The Program of the Biometrics Research Unit

I have often been asked what Biometrics Research is and what we do. Although I am sometimes tempted to respond that biometrics is what biometricians do, I feel that there is some justice in the query. I will therefore try to answer it as well as I can.

In order to understand how biometrics research entered the field of psychopathology, we have to begin by taking a look at the status of the field during the middle third of this century. It was what you might call "the time of the great invasion," when social scientists and biological scientists were "invading" psychopathology and establishing their territorial rights. From the social sciences there came those field theories in which ecological forces began to be examined—for example, in the studies by Faris and Dunham. From the biological field came the geneticists, the biochemists and the neurophysiologists, with their more atomistic approaches—for example, twin studies and studies of metabolic processes. Already well-established in the field were the phenomenological approaches of Jaspers; the psychodynamic approaches of Freud and his followers; the clinical approaches of Kraepelin; and, to a lesser degree, the laboratory approaches of the experimental psychologists, and the diagnostic testing approaches of the clinical psychologists. The appearance of all of these diverse disciplines in the same arena suggested that some form of rapprochement was necessary between these different viewpoints.

Among the first to make such an attempt was Madison Bentley who, with F. V. Cowdry, undertook a study under the aegis of the National Research Council, which was published in 1934. Unfortunately, their efforts were dismissed at that time by some clini-

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cians as constituting "picknicking in the field of psychopathology." In 1939, Dr. Walter Treadway, then Assistant Surgeon-General of the USPHS, organized a conference on Mental Health under the aegis of the American Psychiatric Association, with the cooperation of the USPHS, the National Committee for Mental Hygiene and the Mental Hospital Survey Committee. That conference was attended by psychiatrists, psychologists, social scientists, biological scientists and biometricians. Each discipline brought its own prejudices, and sharpened them against the prejudices of the others; but no consensus could be reached. Thus, even though there had been several meetings, no meeting of minds had ever taken place.

Some decades earlier, Karl Pearson had established the first biometric laboratory, which he named after Francis Galton; the use of statistics and measurement in medicine by the Pearson school—and in psychology by the Spearman school—had thus started to become more common. An indication of the opposition that this laboratory met with is to be found in the following statement by Francis Galton, which probably represents a reaction formation on his part to the clinical opposition of his own time.

General impressions are never to be trusted. Unfortunately, when they are of long standing, they become fixed rules of life and assume a prescriptive right not to be questioned. Consequently, those who are not accustomed to original inquiry entertain a hatred and horror of statistics. They cannot endure the idea of submitting their sacred impressions to coldblooded verification. But it is the triumph of scientific men to rise superior to such superstitions, to desire tests by which the value of beliefs may be ascertained, and to feel sufficiently masters of themselves to discard contemptuously whatever may be found to be untrue.

Even Karl Jaspers welcomed the biometric method. He said:

The biometric methods give us more than figures and correlations. They foster clarity in all fields in which biometric variations can be established. Moreover, through the application of these methods, we have concrete experiences which we would never have had without them . . .

Perhaps the best description of the biometric approach comes from Kraepelin:

As soon as our methodology has sufficiently proved itself through
experience with healthy individuals, it would be possible to approach
the actual ultimate goal of these efforts, the investigation of the
sick personality, especially of the inborn pathological disposition . . .
We, therefore, have first of all to investigate whether it is possible by
means of psychological tests to determine individual deviations, which
cannot be recognized by ordinary observation. If that succeeds, we
would be in the position, through the quantitative determinations at
our disposal, to establish the borderline between health and disease
much more precisely and more validly than has been possible so far.

In my own thinking, I have often likened the biometric approach
to holding up a mirror to the phenomena of psychopathology, in
order to capture a mirror image of what the phenomenologist per-
ceives and in that way to make it available to others besides him-
self. This is, of course, only an image, not the real thing; moreover,
it captures only part of the phenomena, since it misses what is
still shrouded in darkness. What it does capture, however, thereby
becomes a "public event," and as such it is subject to the scrutiny
of others.

Thus there was a willingness on the part of the phenomenol-
ogists and clinicians to tolerate biometrics, and even to regard
it as being sometimes helpful. But there was another reason, be-
sides the one indicated, why biometrics was needed. If the social,
biological, psychological and psychodynamic approaches were ever
to engage in communication across their boundaries, some common
denominator was needed. Biometrics was able to provide such a
common denominator, inasmuch as classification and measurement
were common to all these sciences. Thus, the "invasion" of psy-
chopathy by the new biometric disciplines actually produced
a common language, which was in a position to cross the inter-
disciplinary barriers.

Furthermore, within each discipline, it had become clear since
the days of Lord Kelvin that "When you can measure what you
are speaking about, and express it in numbers, then you know
something about it. . . ." Perhaps the best illustration of the
contribution of biometrics is to be found in an incident that is
reported to have taken place between William Malamud and Hud-
son Hoagland at the Worcester State Hospital. When Hoagland
found unusual biochemical characteristics in some of Malamud's
patients, he rushed over to Malamud to see whether there were any
clinical measures available, with which he could correlate his find-
ings. A little dumbfounded by the request, Malamud nevertheless handed Hoagland a group of bulky case histories. This was much to Hoagland's disgust; but that night Malamud pored over these case histories, emerging with what later become known as the Malamud Sands Rating Scale, one of the first biometric instruments in psychopathology.

As for the Psychiatric Institute, it is not easy to discover the use of biometric methods much further back than the 1930's. It is quite probable that Von Gieson himself, as well as his physical anthropologist, Ales Hrdlicka, and his psychologist, Boris Sidis, did make use of biometric methods; but the first instance of this on record is the famous investigation of Henry Cotton's theory of "focal infection," as a cause of mental disorder; the theory was exploded by Drs. Kopeloff and Cheney, in what appears to have been the first controlled experiment in psychopathology. With that exception, however, biometric research remained at a standstill up till 1930, and the establishment of the Department of Research Psychology.

Under the late Carney Landis, biometric methods began to flourish. All the young interns, starting with Bill Hunt and myself, and all the fellowship holders, including Joe Hunt, were exposed to rigorous training in scientific methodology and measurement methods; some of us took to it more kindly than did others. Dr. Landis himself was a pioneer not only in experimental psychopathology, but also in the biometric approach to evaluating the efficacy of psychotherapy, in epidemiological studies of mental disorders, and in the development of measures for assessing the behavior of mental patients. He did more than pioneer, however; he also introduced a whole generation of research workers to these methods—I was privileged to belong among them—and he provided us not only with encouragement, but also with opportunities and facilities for our work. I can still remember the day when gleefully informed me that the Department of Mental Hygiene had given him a grant of $100 to finance my Hollerith card punching and sorting!

Our debt to Dr. Landis is so great that I could hardly begin to do justice to it here. Yet it is interesting to note that, because of his own devotion to objectivity and scientific integrity and as the result of his insatiable curiosity, he attracted men who were imbued with the same spirit, not only in his field but in cognate fields as well. Not only did he develop an experimental psychological labo-
ratory of the highest caliber; he also brought into being three new departments: clinical psychology, medical genetics and biometrics—which constitutes a true monument to his genius and to the catholicity of his interests. That spirit still pervades the Department of Research Psychology under Howard Hunt, since the latter has already brought into being one new department, Developmental Physiology, headed by Dr. Daniel Stern.

Yet, despite the efforts of the Department of Research Psychology to promote biometric research in psychopathology, we did not meet with much success in that endeavor, apparently because the field was just not yet ready for it. During the late 1930's, and on into the 1940's diagnosis was in the doldrums; prognosis was guarded, and the evaluation of therapy mainly negative—hardly any reasons for clinicians to clasp biometrics to their bosom. However, in the 1950's the scene changed. There were so many therapies vying with each other, so many patients in need of treatment, that the great advantage that could be provided by biometric evaluation became clear. No one felt this need more keenly than the late Dr. Paul H. Hoch. When he became Commissioner of Mental Hygiene, he invited me, in 1956, to establish a Biometrics Research Unit. He did not specify what the unit was to do—except that I was to carry on the biometric work that I had conducted during the previous 25 years, as Associate Research Psychologist in the Department of Research Psychology. This had consisted primarily of the evaluation of such treatments as ECT, insulin, psychosurgery; the assessment of personality, through personality inventories and projective techniques; and prognostic studies, employing various psychophysical, psychophysiological, behavioral, conceptual and interviewing techniques.

Since the Budget division called for a specific statement of what the new unit would be doing, I had to write such a statement. I saw our task as consisting primarily of the development of objective methods for assessing the behavior of mental patients, with the view toward the improvement of diagnosis, prognosis and the evaluation of therapy. In carrying out this self-imposed mandate, I searched the field in order to locate the most important problems, and I then attempted to devise the means for solving these problems.

It soon became apparent that, if I really wanted to carry this out, it would be necessary to go beyond the walls of the state hos-
pital and out into the community, since that was where the action lay. This necessitated expanding the work beyond the confines of psychology, and plunging into the social sciences—and beyond. Fortunately, I was in a location in which students abounded, and I was in a position to have my pick of them. I soon found several capable students in the fields of sociology, anthropology, biostatistics; with the introduction of a Biometric Training Program, through Columbia University, I was able to support many of them through their graduate studies, while they were working on our biometric problems for their degrees. Most of them are now my colleagues in Biometrics Research.

During the last 16 years, whenever people appeared who were available to join in our work, I would invite them to join our unit, on either a grant or a permanent basis, and I would then set them loose on a problem. I regarded it as my function to run interference against all the external obstacles, so as to permit the research worker to carry out his undertaking to the best of his ability. I have had many surprises. I had never anticipated that Sam Sutton, the psychophysiologist, would become as infatuated as he is with "evoked potentials," which were scarcely even heard of at the time when he joined us; or that Ruth Bennett, our gerontologist, would become engrossed with the problem of isolation; or Bob Spitzer, our Evaluation expert, with computer diagnosis; or Kurt Salzinger, our behavior analyst, with "mmhuh" and psycholinguistics; or Muriel Hammer, our anthropologist, with social networks in doughnut-shops and Rufus Americanus; or Joe Fleiss, our biostatistician, with Kappa and air pollution; or Barry Gurland, our diagnostician and psychopathologist, with Lupus Erythematosus—to mention only a few of the surprising results that take place when you give a good researcher the freedom to follow some scientific problem of his or her own choosing, wherever it might lead.

Our growth has been promoted by the State Department of Mental Hygiene, as well as by funds from private foundations and from such governmental agencies as NIMH, NSF, NICDH and NASA.

When one is faced with the proliferating research problems of a unit like ours, it becomes difficult to put your finger on the common denominator that binds them all together. At times when biometricians are faced with such problems, they can often follow the
old injunction: “When in doubt, factor out”! But there is no factor analysis available for integrating the many different facets of our program. So I had to resort to a sort of mental factor analysis. After much whirling of the wheels, I came up with the following vectors: (1) one representing our efforts in the direction of a Mendelejeff-like Table of stimulus-response relationships; (2) one representing etiological models, including a taxonomy of techniques for testing the hypothesis on which each is based; and (3) one representing intervention efforts for the prevention or amelioration of mental disorders.

The “Mendelejeff Table” for the relationship between stimulus and response is an attempt to classify the variety of techniques that can be utilized in measuring the behavior of mental patients, with a view toward noting whether their patterns of response are different from those observed in normals. We had been utilizing the usual classic categories of physiological, sensory, perceptual, psychomotor, and conceptual responses, under such stimulus conditions as the “idling state” (no stimulus being provided at all), and at times when energy stimuli and signal stimuli are applied. Until recently, this represented a rather complicated sort of endeavor, with each rubric in such a table requiring a different technique for its assessment. More recently, our Psychophysiological Section, under the direction of Dr. Samuel Sutton (Zubin & Sutton, 1970), came up with the idea that perhaps “evoked potentials” might serve as a basic measure of the physiological responses of the individual, as well as the carrier waves for sensory, perceptual, psychomotor and conceptual responses. The ways in which this carrier wave is altered in response to various loads or constraints—such as those occurring in sensory, perceptual, psychomotor or conceptual tasks—would then represent the characteristic behavior of the individual. If the patterns that characterize mental patients under these various conditions are different from those of normals, then we have a way of describing the behavior of mental patients in terms of these basic categories. By using the “evoked potential,” we are able to measure the degree of uncertainty that the individual experiences while waiting for the presentation of the next stimulus, as well as the degree of satisfaction that he experiences when his guess is verified. It appears, therefore, that a broad range of central nervous system events—from responses to simple stimuli, such as brief light flashes or tones, to
responses to the more conceptual notions, such as uncertainty and pay-off ratios for correct guessing—reflect themselves in the "evoked potential," and thus provide us with a sensitive technique to use in the classification of mental disorders.

The second vector, which deals with etiological models, represents our search for the causes of mental disorders. Beginning with the descriptive model that is utilized in the diagnosis of mental disorders, and by way of which we have made some advances through the use of structured interviews, we go on to such etiological agents as those involved in the ecological, developmental, learning-theory, genetic, internal-environment, and neurophysiological models.

These models can be classified in two major groupings—field theory models, in which the individual simply occupies a nodal point in the interacting field forces that impinge on him, and atomic models, in which each individual is a discrete unit with his own individual characteristics. The ecological model represents the extreme end of the spectrum for the field theories, while the genetic model represents the opposite end of the spectrum for the atomic theories. The developmental and learning theory models lie closer to the field theories, but they also take into account individual characteristics; the internal-environment and neurophysiological models lie closer to the atomic theories, but they are not unaffected by field forces.

In the area of the ecological model, we turn to interviewing and observational techniques for testing the hypothesis on which this model is based. The researches of the Gerontology Section, headed by Dr. Ruth Bennett, which deals with the role of social isolation, and the researches of the Anthropology Section, headed by Dr. Muriel Hammer, which deals with the role of network relationships, are complementary; together, they provide us with more elegant methods for measuring factors that up to now have been measured very crudely. With regard to the developmental model, the Behavioral Analysis and Modification Section, headed by Dr. Kurt Salzinger, has provided us with a whole series of culture-fair techniques—techniques that can be transferred across cultures without losing their capacity to discriminate between patients and normals. Finally, in the genetic, internal-environment, and neurophysiological areas, we have recourse to psychophysiological measures that attempt to measure the intactness of the nervous system, with ref-
erence to the presence or absence of psychopathology. For these models, the culture-free indicators appear to be the method of choice; since they utilize the first 1000 msec of response time, the influences of culture are minimal, so that they can be used cross-culturally without significant distortion.

The third vector I mentioned earlier was that of intervention. We are now beginning to enter the community, in order to detect high-risk populations for hospitalization for mental disorders of the senium; from this point, we shall be spreading out to search for other populations of high risk in mental disorders. The demographic techniques of sample selection, the development of screening devices for interviews, and all sorts of related problems are gradually being solved, to meet the needs of intervention and the prevention and/or amelioration of disorders. The problems of follow-up are also very important parts of these investigations, since they constitute difficult problems in urban areas—problems that demand solution.

The easy growth of coalitions across disciplinary lines that characterizes our Unit is exemplified in many ways. Our Anthropology Section is highly concerned with problems of behavioral analysis emanating from our Behavioral Analysis and Modification Section; our Psychopathology and Diagnostic Section is very much interested in the sociological aspects of gerontology, and interacts with our Gerontological Section to a great extent. Our Evaluation Section and our Psychopathology and Diagnosis Section are well integrated with our Psychophysiology Section, for the purpose of developing better methods for classification of patients, so as to minimize the variability that arises with heterogeneous patient groups. Two of our sections have developed an integrated relationship with the clinical research and teaching functions of the Psychiatric Institute. Our Evaluation Section serves as the research group for the Washington Heights Community Service, as well as assisting several other divisions of the Psychiatric Institute. Our Diagnosis and Psychopathology Section has established close connections with the Educational Director of the Psychiatric Institute, through teaching and supervising medical students in psychopathology, and utilizing our rich library of videotapes for their instruction in diagnosis and psychopathology. Our Section on Behavior Analysis and Modification has utilized the Children’s Service for the introduction of techniques for the ob-
servation and modification of behavior by learning principles. Our Biostatistics Section has collaborated with scientists from the Medical Genetics, Internal Medicine and Research Psychology Departments of the Psychiatric Institute.

What of the future? As I see it, the demands for relevance, despite the threat they seem to have made to basic advances, can no longer be dismissed without courting disaster. Since we have combined in one organization both basic and applied approaches, the immediate relevance of at least the applied approach may be able to carry the burden for the rest. Under such an umbrella, basic science can continue to develop, even though its immediate relevance has not yet become apparent (Von Bekesy, 1970).

As the patient population moves back into the community, we must follow suit, or else we will be left high and dry—all alone in our ivory towers.

Investigations of high-risk populations seem to be one answer; for this, new techniques and methods for dealing with patients in the community are a "must." Perhaps mobile laboratories, designed for immediate examining, interviewing, and intervening, are among the ways of the future. The days when the scientist could spend his life in a stationary laboratory may be as far behind us as those when the clinician was able to spend all his days in a state hospital. For his new role in work with high-risk populations, the research man will require a totally new training. What form this training is to take can be worked out only by trial; one of the best places to initiate it is in a cross-disciplinary group, where representatives of the social and biological sciences, as well as of the basic and applied sciences, can voluntarily and spontaneously form ad hoc coalitions for the solution of problems. Out of such collaboration—problem-centered rather than discipline-centered, spontaneous rather than directed—will develop the new researcher, freed from his formerly narrow outlook and ready to tackle the problems of the 70's.