smaller doses of epinephrine, down to a gamma or so, led to a marked facilitation of the response, up to sevenfold.
Stimulation of the hypothalamus leads to a facilitation such as one gets with epinephrine, but with a double phase: an immediate effect like that following injection of epinephrine and a delayed effect, after 10 or 15 seconds, which we could show was due to a secondary endogenous liberation of epinephrine. Stimulation of the hypothalamus via the splanchnic nerves, activated the adrenal gland of the cat to supply a second dose of epinephrine—a rather interesting case of positive feedback.

I have personally been led to the view, by a good deal of the evidence as to the action of the medullary hormones on the nervous system, that these may well have something to do with maintaining the level of consciousness. An anesthetized animal can be awakened with a considerable dose of epinephrine; larger doses produce abnormal mental states—infusing epinephrine (but not nor-epinephrine) into humans in very small doses, a few gamma per kilo per minute, produces anxiety, larger amounts might give delirium.

This is perhaps a good place to stop; if I come closer to the problems of mental disease, even at the cellular and organ levels, I will trespass into the domains of my colleagues.

Dr. Miller: It is exciting to work as a close associate of Ralph Gerard, because he has a remarkable ability to integrate conceptually a series of specific researches of this sort and to tie the physiological evidence of the equilibratory homeostatic processes going on in individual cells and organs to behavioral evidence as to what this can mean for the total individual. You have had here in Dr. Gerard’s presentation a series of studies integrated to indicate the tactics of these separate laboratory researches.

There was a time when psychiatry tended to refer to the neurological substrate of most or all psychiatric disease in a general way. Then there was a later period in our history where we were concerned almost exclusively with the psychodynamic aspects of these conditions, having found that it was not tremendously profitable, given the then existing methods, to study the biological aspects of the processes we were dealing with therapeutically. I think it is clear from a presentation of this sort that there must now be a swing in the other direction, since while it is still true that many of the conditions we are concerned with clinically are not observable under the microscope, certainly the techniques of colorimetry and chromatography and other measures of chemical and physiological process are giving us valuable new clues as to the nature of the problems that we are dealing with.

At one of our previous sessions we invited Dr. John Lilly to come and talk to us about some of his experimentation. He has been doing experiments in several areas at the level of the individual animal and the individual human being which were fascinating to us, and we wanted to have a case history of some of his work which would indicate some of the tactics that might be supported in a general mental health strategy. We were particularly fascinated with his thinking about the problems of isolation which have relevance to the psychoses as we see them clinically, and we are asking him this morning to talk about the literature concerning isolation and then about his own particular empirical investigations at the level of the individual. Dr. John Lilly is a new member of our Committee on Psychopathology and on the staff of the National Institute of Mental Health at Bethesda.

Dr. John Lilly: INTRODUCTION

We have been in pursuit of some answers to the question of what happens to a brain and its contained mind in the relative absence of physical stimulation. In neurophysiology, this is one form of the question: Freed of normal efferent and afferent activities, does the activity of the brain soon become that of coma or sleep, or is there some inherent mechanism which keeps it going, a pacemaker of the “awake” type of activity? In psychoanalysis, there is a similar, but not identical problem. If the healthy ego is freed of reality stimuli, does it maintain the secondary process, or does primary process take over, i.e., is the healthy ego independent of reality or dependent in some fashion, in some degree on exchanges with the surroundings to maintain its structure?

In seeking answers, we have found some pertinent autobiographical literature and reports of experiments by others, and have done some experiments ourselves. The experiments are a set on human subjects; truly neurophysiological experiments on animals are yet to be accomplished. Many psychological experiments in isolation have been done on animals, but are not recounted in detail here; parenthetically, the effect on very young animals can be an almost completely irreversible lack of development of whole systems such as those necessary for the use of vision in accomplishing tasks put to the animal.
AUTOBIOGRAPHICAL ACCOUNTS

The published autobiographical material has several drawbacks: in no case is there a true reduction of all possibilities of stimulation and action, and in most cases, other factors add complications to the phenomena observed. We have collected 18 autobiographical cases which are more frank and revealing than most. We have interviewed 2 cases who have not published any of their material. In this account, we proceed from rather complicated situations to the more simple ones, i.e., from a maximum number of factors to the most simple experimental situation.

From this literature we have found that isolation per se acts on most persons as a powerful stress. The effects observed are similar to those of any extreme stress, and other stressful factors add their effects to those of isolation to cause mental symptoms to appear more rapidly and more intensely. As is well-known, stresses other than isolation can cause the same symptoms to appear in groups. Some of our cases are found in the polar and sea-faring literature.

Taking our last point first, we have the account by Walter Gibson given in his book, The Boat. This is the case of the survival of 4 persons out of 135 in a lifeboat in the Indian Ocean in World War II. Gibson gives a vivid account of his experiences, and the symptoms resulting from loss of hope, dehydration, thirst, intense sunburn, and physical combat. Most of the group hallucinated aid, many despair and committed suicide, others were murdered, and some were eaten by others. The whole structure of egos was shaken and recast in desperate efforts at survival.

I cite this case because it gives a clue as to what to expect in those who do survive isolation in other conditions: Gibson survived—how? He says, (1) by previous out-of-doors training in the tropical sun for some years (2) by being able to become completely passive (3) by having and maintaining the conviction that he would come through the experience, and, we add, (4) by having a Chinese woman, Doris Lim, beside him, who shared his passivity and convictions.

In all cases of survivors of isolation, at sea or in the polar night, it was the first exposure which caused the greatest fears and hence the greatest danger of giving way to symptoms: previous experience is a powerful aid in going ahead, despite the symptoms. Physical passivity is necessary during starvation, but, in some people, may be contra-indicated in social isolation. In all survivors, we run across the inner conviction that he or she will survive, or else there are definite reassurances from others that each will be rescued. In those cases of a man and a woman together, or even the probability of such a union within a few days, there is apparently not only a real assurance of survival, but a love of the situation can appear. Of course, such couples are the complete psychological antithesis of our major thesis of complete isolation: many symptoms can be avoided by healthy persons with such an arrangement.

Solitary sailors are in a more complex situation than is the group of polar isolates. The sailing of a small boat across oceans requires a good deal of physical exertion, and the situation may be contaminated by a lack of sleep which can also cause symptoms. The solitary sailors, of which Joshua Slocum and Alain Bombard are outstanding examples, relate that the first days out of port are the dangerous ones: awe, humility, fear in the face of the sea are most acute at this time. Bombard states that if the terror of the first week can be overcome, one can survive. Apparently, many do not survive the first period: there are several pairs of ocean-crossing sailors in which one of the couple became so terror-stricken, paranoid, and either bent on suicide and/or murder, that he had to be tied to his bunk.

Once this first period is past, other symptoms develop, either from isolation itself or from isolation plus other stresses. In the South Atlantic, Joshua Slocum had a severe gastro-intestinal upset just before a gale hit his boat; he had reefed his sails, but should have taken them down. Under the circumstances, he was unable to move from the cabin. At this point he saw a man take over the tiller—at first he thought it was a pirate, but the man reassured him and said that he was the pilot of the Pinta and that he would take his boat safely through the storm. Slocum asked him to take down sail but the man said, no, they must catch the Pinta ahead. The next morning Slocum recovered and found his boat had covered 93 miles on true course, sailing itself.

This type of hallucination—delusion seems to be characteristic of the strong egos who survive: a “savior” type of hallucination rather than a “destroyer” type. Their inner conviction of survival is projected thoroughly.

Other symptoms that appear are superstitiousness: (Slocum thought a reef named M Reef was lucky because M is the 15th letter
of the alphabet and 13 was his lucky number; Bombard thought the number of matches necessary to light a cigarette represented the number of days until the end of the voyage; intense love of any living things (Slocum was revolted at the thought of killing food-animals, especially a goat given to him at one port; Ellam and Mudie became quite upset after catching and eating a fish that had followed the boat all day, and swore off further fish-eating); conversations with inanimate objects (Bombard had bilateral conversation with a doll mascot); and a feeling that when one lands, one had best be careful to listen before speaking to avoid being considered insane (Bennicot refused an invitation to dinner on another yacht after crossing the Atlantic alone, until he could recapture the proper things to talk about). The inner life becomes so vivid and intense that it takes time to readjust to the life among other persons and to re-establish one's inner criteria of sanity (when placed with fellow prisoners, after 18 months in solitary confinement, Christopher Burney was afraid to speak for fear that he would show himself to be insane).

Life alone in the polar night, snowed-in, with the confining surroundings of a small hut is a more simple situation. However, there are other complicating factors: extreme cold, possibilities of carbon monoxide poisoning, collapse of the roof, etc. Richard Byrd, in his book Alone, recounts in great detail his changes in mental functioning — and talks of a long period of CO poisoning resulting in a state close to catatonia in himself. I refer you to his book for details. He experienced, as did Slocum and many others, the oceanic feeling, the being “of the universe,” at one with it.

Christiane Ritter (A Woman in the Polar Night) was exposed to isolation for periods up to 16 days at a time. She saw a monster, hallucinated her past as if in bright sunshine, and became “at one” with the moon, and developed a monomania to go out over the snow — she was saved by an experienced Norwegian who put her to bed and fed her lavishly. She developed a love for the situation and stayed out the next year with her husband. For a thorough and sensitive account of symptoms, I recommend her book to you.

From these examples and several more we conclude the following: (1) Published autobiographies are of necessity incomplete: social taboos, discretion to one's self, suppression and repression of painful or uncomfortable material, and rationalization severely limit the scope of the material available. (Interviews with two Norwegians, each of whom lived alone in the polar night, confirm this impression.)

(2) Despite these limitations, we find that persons in isolation experience many, if not all, of the symptoms of the mentally ill.

(3) In those who survive, the symptoms can be reversible. How easily reversible, we do not know. Most survivors report a new inner security and a new integration of themselves on a deep and basic level.

(4) The underlying mechanisms are obscure.

It is obvious that inner factors in the mind tend to be projected outward, that some of the activity which is usually reality-bound now becomes free to turn to phantasy and ultimately to hallucination and delusion; it's as if the laws of thought had become projected into the realm of the laws of inanimate matter and of the universe: the primary process tends to absorb more and more of the time and energy usually taken by the secondary process.

Experiments to clarify the necessary conditions for some of these effects have been done. One of the advantages of the experimental material is that simpler conditions can be set up and tested, and additional stresses can be eliminated.

EXPERIMENTAL ISOLATION

The longest exposures to isolation on the largest number of subjects have been done in Dr. Donald Hebb's Department of Psychology at McGill University by a group of graduate students. We started a similar project independently with different techniques at Bethesda. In the Canadian experiments, the aim is to reduce the patterning of stimuli to the lowest level; in ours, the objective is to reduce the absolute intensity of all physical stimuli to the lowest possible level.

In the McGill experiments, a subject is placed on a bed in an air-conditioned box with arms and hands restrained with cardboard sleeves, and eyes covered completely with translucent ski goggles. The subjects are college students motivated by $20/day for as long as they will stay in the box. An observer is present, watching through a window, and tests the subject in various ways verbally through a communication set.

In our experiments, the subject is suspended in a tank containing slowly flowing water at 34.5°C., wears a blacked-out headmask for breathing and wears nothing else. The water temperature is
such that the subject feels neither hot nor cold: the experience is such that one actually feels the supports and the mask, but not much else. The sound level is low — one hears only one's own breathing and some faint water sounds from the piping. It is one of the most even and monotonous environments I have experienced. After the initial training period, no observer is present. Immediately after exposure, the subject writes personal notes on his experience.

At McGill, the subjects varied considerably in the details of their experiences. However, a few general phenomena appeared. After several hours, each subject found that it was difficult to carry on organized, directed thinking for any sustained period. Suggestibility was very much increased. An extreme desire for stimuli and action developed; there were periods of thrashing around in the box in attempts to satisfy this need. The borderline between sleep and awakenedness became diffuse and confused. Somewhere between 24 and 72 hours most subjects couldn't stand it any longer and left. Hallucinations and delusions of various sorts developed, mostly in those who could stay longer than 2 days.

The development of hallucinations in the visual sphere followed the stages seen with mescaline intoxication; when full-blown, the visual phenomena were complete projections maintaining the 3-dimensions of space in relation to the rest of the body and could be scanned by eye and head movements. The contents were surprising to the ego, and consisted of material like that of dreams, connected stories sharing past memories and recent real events. The subjects' reactions to these phenomena were generally amusement and a sense of relief from the pressing boredom; they could describe them vocally without abolishing the sequences. A small number of subjects experienced doubling of their body images. A few developed transient paranoid delusions, and one had a seizure-like episode after 5 days in the box with no positive EEG findings for epilepsy.

Our experiments have been more limited both in numbers of subjects and duration of exposures. There have been 2 subjects, and the longest exposure has been 3 hours. We have much preliminary data, and have gained enough experience to begin to guess at some of the mechanisms involved in the symptoms produced.

In these experiments, the subject always has a full night's rest before entering the tank. Instructions are to inhibit all movements as far as possible. An initial set of training exposures overcomes the fears of the situation itself.

In the tank, the following stages have been experienced:

1. For about the first ¾ of an hour, the day's residues are predominant: one is aware of the surroundings, recent problems, etc.

2. Gradually, one begins to relax and more or less enjoy the experience: the feeling of being isolated in space and having nothing to do is restful and relaxing at this stage.

3. But slowly, during the next hour, a tension develops which can be called a "stimulus-action" hunger ("lust" might be a better term because of the high intensity it can reach): hidden methods of self-stimulation develop: twitching muscles, slow swimming movements (which cause sensations as the water flows by the skin), stroking one finger with another, etc.

If one can inhibit such maneuvers long enough, intense satisfaction is derived from later self-stimulations.

4. If inhibition can win out, the tension may ultimately develop to the point of forcing the subject to leave the tank.

5. Meanwhile, the attention is drawn powerfully to any residual stimulus: the mask, the suspension, each come in for their share of concentration — such residual stimuli become the whole content of consciousness to an almost unbearable degree.

6. If this stage is passed without leaving the tank, one notices that one's thoughts have shifted from a directed type of thinking about problems to reveries and fantasies of a highly personal and emotionally charged nature. These are too personal to relate publicly, and probably vary greatly from subject to subject. The individual reactions to such fantasy material also probably vary considerably, from complete suppression to relaxing and enjoying them.

7. If the tension and the fantasies are withstood, one may experience the furthest stage which we have yet explored: projection of visual imagery. I have seen this once, after a 2½ hour period. The black curtain in front of the eyes (such as one "sees" in a dark room with eyes closed) gradually opens out into a 3-dimensional dark, empty space in front of the body. This phenomenon captures one's interest immediately, and one waits to find out what comes next. Gradually forms of the type sometimes seen in hypnagogic states appear. In this case, they were small, strangely shaped objects with self-luminous borders. A tunnel whose inside "space" seemed to be emitting a blue light then appeared straight ahead. About this
time, this experiment was terminated by a leakage of water into the mask.

It turns out that exposures to such conditions train one to be more tolerant of many internal activities — fear lessens with experience and personal integration can be speeded up. But, of course there are pitfalls here to be avoided — the opposite effects may also be accelerated in certain cases.

In both the McGill experiments and in ours, certain after-effects are noted: the McGill subjects had difficulty in orienting their perceptual mechanisms; various illusions persisted for several hours. In our experiments, we notice that after emersion the day apparently is started over, i.e., the subject feels as if he has just arisen from bed afresh; this effect persists, and the subject finds he is out of step with the clock for the rest of that day. He also has to re-adjust to social intercourse in subtle ways.

Experiments such as these demonstrate results similar to that given above for solitary polar living and sailing alone: if one is alone, long enough, and at levels of physical and human stimulation low enough, the mind turns inward and projects outward its own contents and processes; the brain not only stays active despite the lowered levels of inputs and outputs, but accumulates surplus energy to extreme degrees.

Apparently even healthy minds act this way in isolation. What this means to psychiatry research is obvious — we have yet to obtain a full, documented picture of the range available to the healthy human adult mind; some of the etiological factors in mental illness may be clarified and sharpened by such research. Of course, this is a limited region of investigation — we have not gone into details about loss of sleep, starvation and other factors which have great power in changing healthy minds to sick ones. I think that you can see the parallels between these results and phenomena found in normal children and in psychotics—and if we could give you a more detailed account, possible applications to "brainwashing" and its opposite, psychotherapy, would be more evident.

**Dr. Miller:** As we go from the cell level to organ and then to the individual and next to the group, we notice certain characteristic changes in the experimental designs and the research tactics which are necessary. If you were to consider each of these levels as behaving systems which exist in space and time, and recognize that each larger level is in a way the environment of the next smaller level and the smaller system a sub-system of the next larger, then you recognize that all of these systems, in order to maintain their homeostasis, or equilibrium, or adjustment, require certain inputs and consequently certain outputs from their environment. These inputs are both inputs of energy or matter, and of information of various sorts. The mechanisms of balance, of adjustment, of adaptation are largely, as the cybernetics theory has indicated to us, negative feedback mechanisms of one sort or another. Frequently when we find pathology we find that there has been some block to the input or to these negative feedback equilibrium mechanisms, or perhaps, as Dr. Gerard suggested in his last set of experiments, a positive feedback has in some way replaced the ordinary negative feedback.

One of the characteristic differences between the biological and social levels of research is that energy transmissions or transmissions of complex molecules of various sorts seem to be more important and to have greater emphasis in the biological level than information transmission, although we cannot forget that the information theory originated in the physical sciences and has been applied by Quastel and others to the biological sciences.

As we turn now to the social levels we find we are more concerned with symbolic behavior and with information transmission, although it is perfectly clear that for these symbol transmissions at the level of group and society to go on, the basic metabolism of the participating individuals has to be maintained and this involves energy transmission. I am calling this to your attention because it seems, to those of us who are trying to integrate experimental and clinical studies concerning behavior into a general pattern, that the old distinctions between biological and social, between the physical substrate and the mind, between mind and brain have somehow to be rethought and integrated into single conceptual organizations. Some of these notions which appear to be similar at different levels may possibly give us some clues as to how this can be done. At least I would suggest that ideas of this sort might go through your mind as you analyze the interrelationships into a single strategy of these various research tactics.

One of the best known research centers at the University of Michigan is the Research Center for Group Dynamics, which has been carrying on classical work in many aspects of small face to face group behavior which you all know about for a period of years. Specific studies of this group relate clearly to problems of psy-
psychiatry, and I have asked Dr. Ronald Lippitt, one of the senior members of this group from its foundation and a frequent visitor to GAP, to come and talk to us about specific studies which he thinks have implication in the field of mental health.

**Dr. Ronald Lippitt:** I think for purposes of our discussion this morning it might be most appropriate for me to outline briefly the continuity of a series of researches carried on during the past six years on some interpersonal processes of social adjustment in groups of young children. This series of researches has been largely supported by the National Institute of Mental Health.

Fritz Redl stimulated the initial work with his observations about behavioral contagion in children’s groups. Fritz has reported that material in a chapter in the Aichhorn Memorial Volume. Fritz and I started the work in a boys’ camp and a girls’ camp for disturbed children, ages 10 to 14. We found from a pilot study that we could quantitatively record and differentiate behavioral contagion and conscious influence attempts in children. Contagion has been defined as a behavioral act of someone in the group which triggers off similar action in form or type on the part of others without the initiator having the intention to influence others through his spontaneous responses to environment or to internal stimuli. Direct influence attempts, on the other hand, are defined as conscious direct attempts to influence somebody else to do something the initiator wants him to do. These acts usually are of the sort that evoke responses quite different in form from the initiatory acts. I ask somebody to tie my shoes. He does it for me. Whereas in contagion, I do something, somebody imitates this action, somebody else picks that up, and so on.

We found that we could also introduce observers into the camp without disturbing the social and therapeutic processes which were under way.

Our initial hypotheses revolved around notions that perhaps high contagion initiation might be related to individuals with low impulse control or to individuals with very high social status in the group. These might be determinants of both high contagion and of high direct influence ability. We found—and this is going to be a very schematic series of statements—that the status attributed to children by their peers was highly related to both contagion initiation and high direct influence. We found that low impulse control (i.e., high impulsiveness) actually tended to be usually related to low rather than high contagion initiation and high influence, except in very special situations where the group was frustrated with disliked social controls such as restricting adults, rules and regulations. In these frustration situations those members who were high in status tended no longer to be the high initiators of contagion and influence; those with low impulse control tended to take over as the sources of contagion.

Our interest, of course, moved rather soon to questions about the determinants of high and low status and to questions of whether high and low status in the group were stable or not. Also, we wonder about the relationship of high and low social status to mental health status.

We will get back to matters of methodology in a moment, but let me indicate what I mean by high and low status in terms of measurement. The children sorted out the pictures of each other. They either hung them on boards or sorted them into boxes. All the pictures of the members of the group were sorted along a scale of “who is able to get me to do the things he wants me to do,” from most able to least able. Then consensus about any particular individual was computed. We find high consensus in groups of this “social power” criterion going down to four and five year old children. This consensus about high and low power status seems to rest rather consistently, in a wide variety of groups, on the degree to which the child is seen by peers to possess one or more of three types of personal resources. I will again have to mention these very briefly, although they represent a complicated complex of variables in most cases.

One type of resource is what we have labeled “physical coerce-ability,” i.e., the ability to use physical force to get others to do what I want them to do.

Another kind of resource is that of *expertness* in valued group activities. This, of course, varies with the kind of group and the kind of activities it is carrying on.

A third type of resource we have had a much harder time labeling and is more complex in many ways. We call it “social-emotional resources,” i.e., the emotional capacity for expressing friendliness and reciprocating friendliness.

I must emphasize here that these are personal resources as perceived by peers and are not always in line with reality. Let me take, for example, the physical coerce-ability. There is usually quite a
high relationship between attributed power in the group and perceived fighting ability in groups of these age levels. In one type of situation (lower socio-economic camp groups), where there was a good deal of fighting going on, testing of this resource, there tended to be practically no correlation between perceived fighting ability and height and weight. The testing quickly locates the reality criteria of actual fighting ability rather than the manifest external characteristics of physical size, etc.

However, in several middle class camp groups we found quite a different phenomenon: very little fighting going on and a very high correlation between height and weight and perceived fighting ability. The fighting ability had never been tested as a resource, but assumptions were being made which were being used as a basis for attributing power and therefore accepting influence from others. So we see that perceptions of resources are not always in line with reality. Children differ greatly, of course, in objectively having these resources. Also, children differ greatly in their self-perception of possessing these resources, and they differ greatly in their utilization of their resources, i.e., their efforts to use their resources in order to achieve status and acceptance in the group.

Low social status children typically were less accurate in their interpersonal perception of the meaning of behavior of others and of the expectations that others had for them. They were less accurate in their self-perception of their own position in the group. They were, of course, low in actual social influence success. They tended to be awkward in various kinds of social influence techniques, and, perhaps one slight relation to the previous paper, they were relatively socially more isolated in terms of getting information from the group as well as making contact with the group. They were found, these low social status children, to have a higher incidence of the occurrence of social agency judgments of personal pathology and familial background pathology.

Rosen made a study in which he made predictions from social agency case records as to what would happen to social status and social behavior and social perception of these children when arriving in camp and becoming members of a new group. It was possible to differentiate two different background patterns of interpersonal pathology from the case records of the low power children which led to differential predictions. These were: a withdrawn, inhibited social kind of pattern and an aggressive, hostile, acting out pattern.

The predictions turned out amazingly well.

Another fact is that this structuring of social power status takes place with remarkable quickness as the new groups form, and what one finds at the end of a couple of days is closely related to the group structure at the end of the two, three or four week camp period. There is a high stability over time and also a high stability of who is able to influence whom from activity to activity. Although perhaps the relevant resourcefulness changes in terms of who has resources, this does not change the influence pattern. The same people dominate from activity to activity.

From this first four years of research with some 40 or 50 groups of children, we felt we could begin to construct a model of the social process which seemed to maintain this rather unhappy social situation for the socially powerless and relatively rejected and isolated children and also the happy situation of need-meeting social security for other children.

Let me just give a quick schematic picture. Here the child of course comes in to the group where we study him from a background of other groups or life situations, and brings with him certain conceptions of himself and his adequacy in social relations. As he comes into the group he has certain self-perceptions of what he is able to expect from himself, what he can get away with, what he can do, how much he can influence others. This of course is not at all necessarily at the conscious level. He sets up certain action intentions toward others to get them to meet his needs, to get them to listen to his opinions and so forth, which lead to certain interpersonal behaviors of his, and we have some 15 variables of this sort categorized and recorded as types of behavior toward others.

There is a significant statistical relationship between self-perceptions and the behavior that comes out, i.e., that is initiated toward others. But of course it is not the correlation of 1.00 by any means. There are all kinds of discrepancies that are of major interest. These individual behaviors of course lead to the formation of others' perceptions of the actor. As you well recognize in the interpersonal situation, others' perceptions are based largely on the behavior that comes out rather than the intentions which exist within the actor.

This is one of the interesting problems that comes up in the measurement of discrepancies in interpersonal perceptions, that individuals tend to give themselves credit for their own intentions, but other individuals primarily give them credit for their behavior.
leads to some interesting discrepancy problems. This again is a kind of statistical relationship which comes out. We can find clear statistical relations between the behavior patterns of the actor and the conceptions that develop about the person by the other group members—their readiness to accept his influence attempts, the readiness to be deferential, to reject, ignore him or what not. And their perceptions, of course, lead to their behaviors back toward him. So there is continuous flow of information coming back to the actor which has at least, information-wise, a potentiality of influencing his self-perception and providing some correction of his behavior patterns.

Because of the interest of methodology let me just mention quickly the kinds of things that are measured at these various points in the interpersonal interaction process. To measure self-perception we have the children place their own pictures on the scales, asking them at this simple level of measurement how do they see themselves, how they think the others see them. We also have some projecting story material in which there is a chance for them to identify with figures in the story and to indicate at that level their self-perceptions of their adequacy and inadequacy in interpersonal situations.

To get at actual behavior output we have developed behavior observation procedures where observers record behavior quantitatively in standardized situations with children playing games, carrying out activities of various kinds. Then, to get group perceptions of the actor, we have all the other group members making judgments about abilities, fighting abilities, how much I like him, how much he can make me afraid if I don’t do what he wants me to do, and so on. Then, of course, the behavior observations are getting data at the same time on what kind of behavior each person is receiving from the rest of the group. We have found several points in this circular process where pathological states tend to maintain themselves or become progressively worse.

First of all there is, at the point of intrapersonal functioning, a great deal of inaccurate orientation as to his own resources. Second, there is considerable inaccuracy or insensitivity to the needs and expectations of others, so that there is misinterpretation or lack of reception of messages relevant to a realistic conception of the self.

Then, in the area of interpersonal interaction, there is often awkward behavior in expressing needs and intentions. In other words, the problem of moving from intentions to behavior shows consistent patterns of inadequacy. Usually there is a strong need to be accepted and intentions to achieve influence status with peers. We find some children at the bottom of the power hierarchy in the group that we can label “under-users.” They have more influence potential than they use. Others we call “over-users,” who maintain a situation of feeling inadequate and trying to make up for it by high activity and very aggressive behavior. There is rejection of this behavior by the group, leading to still more active and aggressive efforts to get status. This process goes on and on. With the “under-users” there is a process of more and more withdrawal and inhibition.

On the interpersonal or group consensus side of the process there is a great deal of stereotypy of the individual. An initial impression of some kind is made, and in spite of the efforts of the actor to change his behavior pattern there is a tremendous perceptual lag, so that the individual does not get reward for the efforts to change which he makes. The group members are still reacting as though he was the way he was before, rather than the way he is now acting. This perceptual lag problem represents one major area of group pathology.

Another process pathology is the lack of appropriate feedback on the basis of which an individual can guide his behavior. We have some evidence that these young children do better in giving more accurate feedback than do sorority women about whom we have some comparative information. More misinformation is given by the adults. The children are relatively open in the reactions they give to each other by comparison. So there is more accurate self-perception of our position in the group by the youngsters than the adults.

Now the question which we have been asking and on which I want to remark in closing is: Can the process of maintenance of low status or powerlessness and rejection be changed for an appreciable number of children without intensive individual therapeutic effort or re-grouping? We are now completing the first year of a two-year experimental study of this question, with the population of about 40 elementary school classrooms, largely second and fifth grades.

Let me briefly outline the design. We are making three types of attempts to induce a change in the interpersonal situation of these children: first, by helping the socially rejected children directly to achieve greater accuracy of interpersonal perception, greater sensitivity in perceiving, in setting their own interpersonal intentions toward others, i.e., to meet their own needs, and at the same time
consider the requirements of social situations, and to use greater
skill in behaving to carry out these intentions. The experimental
helping method which has been developed is called “The Chair
Game” by the children. They are out of the room in groups of three.
There are a variety of chairs which have personalities. “AC” stands
for aggressive, an acting out member of the group. A chair with the
name “DO” on it stands for the docile, passive child in the group.
The adult sits in various chairs and behaves expressively. The kids
play a guessing game, what is the meaning of that behavior, what
is the person trying to do. Then the chairs begin to act toward the
children and they have to make guesses again of why would that
chair be behaving at the drinking fountain like that toward me, and
so on. Then they begin to interact with the chairs, getting the con-
sequence of behaving one way, trying it another way, and so on.
These situations are set up to fit the diagnostic data for the various
individual children in the experimental groups.

A second procedure is by attempting to work through the most
powerful children in the group. They are taken out for special kinds
of training activities to achieve sensitivity to the needs of withdrawn
and acting out children. They work on skills of either encouraging
or inviting or controlling the behaviors of those who are on the
margin of the group in acceptance and influence.

A third methodology is by helping teachers to influence the group
standards of the group as a whole about social relations. The teachers
are carrying on specific activity projects under training and super-
vision. Then there are combinations of these three methods that are
being carried out in other classrooms.

The design for assessment of change, i.e., of success of the meth-
ods, includes a control period of several months for each child during
which the children carry on normal classroom activity with measure-
ment at the beginning and end to see what kind of normal changes
are going on in the group relations before the work with experi-
mental children starts. Also, there are control children in the same
room as experimental children. Both experimentals and controls are
taken out with an adult, but nothing of a particular training kind is
attempted with the controls. There are also control children in con-
trol classrooms that are not worked with at all. The assessment of
change involves both direct measures of behavior change at time 1
and time 2, which is before experimental work begins, and from
time 2 to time 3, during which the helping sessions are held. Then
there is another measurement, the following school year, to look at
whether there has been regression or continuation of any change
phenomena. The measurements include checks on changes in peer
perception and attitude, in teachers’ ratings, in self-perceptions, and
also any changes in classroom achievement.

There are several related studies going on in the families of these
children, to make predictions from the participation and role of the
child in the family to his role in the peer group, to try to track down
certain causal elements. Then, because of the physical and intel-
lectual development data we have on some of the children, we are
comparing interpersonal functioning of children of high and low
physical and intellectual maturity. Also the clinicians are helping us
with diagnostic judgments of the children who are predicted to be
not helpable or who will be helpable by this level of helping tech-
nique. We have no illusions that this level of help will meet the needs
of all children. We think it is probably appropriate for a significant
number. Our program, even if appropriate, of course may not be
long enough. These children have a series of 15 weekly sessions in
these subgroups of three or four. And then there is always the prob-
lem whether we have yet achieved any sensitivity of measurement of
this kind of change phenomenon. But certainly we need to develop
and evaluate programs of this sort which can be carried out by
teachers, visiting teachers, and other auxiliary mental health personnel.

Dr. Miller: A classical philosophical question raised for many
years is: What is the difference between a mob and a collection and
a group? Tradition has been to answer this at the philosophical level.
I think you have spelled out for us here how it is possible to study
more precisely the variables of interaction among the individuals
who are the subsistents of the group to see how they do have feed-
back, as you have called it, among themselves and precisely what
can be done in efforts to quantify these types of interactions or
feedback.

Of course, the problems of group and social research are in some
ways complex, in others, no more complex, than the problems of
research on cells, organs or individuals. Nevertheless, I think it is
quite clear that we are passing beyond the stage where we can only
make generalizations or rough approximations of the magnitudes of
forces operating in the interpersonal dynamics of groups and are
coming more to a point where we can begin at least to develop some
sort of ordinal measuring scales of these phenomena.
We turn now to the level of society and social interactions. The University of Michigan, Institute for Social Research, contains two centers. Ron Lippitt is a member of one of those centers. The other, the Survey Research Center, is concerned more with process at the total social level. Some of the concepts of cybernetics may deal with problems at the social level. We have been pointing these out as a theme without any effort to sell any particular philosophy, but simply because similarities appeared to us as we consider the concept matter of these researches. The Greek word "cybernetes," which is related to the Latin word "governator," from which we get "governor," including the governors on all steam engines as well as governors of modern states, is the origin of the word "cybernetics" which we have been using.

The question to which I suggest you devote your attention during this final presentation of a specific research tactic is whether the maintenance of equilibria at other levels which have been described in terms of feedback might also apply in some ways to the balance of power and of organizational forces in the community which result in the government of man.

Stephen Withey has been in charge of research projects in the Survey Research Center which I believe have clear relevance to problems of psychopathology and belong in a general area of research in mental disease, and I am going to ask him to talk about them now.

**Dr. Stephen B. Withey:** There is no clear chasm, I think, between considerations of the behavior of groups and the considerations of societies. Simply because there are levels of interest directly related to the size of the group we move almost imperceptibly in our studies, our concepts, or our methods for carrying out the research from the considerations, say, of ego psychology on to notions and methods dealing with group roles, to perception of the social, to considerations about pathological situations, to ideas on situational requiredness, notions about reference groups, the behavior of the representative, ideas and methods for investigating patterns of organizational behavior, notions about mass versus bureaucratic publics, community structure, community dynamics and so on. Begin to expand the size of your community and almost imperceptibly your concepts about it take on a different slant, a different meaning, and the method of inquiry begins to shift a little from the level of direct observation, to inquiry about, and on to a level where you have to make calculations about the total society you are talking about because you cannot observe it in its totality.

Two factors are particularly applicable to thinking about studies of society and particularly applicable to methods. One is the high variability among units, among individuals, if you will, or among groups. The difference from the study of the individual is in some ways considerable. One is dealing with large numbers, numbers so large it is impossible to contact, observe, or get a record from everyone. Even the census does not contact everybody. Yet the limitation on a census is severe. The restriction is on the amount and quality of information it can gather. To increase your information you have to decrease and control your contacts.

All right, we move then almost imperceptibly away from individual intrapsychic factors to commonalities, communication networks, circuits of influence, limitations set by community patterns, always keeping, however, a reference back to the individual either as the basic cell of our data or as the agent through which we are getting reports and experiences of the social, adding them cumulatively or perhaps as the element we want to relate our findings to in the long run.

In studying society we tend to report data on three levels. The most common is simple distribution or incidence of characteristics. Numerous sheets of paper report the distribution of characteristics of one sort or another in society. Secondly, we have data that deals with structure. Now I am implying scaling, order, rank, hierarchy rather than simple incidence. Lastly, we have some data on the dynamics, or functioning of society, usually control aspects, some maintenance factors, others making for change. The data go down in reducing amounts. We have little on the last area. Then we have analyses dealing with the factors that influence a distribution, the influences on structure, the influence for change or stability.

In doing this sort of thing, however, we run up against three problems that I think deserve mention. One is the necessary complexity of methods arising out of dealing with large numbers, in dealing with tremendous intrapopulation variability. One of the results of this is the need for a large administrative unit. You cannot do research on society all by yourself, not very handily. You need field staffs. You need organization. It ceases more and more to be an individual attempt. Also, we find ourselves moving more
and more into areas of probability predictions, and thirdly, we find ourselves inventing, revising and translating concepts from other fields and the invention, revision and translation is, to a certain extent, suspect. Let me make some comments on these three topics.

One of the greatest advances, that has made research on society more meaningful, is the development of sampling. If you are going to study something like 160 million people in the United States you obviously have to limit your study to certain members of that society, and the question is: Can you pick or isolate certain individuals, use them as your responding interviewees, your subjects of observation, and still speak with confidence about the 160 million? The mathematical developments in sampling are such that this can now be done with high confidence, with comparatively small numbers of observations, observation of 1 or 2 in 50 thousand and yet speak with confidence about the several millions. The new procedures for handling multivariance analysis very definitely permit one to handle the number of variables in an ongoing society which you cannot put in the laboratory easily. There has recently been developed a mathematical device whereby you can handle a matrix of about 100 by 100, something of almost unheard of complexity for analytic assignment. High speed computers were partly developed for societal investigations. If not developed we would be severely limited in the sort of analysis of complex factors that could be worked out. The need for research teams to carry out operations of this complexity I have already mentioned. Problems of individual prediction are somewhat subtle, slippery and difficult. It is not easy to predict exactly what an individual is going to do. It is not as difficult, however, to predict what a society is going to do. It is an interesting phenomenon of work on society that where you may interview a sample of people on a variety of topics you will often find that the person you have interviewed will not carry out the prediction that you or he makes, a prediction regarding passage of rumor, a prediction regarding purchase of some item of goods, a prediction about certain behaviors in relationship to joining or non-joining organizations, etc. However, the predictions you will make on a probability basis regarding the extent of this activity in a population will be moderately well sustained, much better sustained and more accurate than the predictions you will make about the individuals themselves.

Lastly, on notions or concepts applicable to society. We have some new ones but it is too early to know exactly how worthwhile they will turn out to be. We have notions about society as a machine, networks of influence, networks of communication. We have some new ideas on value systems; quantitative measurement of the hierarchy of these values and the salience this may have to various behavioral situations. We have notions about mass versus bureaucratic publics, bureaucratic public being defined as those persons in an occupational setting having more than two hierarchies — what can you do with a concept like this. We find interesting relations between this and child rearing practices, a superficially unpredictable relationship. We have contributions from systems and feedback notions from role theories. These are new enough for us not to be able to predict exactly the value that they may have in explaining what goes on in a society, large or small.

But one of my assignments at this time is not simply to review the problems or particular aspects of investigating a society, but to offer as an example of procedure one particular study, also to offer this as an example of findings and, perhaps, to offer it as suggestive of uses of methods of investigation now available in the study of society.

The study I am going to report is a study of activity in a society confronted with an issue, problem, question or threat. The issue or threat has some salience to the values of the society, although I am not necessarily speaking of the destruction of the society but at least something of relevance to the continued functioning of society in valued ways. I need to explain this dependent variable a little because I am going to talk of findings describing who are the members of the group that can be defined as being active people. By “active” I refer to attention, consideration, thought, discussion, behavior about this issue, about this threat, about this problem. I do not refer to the particular type of thought, pro-ness or con-ness regarding the issue. I do not refer to whether the person takes an administrative post or subordinate post to do something about this. I simply refer to the attentiveness, time and energy which the person devotes to this particular public issue. One might regard this as an interesting variable basic to considerations about mental health in terms of the response one can get from a community or a large group of people who are relevant to doing something about a particular issue. This is the dependent variable which I am calling “level of activity.”

What are the factors that influence this level, or to look at it in
another way, what factors can I find that are related or correlated among these people who report highly on level of activity?

In terms of methods, my data come from two studies, one a sample of the United States adults 21 or over, a sample of 1500.

What is the validity or reliability of a sample of this size percentage-wise? I can speak with confidence from a sample of this size within plus or minus 4 per cent. It is that good. You can prove this mathematically. This is not just an empirical matching. You can prove how right or wrong your results can be.

I report also from another study, around 400 individuals, adults from the adult labor force in Detroit. This is a smaller sample and the margin of error on such a sample would be around 8 to 9 percentage points plus or minus. But, I am not going to report percentages here. They are dull in any case, and I am simply going to report correlations that are significant in a chain, backing up from level of activity, through a variety of social variables, and finish up on an individual level. These samples of people were interviewed in non-directive, face-to-face interviews. The data so obtained were analyzed by content analysts rather well trained in their tasks. Scales were developed for handling the data in a quantitative fashion. The data were then put on IBM cards for handling in cross analysis tabulations. Finally the data were submitted to statistical analysis.

Starting, then, with the data on the level of activity which I have already referred to as a dependent variable for which I was trying to get some explaining factors, I have data on ideas about solutions for these problems, threats, or public issues confronting the society. I have information on seen mechanisms for achieving these solutions from individual reports. From individual reports I also have estimates of the likelihood of solution. How likely it is that these solutions will really be accomplished, probability guesses. I have data on the felt effectiveness of people, like the respondent on decision makers, and how sympathetic, or responsive, or sensitive they are to public interests. I also have data on the estimate of effectiveness of these decision makers, largely at the national governmental level, not necessarily on a specific policy, simply general effectiveness. Lastly, I have some measures on effectiveness as perceived in the immediate personal world of the interviewee: general questions such as: do you feel that you can plan in the years ahead in terms of how your life is going to work out or have things gone the way you would like, if you look back on your life over the last years? — no particular reference to particular successes but a general level of efficacy as measured on three or four questions and therefore scalable. Thus we get a spread, not a simply typology, but a spread from those who are ranked at one end to those who are ranked at the other end, the name being given to extremes, but no sharp bipolar distribution being evident.

Before I report the relationships we found among these measures I think it would be handy simply to introduce some notions we had that made us pick on these measures and perhaps offer some explanations for the sort of relationships we did find. There are four that I think are worthy of mention at the moment. One is that what is important in activity, or at least an issue-confronting-activity, is the ratio of information about the issues to the information resources for doing something about this issue. This ratio is important. Raise the information about the issues without raising the information about the resources and I would predict no particular increase in activity. This was a hunch we had.

Secondly, what is important in activity is not just the attractiveness of the solution but conviction about the probability of its success. This is not a valence concept in Lewin's terminology, but the passability of the path.

We also had a hunch that "felt-probability-of-success" is greater, on the average, for the person with more ways and means or greater resources. So that it is not just a matter of probability of getting through one way. The person with a multitude of ways to choose from has suggestive feelings of probability that are higher than the person with one way or means of achieving a solution. There is some peripheral evidence that mathematical ratios don't hold for psychological probability; studies of betting, gambling, things of this sort show psychological odds of 20:20 are very different than odds of 1:1. Take a gamble where there are 20 sorts of ways to lose and 20 sorts of ways to win as opposed to a game where there is one way to lose and one way to win, and people will take a bigger risk in the former. There are 20 ways to win, even though mathematically it is a ridiculous situation; the odds are 50:50 and that is all there is to it.

Lastly, we had some notions about personal experience, including the cooperativeness or support of in the individual's personal situation. Direct personal experience seems to act as a code. The things
With a high level of activity are more concerned with non-materialistic and other-centered values as opposed to self-centered values in decisions. I have no data on what the childhood experiences are, or the variety of other intrapsychic experiences that might lead the people to be classified in these categories. All we know is that if, at present, you discuss with a high active person the values that he holds, he tends to have chosen his occupation more from considerations of service than considerations of material return; also, when faced with child rearing problems he tends to talk more about the interest of his child than the parent. This is the sort of statement I am referring to when I allude to non-materialistic and other-centered values.

Lastly, those with a high level of activity or the effective, or the people who are optimistic about means of solution show no distinction or correlation with education. There is only a small relation to education. There is no relation with income and there is practically no relation with occupation.

Dr. Miller: Here we have a final tactic for research that we are going to consider today and I think Dr. Withey has made it clear, as have others who have been working in the Survey Research Center, that there is a great deal more to polling than coming out with the wrong answer about the 1948 election. Quantification, even with the limitations of the interview method, can contribute important new understandings of social process and the relations between the individual and the society. We see here also, as throughout all of these case histories, that a theoretical orientation toward the collection of empirical data can make the empirical data more profitable. I don't know whether hearing these four case histories has imparted to you any sense of uniformity in the behavioral process at different levels, or an increased belief in the possibility that some sort of integration can be made. Some of us are working on the latter possibility, but even a primitive integration of facts such as these at each of the levels is a long way off yet. I think all of us sense in the progress of basic behavioral sciences a tendency toward the disappearance of schools and discipline and integration into a more rounded picture which is undoubtedly a very long range tendency, which I think is increasing with the same sort of momentum that we find in the rapid exponential increase in the collection of empirical data.

There are one or two observations that I would like to make about
this series of papers in what is essentially fundamental research. The first concerns the motivation of each of the speakers. While he did get involved in a problem of fundamental research, each speaker had practical application to a real problem about human behavior in the immediate present, clearly in his mind. The interplay between the fundamental and the applied appears very clearly as you think about these researches.

A second point which I think is worth making is that each of these empirical studies rests in a body of theory. We have people who have spoken this morning with the quite different backgrounds of Gestalt psychology, of Kurt Lewin, of psychoanalysis, of electronics, of medicine, of the biochemistry and physiology of neural and endocrine functioning. However, when these theories are tested empirically they tend gradually to be altered and warped into directions of conformity with other points of view.

This also is an interdisciplinary activity, not in the sense of people of different backgrounds sitting around and talking to each other, but in the sense that information about activities of people in other disciplines and from other schools may suggest approaches to the experimenter. Ordinarily, when it gets to the empirical worker, one individual or a small group carries out the specific operations in a very hardheaded fashion. Research is not talk. It is a great deal more precise and hardheaded than that.

The final point I would like to make is that though behavior in all fields is extremely difficult to quantify, each of these approaches makes a more or less successful effort toward quantification because only by creating some sort of scaling technique is it possible to test hypotheses precisely.

Now this whole program would have no point unless this sort of almost random sample of the increasing literature of behavioral research were not shown to have some application to the day to day activity of psychiatrists and others in the mental health disciplines, and so as our final speaker I am asking our Committee member, William Lhamon, from Baylor University Medical School, to talk as a practicing psychiatrist about the implications he sees in this work.

**Dr. William T. Lhamon:** The implications of what we have been listening to from Dr. Gerard and Dr. Lilly and Dr. Lippitt and Dr. Withey will depend really not on my more or less immediate reaction but on your questions to these presentations and your reactions to them. What are the implications of this for clinical psychiatry? I would think that your own questions from this group would determine in large practical degree what implications this type of work has. So I am going to take the opportunity of cutting this down to just a few minutes and then suggesting that all of you search for implications yourselves.

I have one or two brief thoughts about the overall problem of presenting strategies in a series of increasingly complicated or complex systems, about the idea of looking at the overall forest as distinct from trees, as made up from systems, supersystems and subsystems. I find this somewhat cosmic and difficult to digest, and I think it is a matter of changing intellectual gears, which I find difficult. I don't deny the probable utility of this overall view, but I would like to express a sense of personal difficulty at encompassing so much material in a unifying system. I am reminded somewhat of Bertrand Russell's comments about John Dewey. Russell accused Dewey of being cosmically impious. Maybe we are trying to force everything into a system; I don't know. On the other hand, looking at things from the point of view of the system seems to bring a comprehensiveness and perhaps an orderness into the material that we don't ordinarily find in a complex field.

I was struck by a remark that Dr. Miller made, saying that we have been for some time focussing on the individual, and new methods such as those mentioned by Dr. Gerard, chromatography, and such as the culture of individual cells may really force us to turn back into the biological sphere in thinking about psychiatric matters.

New methods, probably more than anything else, such as those mentioned by Dr. Lippitt and Dr. Withey, have a similar effect in the other direction. Imagine the complicatedness of analyzing a matrix 100 x 100. It does not seem possible that this can be done. The computing devices and methods that are available, all of these may force us to turn from the individual in the other direction of the spectrum towards the social and the group levels.

As a member of the practitioner group and also, perhaps even more, a member of the teaching group, I think one thing that I have experienced while listening to these papers perhaps should be mentioned, and that is the zest. The fun that intellectual activity can be should be communicated to practitioners, and particularly to students. I am not so sure that people who do this sort of work do it out of an attention toward the practical problem. I have the feeling
that the fun and the zest of the work itself are in large proportion the main motivations. The curiosity involved, the rewards that come out of investigating something, are the things that strike me as being pertinent to practitioners and pertinent to student activities.

These points occurred to me, and I would suggest that the real implications of these research papers depend in reality on the questions that the group has.

**Dr. Miller:** I am going to ask those of you sitting out in the audience now to become participants. I would like to get your reactions on the specific comments of each speaker concerning his research, and also comments concerning feasibility of efforts at unification of such research in the broader pattern, and even on issues of administration. If there are going to be research programs in many places in the country as there are now, and will increasingly be, should these programs embrace all of these levels and approaches, or should they be limited simply to a single level and certain definite approaches? If there is specialization, how are communication and integration possible among the various programs?

There are a great many things to talk about and relatively little time. Are there some comments that you would like to address to any of the speakers?

**Dr. John Spiegel:** I would like to ask Dr. Withey: In what degree are our findings in any one of these hierarchical levels capable of generalization to another? I got the impression while various people were talking that this tends to happen anyway. For instance, the study reported by Dr. Lippit on children, that had to do with relatively normal children and was kicked off by observation relative to behavior contagion in disturbed children. The impetus may be generalized relatively to the same level in Dr. Withey’s study of the relationship between activity and possible levels of solution, and resources for solution. It occurs to me that this might be the kind of thing that might be generalized from the higher level to the lower level. For instance, the question at least can be asked: Is it applicable to studies, for example, of a family in a crisis situation or to small groups in crisis situation? Is the way a crisis is handled also related to the ideas of the family of what possible solutions? There are resources of the community for solution and that sort of thing. Do you have comments about whether you can generalize from society to group on matters like crisis?

**Dr. Withey:** It seems to me that since society is a non-descriptive phrase— it really is several levels of groups of increasing size—probably you are quite capable of generalizing from a group of one size to a group of another. I must admit that some of the thinking that went into our research went the other way—brought about methods of solution in intra-personality and considerations—and in trying to generalize that out you attempted apparently to generalize down. I think it is possible, but Dr. Miller might want to talk about the philosophy or the legitimacy of doing this.

**Dr. Miller:** I do not really want to get involved in this in any great detail. I do have personal feelings about the advisability of spending a good deal of effort on locating formal identities or similarities in problems at different levels, partly because if the problem has been solved at one level, it may lead to clues for rapid solution at another level and consequently to acceleration of our general understanding of normal and abnormal behavior. It seems to me that if we are all part of one society, somehow there must be commonalties of behavior at these various levels; the components must interact according to certain principles which apply to the larger systems as well as to the components. I don’t think we have given much attention to this in general in the various sciences of behavior but it would be worthwhile to do so. However, when identities are discovered they still are not of value scientifically until they have been empirically tested and checked.

**Dr. Harold Lief:** One of the things that intrigued me about Dr. Lilly’s paper was its implication concerning theory. For example, some of the basic Freudian concepts have to do with the fact that reduction in tension and the pleasure derived from that is a basic drive in the individual. There has been very little emphasis placed on the need for stimulation. I was wondering whether Dr. Lilly might have something to say about the theoretical implications of his work.

**Dr. Lilly:** At this time we have not fully integrated the findings with theory. Obviously a good deal of our thinking is given over to this very matter, and the persistence of any instinct is so obvious under these conditions that one no longer questions it or the terrific intensity and minute details of the arousal of instincts and its terrific persistence under these conditions. In fact, it becomes so important that nothing else matters. The mind is not capable of withstanding
the intensity which can develop without sacrificing a good deal of its own integrity.

Now obviously there is a lot more to this and the primary process does become dormant, not seeming to take over completely in those who survive the experimental situations. There are many ways of escaping this, as you well know, of lowering the tension, not just self-stimulation but the persistence of the effects for several days afterwards. The McGill experiments show that some people take a long time to integrate this type of exposure. In talking in great detail with one subject who went for three days, I found that not only had he taken about three or four days to recover from the three days but that he had developed a conviction that he would never go through that experience again. He was protecting himself thoroughly from a further exposure to this type of activity.

**Dr. Miller:** I am going to ask Ralph Gerard if he will consider himself part of the panel. He is here as a pre-stimulus and is capable of answering the questions.

**Dr. Henry Brosin:** Although I am fascinated by all of the presentations and I hope that all of the people will write them in places where we can read them easily, I would like to ask Dr. Lilly if his current studies and investigations have permitted him to fractionate or have graduated partial stable states, so that he can tell more about the various components of the primary process and know to what extent he has entered into definition of the differences, let us say, of functions, the nebulous field between the various kinds of unconscious functions, pre-conscious functions, as well as the conscious.

**Dr. Lilly:** You struck at a good many of the roots of the matter, but I am afraid that I cannot give you an integrated answer at the present time because this research we look on as exploratory and we still feel that we are in the process of experiments and new material turns up with each experiment. So that as it is we must hold off the kind of judgment you wish me to make at the moment, until we have a good deal more data and also a much longer time to integrate the data.

**Dr. Viola W. Bernard:** I'd like to ask Dr. Lilly, in connection with his interest in the polar solitude and sailing alone, if he is interested in some of the East Indian experiences, some of the Yogi methods in which highly traditional conditions of solitude,
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Forced Isolation and Confinement

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