

CASE IDENTIFICATION IN PSYCHIATRIC EPIDEMIOLOGY: METHODS AND PROBLEMS

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THERE are a number of methodological problems which are of concern to the psychiatric epidemiologist. Some of the most difficult of these arise in connection with case finding. Case finding, which is the act of finding those members of a population under study who have the disease or disability with which the investigator is concerned, has two components. One component consists of the means which are employed to locate potential cases; the other component is concerned with the identification of actual cases. The means commonly employed for locating potential cases include community surveys, the use of key informants, reference to social agency records, the use of psychiatric outpatient lists, and the selection from among mental hospital inmate populations. The commonly employed means for identifying cases according to pre-defined criteria of illness include the psychiatric interview, structured interviews, guided interviews, psychological tests and scales, symptom reporting, the diagnosis of physical illness, the designation of those who have assumed the social role of the patient, and ratings of social adjustment.

It is the purpose of the present article to consider the methodological problems in the second component of case finding by evaluating the different criteria which have been employed for case identification in psychiatric epidemiology. The article will conclude by indicating some of the areas where work is needed to establish sound bases for case identification in future studies.

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THE PSYCHIATRIC INTERVIEW

The interview is the main tool of the psychiatrist—the means he uses to arrive at a diagnosis. It is also the ultimate criterion against which other means for identifying psychiatric disorder are validated. The question is, how good a tool is it for the epidemiologist who wishes to identify cases? Can diagnostic judgments be accepted as reliable and valid, or does the psychiatric interview itself present a source of error which can lead to discrepant estimates of prevalence rates for disorder in the population under study?

Reliability is the extent to which an instrument or measure is consistent with itself from one application to the next. Reliability is also measured by the extent to which the same instrument in different hands produces the same results or the extent to which one part of the procedure is consistent (internally) with another part of the same procedure. In the case of the psychiatric interview, the research on its reliability as an “ultimate criterion” yields some disturbing findings.

Ash (1) reports that three psychiatrists working in a clinic could agree on the major diagnostic category on only 45 per cent of the patients who were seen by each. On specific diagnosis, agreement occurred only 20 per cent of the time. Clausen and Kohn (14), studying diagnostic consistency among patients with two or more hospital admissions, found that there was consistency on the major syndrome diagnosis in only 28 per cent of the patients who were diagnosed in first one and then another hospital. Lilienfeld (58), reporting on field interviews, found only a 55 per cent agreement between two interviewers.

Using military data Terris (93), in reviewing selective service statistics presented by Stouffer (91), points out that the range of neuropsychiatric rejections varied from .05 per hundred to 51 per hundred, depending upon the induction station. Stouffer also reported a wide variation in specific diagnosis given to rejected selectees, for example the proportions di-

agnosed as psychoneurotic varied among stations from 2.7 per cent to 90.2 per cent. Navy experience, cited by Hunt and Wittson (42), shows that "differing local diagnostic customs" could account for the fact that at the Great Lakes Naval Station during one month, 30 per cent of "special discharges" were diagnosed as constitutional psychopathic states whereas in Camp Farragut, 60 per cent of the selected discharges were so diagnosed. The diagnosis of psychoneurosis was applied to 24 per cent of the Great Lakes' discharges as opposed to 10 per cent of Farragut's. Two years later only 2 per cent of the Great Lakes special discharges were being diagnosed as psychoneurotic.

In a study of Marine Corps officer candidates assessed by psychiatrists and line officers, Raines and Rohmer (81) reported that different psychiatrists rated the men according to consistently different proportions of favorable and unfavorable, and that different psychiatrists showed consistent individual tendencies to diagnose according to particular dominant personality characteristics. One physician, for instance, most frequently termed men 'anxious,' another 'compulsive,' while another termed them 'schizoid' or 'paranoid.' Examining the psychiatrists as well as the officer candidates, the authors conclude that the personality of the psychiatrist, his emotions, defenses, values, and self-image all contribute to his psychiatric evaluations. This finding is consistent with the work of Hollingshead and Redlich (40), which identifies selective perception and the interest focus of the psychiatrist as factors contributing to differing diagnostic findings. These investigators state, "There are widely varying responses on the part of the physicians to the same varieties of behavior. . . ."

Another study of officer candidates, this time in the British Army (95), required psychiatrists to judge candidates on 14 to 18 traits during a thirty to sixty minute evaluation. High agreement was obtained on pooled judgments (where physicians met together to make or reconcile their ratings) but in general, "the agreement between individuals was not high."

The reliability coefficient for appraisal of a candidate's general suitability as an officer was .65 while the median reliability coefficient on all traits rates was .47. In a mental hospital, Mehlman (66) distributed one group of 597 patients for diagnosis among nine psychiatrists and another group of 1,358 patients among sixteen psychiatrists. He tested for significant differences in diagnostic tendencies among the psychiatrists, and found differences significant at the .001 level between the proportions of patients different diagnosticians assigned to the organic versus the psychogenic categories. There were differences significant at the .01 level between the proportions various psychiatrists diagnosed as manic-depressive versus schizophrenic.

Other studies of reliability report similar findings. Hoch (37) observed that the ratio of diagnoses of manic-depressive psychosis as opposed to schizophrenia on state hospital first admissions reversed itself over a five year period. He attributes the reversal to change in personnel and policy which effected psychiatric diagnostic reliability. Pasamanick and his colleagues (80) report significant differences between diagnostic classification proportions between (similar) wards within a hospital, and notes that diagnoses changed as ward administrators were changed. Leighton (52), reporting on a pre-test in Sterling County, remarks that five psychiatrists read protocols on fifty adults and reached the "almost uniform agreement" that thirty out of fifty were ill. The diagnosticians were equivocal on fifteen persons, and their ratings of which persons were "well" were not at all in agreement. Leighton (54) comments that reliability improved when attempts to diagnose were dropped and raters concentrated only on agreeing about who had symptoms. Hollingshead (38), reviewing these studies, emphasizes the difficulty in achieving a reliable tool for case identification. He also calls attention to the absence of a criterion for validity.

The validity of the diagnostic interview is a separate methodological concern. [Validity is the extent to which an instrument measures what it purports to measure.] To test validity

one must have a criterion against which to test the accuracy of the findings achieved by the technique being examined. In psychiatric diagnosis this requirement poses a serious problem, for by definition the physician's opinion is the diagnosis and by convention, the only standard against which to test it is another physician's opinion. Such a test, if applied, is then ordinarily considered an estimate of reliability, since one is comparing two applications of the same method with one another. It is no wonder that there are but few studies which claim to escape circularity and to test the validity of the psychiatric diagnosis.

Masserman and Carmichael (65) considered it a test of validity to compare an initial, exceedingly thorough work-up of one hundred hospitalized patients against a one-year follow-up which utilized a repeat diagnostic interview, a review of the intervening year's adjustment, the reports of friends and relatives and, when available, social agency data. Such a test compares one diagnostic interview with a second, but assumes that the later one is more accurate by reason of benefit derived from additional information gathered during a one-year period. The authors reported that 41 per cent of the cases required "major revisions in the nosological classification" and conclude either that "the original diagnostic group . . . had been in gross error" or that "the nosological concepts that had been used were themselves of little prognostic, therapeutic or heuristic value."

Another validity testing effort has been made by having several psychiatrists make pooled judgments about the presence or absence of interviewer errors in tape recorded interview sessions.

Brody, Newman and Redlich (9), using this approach, employed tape recordings to monitor psychiatrists engaged in therapy. Their conclusions, which should be capable of generalization to case identification interviews, are that "even the most proficient note-taker misses critical material . . . more important . . . is the influence of conscious and unconscious

screening . . . omissions, distortions, elaborations, condensations and other modifications of the data occur and these all contribute to the difficulty of evaluating what really happened."

Long term observation is another validating technique which can be employed. One example comes from the California Growth Study (27) in which three (psychologist) clinicians worked in close cooperation to study a group of normal children over a seven year period. Throughout their observations they differed "markedly" about the patterns or sets of psychological needs which existed among their subjects. More heartening is the report that they did agree over the years on the ratings of single needs in each child.

If one is willing to test a physician's judgment against the judgment of a layman who has more knowledge of the person being examined than does the psychiatrist, one can compare diagnosis against data which, while relevant, cannot be "diagnostic" in a strict sense. An example of the approach comes from Raines and Rohmer (81), who correlated psychiatrists' ratings of Marine officer candidates with the ratings made by line officers and peers who were well acquainted with the performance of the men. While officers and peers agreed highly with one another, $r = .67$, the psychiatric evaluation correlated .30 with officer ratings, .34 with peer ratings, and .34 with the end-of-observation period status of the candidate. Correlations of this order account for only 9 to 12 per cent of the variance. Another study (6) compared the judgment of five psychiatrists with that of ward attendants rating thirty-one patients on twenty-three operationally defined characteristics. Average per cent of agreement was 52. There was no agreement on any characteristic that achieved statistical significance.

Another test of validity may be sought in predictions from psychiatric evaluation to performance rated on specific variables. Two major selection studies illustrate the hazards of this approach. One was the assessment program undertaken for the Office of Strategic Services during World War II (75) in which attempts were made to select men suited for various

OSS assignments. Psychiatric interviews were augmented by a variety of other ingenious assessment devices. When compared with later job performance, there was no indication that the predictions made had been correct. A second major study was undertaken by the Veterans Administration (49) in an attempt to select clinical psychology graduate students. Clinical interviews (usually by psychologists) were combined with objective, projective, quantitative and other varieties of clinical data. Except for two written tests, neither any single diagnostic method nor any combination of methods could predict student success in academic, research or clinical areas.

The results of these few efforts to test or to infer the validity of psychiatric diagnosis cannot be said to have demonstrated the inadequacy of that device for case identification, for in each of the foregoing studies there are questions about research design, theoretical assumptions, or inferences from data which prevent them from being crucial tests. Nevertheless, there is sufficient evidence to show that the sources of interviewer error are many (43) (48) (81) (39) and that unless these are carefully controlled the psychiatric interview can prove to be a very unreliable and inadequate tool for epidemiological use. As Terris has commented, (93, 223) “. . . the whole problem of validation has to be studied and explored, and . . . psychiatric procedures, definitions and diagnostic criteria have to be standardized on a scientific basis. Unless this is done, I am afraid that we can expect little but confusion from epidemiological studies . . . of mental disorders.”

In spite of the serious methodological questions, in practice the psychiatric evaluation remains the primary means for making judgments for the purposes of case identification. It will probably continue as the ultimate criteria either until a more reliable, demonstrably valid and practical alternative is developed, or until confidence wanes in the value of medically oriented investigations into those kinds of human behavior which, when labeled as psychiatric disorder, are now considered to fall within the medical domain.

In the meantime the investigator who plans to use the psychiatric interview either as the primary case identification device or as the one against which other field methods are tested will probably wish to harken to the discussion of the control of interviewer error to be found in Hyman (43) and in Kahn and Cannell (48). He will also wish to follow Hollingshead's (39) suggestions that there be careful planning, operational definitions, training of the field staff, and pilot studies. He will be well advised to listen to Gruenberg's (71) argument that one should use investigators who are specially trained to obtain consistent data according to a previously specified plan. Whenever possible pooled ratings should be employed.

OTHER CASE FINDING METHODS

There are alternatives to psychiatric diagnosis which have already been employed for case identification; however, most of these have relied upon a psychiatric opinion as the final criterion. Among the alternatives are many which are cheaper, faster, and more easily standardized and quantifiable. These alternatives each raise problems of their own; especially the crucial continuing questions of reliable and valid measurement in population samples.

Structured Interviews. The structured interview employs a format (schedule) of written questions which a trained non-psychiatrist interviewer is usually employed to administer. Except for probing inquiries to open-ended or unanswered questions, (the probes are themselves designated in advance) the interviewer is not allowed to deviate from the questions or the order of the questions printed on the schedule. The interviewer may be allowed to append additional observations or comments which he thinks are relevant. The structured interview has been widely employed in epidemiological work; for example, in the studies of Rennie (83), Gurin (34), and Leighton (53) (56).

The content of the questions asked varies depending upon the definitions of disorder employed by the investigator. Ques-

tions about happiness, optimism, satisfactions, worries, emotional problems, relations with others, adjustments to work, activities, nervousness, and past medical treatment are common.

Typically, there are two ways of handling the replies to interviews structured for the purposes of case identification. One is to consider reports of, for example, self-dissatisfaction as direct evidence of a mental problem *per se*. One needs only to count affirmative replies to the criteria questions in order to identify cases. The other way of handling the data calls for interviews to be interpreted by a psychiatrist who makes a diagnostic judgment by abstracting and inferring from the replies which respondents have given to the interviewer in the field. In this method the psychiatrist identifies cases he has not seen.

Both approaches entail problems. In either, one is always faced with the question of awareness on the part of the respondent: how able is he to report data accurately about his own behavior and feelings? Secondly, who among the respondents may be concealing information and to what extent? Then, regarding the use of replies as *prima facie* evidence of mental disorder, how has this assumption been validated? As Inkeles (45) has found, life miseries and dissatisfactions are part and parcel of low status in industrial societies the world over. Can these nearly universal responses to living be used as criteria of mental illness? Obviously the use of direct questions and the counting of misery-betokening replies as evidence of disorder leads immediately into conflicts over the definitions employed, the core of the criterion problem.

What if one takes the interview as raw data and subjects it to the refining process of interpretation and once-removed diagnosis by psychiatrists? What happens? In one study by Pasamanick *et al.*, (80), psychiatrists reviewed reports made by physicians who examined a sample of patients. The number of psychiatric diagnoses made led to an estimated rate of 100 cases of psychiatric disorder per thousand population. In Rennie's Midtown study (83), psychiatrists reviewed data supplied by trained field interviewers who used a questionnaire

which contained questions of psychiatric significance. The rate arrived at for the prevalence of mental disorder was 750 per thousand.

Both teams of psychiatrists reviewed original structured interview data, but came to widely divergent findings. Part of the reason is that the two studies employed widely differing criteria for judging whether or not psychiatric disorder was present. In addition there was considerable difference in the populations under study, in the setting in which interviews took place, in the orientation and training of the interviewers, and in other aspects of methodology. Reliability problems may also have existed among and between the two groups of psychiatrists. It is also probable that as the number of steps within a system increase, the chances for other error increase as well. In any event, two epidemiological studies which employed psychiatrists to evaluate structured interview data came to widely differing conclusions about the prevalence rates for disorder in their populations, differences that cannot be readily explained on the basis of the dissimilarity of the two populations under study.

In spite of the difficulties, the structured interview is a valuable device. It is widely employed in epidemiological work, for non-psychiatric interviewers can be trained to work effectively and relatively cheaply in the field. Interviewers can be selected and trained for high reliability and carefully conducted studies can achieve remarkable control over interviewer error (43). The disadvantage of the structured interview method itself is in its inflexibility. Because it is structured it prevents the interviewer from changing his approach, his question order or his probing of interests in order to meet the changing requirements of differing settings or of differing needs, moods, or personalities of the respondents. In achieving high reliability by this means, one may sacrifice rapport, judgment and significant psychiatric data.

In regard to case identification from structured interview data, when the criterion is the presence of affirmative replies

to misery-laden items (for example work-dissatisfaction, feelings of inadequacy, nervousness, unhappiness, worries, etc.), which are counted in a straightforward manner and taken at face value as identifying a psychiatric case, there is likely to be overestimation of prevalence, i.e., false positives in the identification of illness. On the other hand, if structured interview data is submitted to psychiatrists for evaluation in order to identify cases, one is faced with the unreliability of psychiatric judgments plus the additional errors which may be introduced by the processes of inference and second-order evaluation from limited original data. Unless very carefully handled, the results from the latter are not likely to inspire confidence.

The Guided Interview. The guided interview differs from the structured one in that the interviewer is given greater freedom to make inquiries and observations within given areas or topics set forth on an interview schedule or guide. The Syracuse survey of older people, by Gruenberg *et al.*, is an illustration of this usage (71) (72). Intensively trained interviewers employed a schedule which had questions for the interviewer to answer about the respondent in specified areas of mental functioning, past history, and present situation. The interviewers "were left free to obtain the desired information by phrasing questions and raising issues in whatever seemed the most appropriate way to each interviewer. . . ." Interviewers were trained to elicit and record psychiatric signs and symptoms, to record observations and to make ratings. Three professionals, one a psychiatrist, then read the protocols to arrive at case-identifying decisions.

The advantage of the guided interview (the Syracuse investigators term it an "unstructured" one) is, in its obvious flexibility and sensitivity, in its encouragement of spontaneity in the respondent, and in the fact that the pursuit of an item of information is not considered important enough to jeopardize the rapport between interviewer and respondent. In contrast to psychiatric interviews which can be made unreliable by differences in training and outlook among the physicians,

the guided interview allows the training of interviewers "to obtain consistent data according to a previously specified plan" (71).

The disadvantages of the guided interview were clearly seen by the Syracuse investigators. These include decreased interviewer reliability, uncertain instrument validity, and the necessity for exceptionally careful attention to the selection, training and field supervision of workers, for the common survey problems of interviewer error and bias do loom larger as the degree of structure in the interview schedule is reduced.

As in every study, validity will depend upon the criteria employed. In the Syracuse study, a psychiatrist reinterviewing a small segment of the respondents constituted one test of validity. While the figures are not reported, it was observed that the guided interview tended to result in under-reporting of disorder. As far as the reliability of the ratings for case identification is concerned, the procedure called for the three raters independently to evaluate the completed protocols by classifying cases on a three-point scale of "certifiable" (to a state mental hospital), "not certifiable" and "unknown." There was agreement (concordance) on 1,387 ratings and disagreement on 205. Inter-rater reliability of 87 per cent as achieved here is highly satisfactory and demonstrates that the guided interview can provide sufficient information to allow trained raters to achieve high reliability in the identification of cases.

Psychological Tests. Psychological tests have been used as screening devices to identify cases. The Cornell Medical Index (8) and its modification (74), the Army Neuropsychiatric Screening Adjunct (40) and its modification (74), the Guttman-type scale of Wilner and Walkley (96), the Hildreth Feeling and Attitude Scale (35) (41) (44) and the HOS developed by MacMillan (62) (63) are examples. Tests can be constructed to achieve high reliability, are easily standardized and made to yield quantifiable scores, and are easy to administer. Their validity depends upon the criteria against which they are developed. If an unreliable criterion like psychiatric

diagnosis is employed, test validity may be difficult to establish (86). That psychological test validity may be discouragingly low in the matter of personality description and subsequent case identification has been shown by the important study of Little and Schneidman (59).

Besides the screening tests there are a variety of other paper-and-pencil and projective tests which might be employed in epidemiology. However, many are elaborate and require special conditions to insure respondent cooperation and comparability of testing situations, conditions which cannot be assured in the usual community study. Sanford (85), Scott (87) and others have developed simple projective tests for use in attitude surveys; as a prototype, they suggest a kind of test which might be expanded to meet certain epidemiological requirements where sensitive, indirect tests seem appropriate.

Rating Scales. Ratings scales are devices which are related both to psychological tests and to interview judgments. Usually constructed so as to call for designating where a person stands on a categorized continuum with regard to one trait, quality, or kind of behavior, they can be divided into two major types. In one, an interviewer observes a respondent and makes a judgment which is recorded on a scale. In the other type, called self-ratings, the respondent answers a question or set of directions by ranking himself, his feelings, attitudes, etc., in one category or interval of a scale.

Illustrations of the use of rating scales relevant to personality or adjustment description for case finding can be found in the excellent work of Glidewell, Domke, *et al* in St. Louis (29) (30), the Barrabee (3) Social Adjustment Scale, Lorr's (60) (61) behavior rating scales, and the Ineffectiveness and Discomfort Scales of Frank, Parloff, Kelman, and Imber (25) (50) (77). Generally speaking, rating scales have been more effective in the evaluation of personality change than in case identification in epidemiology; primarily because of the intensive training needed to achieve inter-rater reliability (for scales where an observer makes judgments) and because of

the error which can occur in the field work coding of abstractions with the possible loss of concrete referents. As Jahoda (46) and others point out, even when one obtains reliable ratings in a given situation, there is still a validity problem; for there is no abundance of evidence to demonstrate that ratings from one specific situation can be generalized to other behavior or to abstractions of nosology. Consequently, interviewer ratings of specific respondent behavior may not prove valid for case identification purposes.

Self-ratings such as the Q Sort, adjective check lists, or the Discomfort Scale (which is derived from the Cornell Index) have been successfully employed in treatment evaluation studies (50) (60) (68) (77) (84) (90), but often pose problems in epidemiology surveys because they require respondent literacy, understanding, and cooperation. Whereas one can secure these latter among relatively "tame" therapy populations, they cannot always be assured in community studies. Nevertheless, short, simple and concrete self-ratings scales which refer to common human experiences or complaints have been used in the national mental health study of Gurin *et al.* (34). Morris (70) and Cartwright (12) have employed them in morbidity studies within communities.

Reliability can be achieved for self-ratings over short periods of time and for observer ratings by careful interviewer training and control of observation situations. The validity problems of self and observer ratings are many, but given care in their development, it is likely that ratings scales will be of great use to the epidemiologist.

Symptom Reporting. The Expert Committee of the World Health Organization (22) in discussing desirable case finding methods states, "The Committee . . . considers that it may . . . be fruitful to base a classification . . . on simple and non-ambiguous clinical syndromes . . . or even single abnormal behavior patterns (page 9) . . . the most standardizable, countable, and comparable units of observation . . . appear to be symptoms." (P. 16) The Committee's view is widely shared.

Symptoms or complaints have been used as methods for identifying psychiatric disorder in studies by Leighton *et al* (53) (56), Rennie *et al.* (83), Gurin *et al.* (34), Frank (25), Glidewell and Gildea *et al.* (29), Miller (68), Kelman and Parloff (50), and others working in psychiatric epidemiology and evaluation. Morbidity studies of physical health have frequently employed symptom reporting.

Symptom reporting may also be employed without making inferences as to the order of association with the higher order abstractions of psychiatric diagnosis. As Gruenberg has indicated (33), one may wish to study the distribution of symptoms *per se* among various population groups selected to test hypotheses regarding the correlation between symptoms and various conditions, or between a given diagnosis and symptoms. One may also find, as Gruenberg did that symptom counts of presumably discrete symptoms may, upon the re-grouping of data, reveal syndromes related to other variables under study. Consequently, symptom counts may be useful in etiological studies, in studies of illness classification and syndrome re-definition, and as hypothesized variables in other epidemiological concerns as well as in their use in case finding as direct measures of psychiatric illness.

Symptoms as criteria for case identification present difficulties as well as advantages to the psychiatric epidemiologist. In their study, Gurin *et al.* (34), praise the simplicity and acknowledge face validity for symptom lists, but point out that "the lack of a developed conceptual and theoretical framework for this technique limits its appeal." An example of difficulty is found when one compares Gurin's finding, based on the use of a symptom list which contained questions culled from Leighton's Sterling County study (using MacMillan's HOS) and from Rennie's Midtown questionnaire. These items were ones which had differentiated respondents diagnosed by psychiatrists as having psychological difficulty from respondents not so diagnosed. By factor analysis Gurin and his colleagues found that one cluster of these case identifying items

centered about physical disorders. Examining their data they found that physical symptoms increased as education decreased (and increased with age). Since low education is correlated with low socio-economic class, by implication one would expect to find high prevalence of psychiatric-illness-correlated physical symptoms among poorer people. Yet when one turns to the Pasamanick study (80) based on physician's examinations and psychiatric inference from the physician's reports, one finds that "in the psychophysiologic, autonomic and visceral disorder, prevalence increases directly with income." (P. 191). Gurin's suggestion that people with less education react to problems with physical symptoms whereas better educated (and correlatively higher in income) show more stress in a psychological manner (anxiety, etc.) is perfectly reasonable, but not compatible with Pasamanick's findings.

The discrepancy points up a problem in symptom reporting as opposed to physical examination findings. Symptoms are subjective complaints which do not necessarily coincide with objective signs or the final clinical opinion of the examining physician. The work of Trussell (94) in New Jersey is an illustration; there only 22 per cent of the respondents reported conditions which the medical examiners found to be present.

There are a number of variables which influence symptom reporting and which thereby can contribute to its low validity for case identification when compared to medical examination findings. These include cultural-ethnic factors (97) and social class (51) (45). In addition, Cartwright has shown that survey procedures themselves influence symptom reporting, for the wording of questions, the time span asked about, and the categories employed in coding all make a difference in reported morbidity (12). To illustrate, Cartwright has shown how one method of asking morbidity questions using symptom reporting led to a rate of headaches, for males, of 65 per thousand. Another survey method using differently worded symptom questions asked of the same population led to a rate of headaches, for males, of 168 per thousand.

An objection can be made to the direct count of symptoms as a means for case identification. The objection is akin to that made against the use of direct tabulations of "misery" response to structured interview items when these are employed as *prima facie* evidence of disorder. In both cases the rate of cases identified will be quite high; furthermore, it is totally dependent upon the presumed accuracy of the respondent's awareness and reporting and is, in fact, usually implicitly assumed to reflect some other significant but unstated aspects of the respondent's life or function. Jahoda (46) and many others (92) (88) reject that utility of the symptoms as a criterion for case identification. Jahoda states that "behavior cannot be viewed in terms of isolated symptoms, but must rather be viewed in conjunction with the social norms and values of the community in which the symptoms are observed." One suspects that Jahoda means either to say "signs" or to discuss "symptom reporting" rather than "observation." Nevertheless hers is a reasonable request for consideration of genotypes and broader concepts of personality and social function, in opposition to the phenotypes and restricted foci which are symptoms. These latter may prove misleading in case identification for, as Barron's research concludes (4), "psychopathology is always with us, and . . . (balance and maturity in interpersonal relations) is a way of reacting to problems, not an absence of them." Gurin (34) states about the limitations of symptom indices, "individuals have great capacities for isolating different aspects of malfunctioning."

For the epidemiologist symptom reporting is one of the simplest means for case identification. Methodologically its limitations arise from the evidence that willingness or ability to report symptoms varies with the social and biological characteristics of respondents. Furthermore, it is likely that the reliability of symptom reports will be adversely affected by time (69) and by changes in circumstance (11) (12). Nevertheless the validity of symptom reporting for case identification of "neurotics" in a homogenous population has been achieved

by MacMillan (62) with a reported agreement of 86 per cent between the HOS and a psychiatrist. The finding shows that carefully constructed symptom lists applied to homogenous groups can be a valid as well as simple tool for epidemiology.

Physical Illness. While symptom lists used in psychiatric case finding presumably are focused on conditions which are considered to reflect psychiatric disease, the symptom lists which have been used sometimes contain items relating to somatic illness. For example, Gurin's (1) factor analysis of his modified HOS-Rennie symptom scale results in four clusters of factors: psychological anxiety, physical health, immobilization, and physical anxiety. The Cornell Medical Index has predominantly somatic items, yet a high positive score on these independent of or combined with the emotional items, has been reported to be correlated with psychiatric illness (8). Similarly, other investigators report that symptoms of physical illness are associated with positive psychiatric diagnosis.

There may be several reasons for the association. Malmö (64), Dunbar (20) and others speak of the somatization of emotional distress leading to physical symptoms. Freedman and Hollingshead (26) sound a different note, suggesting that the self-diagnosis which leads a person to seek psychiatric treatment can occur when one's body hurts or functions poorly. Others like Simon (89), Zwerling and Titchner (98), and Janis (47) have shown that the fact of physical illness itself leads to emotional distress and diagnosable psychiatric disorder. Reid (82), Downes and Simon (19) and Buck (10) suggest that somatic ills and psychiatric ones, for example cardio-vascular disease and neurosis, are correlated by virtue of a common origin or cause. Hinkle and Wolff (36) suggest that all illness may be associated with periods of life stress, and that one may not need to differentiate between physical and emotional disorders in showing the susceptibility to malfunction of one or another body system in response to difficulties in adapting to threatening life circumstances.

All these workers have focused on physical symptoms in psychiatric research: some use symptom scales with physical illness items as presumptive indices of psychiatric disorder. One must conclude that physical symptoms are in fact used as criteria for identifying psychiatric cases. It is done without any necessary statement of the nature of the presumed or anticipated relationship, and without any large body of data describing the patterns of relations between symptoms of physical distress and other criteria for identifying psychiatric illness. For the epidemiologist, the potential utility of physical symptoms for use in case identification is an exciting area for both practical and theoretical investigation.

Being Ill. While not rationally employed as a case finding method *per se*, the means through which nearly all psychiatric cases have come to be recorded by clinics, and to some extent by hospitals, has been as a result of the action of the "cases" themselves. In other words, psychiatric case finding based on clinic and institution records has been dependent upon the self-selection and self-diagnosis (or family diagnosis) of the need for psychiatric care. Sociologically speaking, these are people who have been willing to call themselves "sick" and to seek the role of the patient *vis a vis* a medical healer (74). Sometimes becoming a patient involves people other than the patient; family and friends, and even police or employers may decide that a person is sick and should be treated accordingly. As Felix and Bowers (24) observed, it is the community that sets the criteria for case finding. Only in the final step in the social process of becoming a patient does a psychiatrist and perhaps an institution enter in, usually to concur in the lay judgment and to assign a diagnostic tag in accordance with the formal patient role.

Occasionally it is difficult for the person to accept the patient role, even if family and physician have already called him a case. Sometimes convincing the reluctant case to be a patient takes time; when he finally "recognizes he has a problem," i.e., succumbs to the patient role, it is generally thought

that therapeutic progress has been made. Looked at another way it has been a process of persuasive role assignment and final role acceptance leading to self-identification by the person of himself as a psychiatric case.

In the past, becoming a patient has been the primary case finding method for psychiatry; it must not be overlooked in planning future community studies. The bulk of outpatients are people who have decided for themselves long before any professional diagnosis that they "have a problem," "need some help," etc. With institutionalized persons the self-diagnosis may have been a less important stepping stone to case identification, but the community diagnosis of "crazy" has usually been made before professionals entered the picture. The point to be made is that most psychiatric research to date based on clinical cases and most treatment methods as well have been using specially selected populations where the crucial tool in case finding has been self, family or community diagnosis based on folk notions and filtered medical ones about what constitutes psychiatric illness and what should be done about it.

The basis and result of lay diagnostic process may not agree with medical criteria for case finding as the work of Cummings (18), and Clausen and Yarrow (15) has shown. An illustration of the problems posed comes from Cartwright (12) who shows that once a lay diagnosis has been made, symptom reporting may be altered. For example, respondents "when asked whether they had . . . painful or swollen joints, some of the people who said 'no' later reported (they had) rheumatism . . . once a complaint has been diagnosed and labeled medically it ceases to be thought of as a symptom. People fail to recognize their rheumatism in a question about swollen and painful joints." Demonstrating the social class determinants of self-diagnosis, Koos (51) found that 57 per cent of the middle class respondents interviewed said loss of appetite deserved medical attention, but only 22 per cent of the lower class respondents concurred. Generally, Koos' findings are to the effect that the lower the respondent's socio-economic

standing, the less agreement there will be between his criteria for defining illness and those of the physician.

On the other hand, it is likely that the psychiatrist will concur in nearly every case with the self or community diagnostic criteria, for one suspects that the incidence of persons who come to the psychiatrist seeking treatment but who are turned away with a diagnosis of "no illness present" represents but a small fraction of those who apply for care. Unless folk criteria are more perfect than the evidence would lead us to believe, it would appear that the psychiatrist's self-referred case finding encompasses a variety of folk criteria for the identification of mental disorder.

Of all the case finding means, the self-diagnosis and help-seeking processes are the most complex. Most are beyond experimental control as research methods. They are research concerns instead. Yet, as criteria for case definition, the lay diagnosis and the subsequent exposure to psychiatric care must not be ignored. Future studies which deal with case finding criteria must attend to the person's own self-descriptions, his illness definitions, and his help-seeking endeavors.

Social Adjustment. Various kinds of social adjustment have been used either as criteria for defining a psychiatric case or as measures of impairment due to psychiatric disease. Freedman and Hollingshead (26), examining overt interpersonal difficulties among diagnosed neurotics, found that these may be categorized conveniently in terms of family adjustment, peer group and community interaction, work relations, and legal difficulties. Ginsberg (28) contends that the ability to work, have a family, be lawful and to enjoy leisure are the adequate criteria for mental health. Except for the addition of being "lawful" these repeat Freud's requirements for the capacity to work, love, and play.

Emphasis upon work as a criterion has been given by Hinkle and Wolff (36) who state that "impairment as manifested by absence from work is the most important operational definition for illness frequency and extent of disability." The

Expert Committee of the World Health Organization commends work absence as a case finding method, as well as the use of police and social service agency records. Social maladjustment is a condition of case definition according to the WHO Committee, who write, "The Committee suggests that a 'case' be defined as a manifest disturbance of mental functioning specific enough in clinical character to be consistently recognizable as conforming to a clearly defined standard pattern *and* severe enough to cause loss of working or social capacity, or both, to a degree which can be specified in terms of absence from work or the taking of legal or other social action." (22)

The WHO definition requires that manifest disturbance in mental functioning be accompanied by obvious social maladjustment. Neither social malfunctioning nor mental disturbance alone are considered sufficient for case identification. One can infer that a syndrome is assumed by the Committee in which the two go hand in hand.

The WHO requirement for case definition would automatically exclude large numbers of persons who are now considered psychiatric cases. It would exclude individuals who suffer clinically observable distress but who do not demonstrate the loss of working or social capacity, e.g. many psychoanalytic and psychotherapy patients. It would also exclude many individuals with obvious social impairment, e.g. narcotics use, adult homosexuality, who, except for their social conduct, may not demonstrate conventional psychopathology, but in whom psychiatrists have interests sufficient to justify their identification as cases.

Reference to available data supports the contention that the use of social maladjustment as a prerequisite for the diagnosis of psychiatric disorder would exclude many individuals now considered as cases. Turning to Freedman and Hollingshead's data (1) (26), one finds that subjective dissatisfaction or discomfort is the phenomenon most often characterizing diagnosed neurotics, followed second in frequency by somatic

complaints (functional). Social maladjustment is distinctly a class phenomenon in neurotics; 20 per cent of the lower class neurotics were in trouble with the law while only 5 per cent of the upper class neurotics were. Even so, subjective distress is more common in all classes, with an average of 90 per cent of the total (all classes) experiencing subjective distress, with only 9 per cent of the total in trouble with the law. Combining all forms of overt interpersonal difficulty (family, peers, community, work, legal) one finds among Freedman's neurotics that only 25 per cent showed any kind of overt troubles whereas only 11.2 per cent of the total diagnosed neurotics fail to show subjective distress.

If one begins with individuals who have been diagnosed as psychotic, one will find that some are rated as without obvious social maladjustment. Ødegaard's data are relevant here (73). Among Norwegians who were not hospitalized for a period of years although they were psychotic, 17 per cent were rated as having good social adjustments. As for work, 16 per cent were self-supporting and 26 per cent had part-time jobs.

If one reverses the approach and takes a group with known social troubles, the frequency of labeled psychiatric disorder among them will probably be much higher, depending upon their access to and the viewpoint of the diagnostician. Often the fact of social maladjustment is criterion enough for a diagnosis. That it is not an agreed-upon criterion is shown in the comments of Albert Cohen (16) writing about social disorganization and deviant behavior; "The most pressing problem in the (sociological) field . . . is to define these terms. The major task is to get rid of the notion . . . that the deviant, the abnormal, the pathological and, in general, the deplorable, always come wrapped in a single package."

Let us give instances of psychiatric diagnosis applied to people with social maladjustment. Cowgill in Wichita (17) reported 33 per cent of an indigent group on welfare were psychiatric cases; on the other hand Gotcher in San Mateo (31) estimates that 50 per cent to 60 per cent of the indigent

welfare cases have mental disorders. In a prison population Barnes and Teeters (2) citing work of Glueck, Banay and others, state that 25 per cent of the prison population have mental disorders, with an estimated 1 per cent psychotic. Overholzer (76) found 15 per cent of a sample of criminal defendants to have disorder; with psychosis in 6 per cent. Yet at San Quentin Prison (7) 10–11 per cent are judged psychotic and 100 per cent of the inmate population are diagnosed as having some form of mental disorder. While the 100 per cent rate of disorder at San Quentin is achieved through psychiatric evaluations in which the diagnostic criteria are not specified, one can argue that being in prison constitutes a sufficient criterion for case identification at San Quentin.

Some psychiatrists (WHO) require social maladjustment as a prerequisite for case identification, but not as the only prerequisite. Others use the fact of social maladjustment as the sole criterion for making a psychiatric diagnosis (57), while a third group will identify as cases persons with clinical symptoms but who have no remarkable social maladjustments, at least in comparison with community norms or the ratings of others (21). Given these disagreements about definition and relationships, it would be valuable if future research were to investigate the relationships between particular forms of social maladjustment and other criteria for case identification.

Concepts of Mental Illness. The foregoing discussion has led, inevitably, to the problems of orientation and belief which underline certain of the methodological difficulties encountered in case finding. These have been subject to increasing scrutiny and evaluation by scholars in recent years; workers who have been concerned with the values and concepts inherent in definitions of mental illness and mental health.

There is considerable agreement that "mental illness" is not a unitary concept, nor are its components well delineated; further the diverse phenomena ordered under "mental illness" are themselves capable of multiple determination. We are not dealing with *a* disease or *a* cause. No wonder that WHO's

Expert Committee on Mental Health wrote, "In epidemiological studies, the definition of a 'case' is of crucial importance . . . however, there seems to be little prospect of producing a definition which would cover all the major and minor aberrations in social behavior or manifestations of disordered thought. . . ." (22) (P. 15). Gottlieb and Howell have observed (32), "The success of public health measures has been in large part dependent first upon the identification of specific important etiological agents. . . . For mental illness, unfortunately, we cannot isolate a single variable . . . but must be prepared to deal with multiple factors." Leighton, Clausen and Wilson (55) comment that whatever criteria one may employ, it may be unwise to consider these criteria as the illness itself or to infer cause. Further, "It is hazardous to propose a single set of objective standards against which health may be assessed." (P. 403)

Ewalt (23), summarizing Jahoda's monograph, writes (page XI) "No completely acceptable, all-inclusive concept exists for physical health or physical illness, and likewise, none exists for mental health or mental illness. A national program against mental illness and for mental health does not depend on acceptance of a single definition and need not await it." Nor, we would add, should research on case finding for epidemiological purposes await such agreement.

Szasz (92) prefers not to attempt to define, but rather to cast out the notion of mental illness; the better then to face the neurological diseases of the brain and the psychosocial, ethical, and legal deviations which constitute those "problems in living" which, he contends, are the subject matter for psychiatric concern. Scott (86) would dispute him, denying that anyone is as yet agreed that deviation from social norms is a prerequisite for disorder.

Jahoda's 1958 monograph (46) elucidates many of the issues, but alas, elucidation does not assure agreement, either. While her emphasis is upon conceptions of health, her analysis deals with illness, too. She contends that community values

are crucial to definitions, and that the notions or statistics of normality per se should be no criterion. She also rejects as criteria unhappiness, lack of well-being, and self-satisfactions; contending that these may be appropriate responses to life situations; that personal reactions cannot be divorced from social conditions.

Along with Smith (88) and others, Jahoda demands a multiple criterion approach, introducing her argument by referring to Merton's (67) explication of the fallacy involved in classifying enormously varied conditions and processes under the single heading of disease, and then attempting to evolve a theory to account for the diversities observed. Jahoda states that there can be various types of mental health, that a man can be healthy in one way and not in another. He can be healthy in terms of his enduring attributes but not in terms of short term actions, or vice versa.

As far as the values of mental health are concerned, she cautions that mental health is one goal among many and that probably the most acceptable value is that "the individual should be able to stand on his own two feet without making undue demands or impositions on others."

One further point needs to be made; it is both a warning and a hope. Case finding not only depends upon reliable methods, criteria studies, and explication of assumptions about values and orientation; it implies a certain appropriateness of classification based on hopes for treatability. Given what is now known about psychiatric treatment efficacy, one would certainly have to be restrictive if one limited the definition of cases to those persons who were considered good risks for treatment success using present methods. Excluding certain treatable organic conditions such as phenylpyruvic amentia, one would attend primarily to the better educated, more intelligent, non-institutionalized people whose illness had been rather acute, of short duration, and as mild as possible. Paradoxically one would end up with cases who might be considered, compared with other criteria, as least sick of all.

Yet if one extends the definition of a case to people with disorders which at present do not have a high incidence of successful treatment, e.g. character disorders, schizophrenia, one does assume that the identified cases should come within the scope of traditional psychiatry. Whatever one *hopes* for the discovery of etiology and consequent treatment and prevention, as well as recognizing one's already shouldered burden of medical responsibility for such cases, the inclusion of traditional groups of psychiatric patients in the definition of psychiatric disorder does continue to commit community psychiatry to the provision of service to large groups of people for whom there is little demonstrable evidence of curability; to a group for whom there is no real promise that prevention or cure will be found within the scope and methods of medicine and its associated professions, however broadly medicine's limits are conceived.

This is not to say that diagnostic groups for whom there is low probability of treatment success should not be considered grist for the psychiatric mill nor should they be excluded from case finding efforts. Treatability is just one of the many considerations an investigator must attend to when he begins to set up his criterion and subsequent case finding methods in the light of his study's aims. The fact remains, as Gruenberg has stressed (33), that *case finding methods must always be related to the objectives of the particular study*. If one is seeking to find treatable cases in a community one will establish criteria quite different from those to be employed in a study which sought to identify the persons in a community for whom, by virtue of their diagnosis, psychiatric services have traditionally shouldered responsibility. Indeed an epidemiological research aim which seeks to identify only poor treatment probability cases is conceivable; for example, to determine differential characteristics or to set up evaluation programs using experimental therapies.

In regard to presently untreatable groups one must be forewarned that the future may demonstrate that there are no

cures available within the compass of psychiatric medicine; either because science itself is of no avail, or because prevention but not treatment may prove possible, or because effective treatment may be outside the domain of conventional psychiatry. In the latter event one may wish to redefine the boundaries of the healing professions rather than to exclude particular disorders from our concern. To be both speculative and specific one may find oneself dealing with causes and cures which are economic, social, political or moral or, at the other extreme, surgical, endocrinal or genetic.

In the meantime since the conditions which are currently found under the heading of "mental disease" are multiple, the choice of case finding devices and selection criteria must always be highly selective. Research on case finding will in itself be a means to redefinition and criterion correction as evidence on etiology, nosology, and treatability is generated.

Some Research Needs. Jahoda (46) outlines the research problems which she considers particularly relevant to the current status of conceptions of mental health and illness. Her suggestions are consistent with those that others have made and are based on the problems and considerations which have been outlined in the preceding pages. They are certainly relevant to a concern with the advancement of epidemiological work through case finding criteria. Observing the lack of agreement on definitions and the lack of knowledge of the relationships of the non-unitary multiple components which constitute mental illness and health she says, "*The most urgently needed study is one of the interrelationship of criteria.*"

She anticipates, no doubt correctly, that relations among criteria will vary depending upon the social and biological characteristics of the groups studied. Among the groups which require study are psychiatric patients themselves. With them she says, "Perhaps most immediately needed . . . are descriptions of the degree to which various mental health criteria co-exist with various types of disturbance."

Expanding Jahoda's suggestions, Smith (88) calls for par-

ticular attention to be given to basic research in personality structure, with particular reference to the stability, resilience and viability of the external or adaptive aspects of personality and of its internal processes. Along with Clausen he (13) says one must distinguish between positive mental health values which are concerned with human development and function and those aspects of the organism which are specific to resistance to illness under stress. Scott (86), summarizing the conceptual problems for mental health research asks, "Does mental illness refer to a unitary concept or to an artificial grouping of basically different specific disorders?" Is it "an acute or chronic state of the organism?" "Is maladjustment (or deviance from social norms) an essential concomitant of mental illness?" And, "Should illness be explicitly defined according to values other than social conformity? Scott expects the answers to depend partly on theoretical predilections and partly on the outcome of future empirical research.

Where to From Here? Most mental health research persons and most public health administrators would concur in the statement that epidemiological studies and community work in psychiatry are promising activities. Most would probably agree that at present both suffer limitations; epidemiology limited by uncertain or inadequate methods, and community service limited by an insufficiency of information of the kind which must be gained through epidemiological work. Paramount among the method problems in epidemiology is that of case identifying criteria, and, assuming multiple criteria, the interrelationships and predictive capabilities found therein. Once criteria are set, one needs assurance that the criteria measures themselves have known characteristics of reliability, applicability, and validity with relationship to the criteria.

The obvious need is for studies of the case finding instruments themselves. Should present instruments, or combinations of them be shown to be inadequate as standards, new devices must be developed. But before we attempt to find new tools, we must know more about the characteristics of the ones

now available. These are the psychiatric interview, psychological screening tests and scales, the structured interview, symptom reports, reports of physical illness and disability, patient role status, and ratings of social adjustment to family, peers, work and community.

For each of these we must learn more about its reliability, its sensitivity to change, its stability over time, and the feasibility of practical application to each of the biological and social groups which one would expect to encounter in a population of interest. Within and among these groups the distribution of findings, whether ratings, scores, or direct responses, must be known so that each measure can be examined in relationship to every other measure, for only in this way can we specify the interrelationship among our criteria.

In addition, we should be concerned with other methodological matters; with the interaction effects of major biological and social characteristics on the findings of a given instrument; with the consistent patterns of findings from several instruments which may isolate "natural" disability groups having syndromes in common, and with a study of the factors which reflect common response tendencies to items within one instrument or among several instruments. With these findings in mind we would then wish to attend to the modification and combination of instruments so as to reduce item duplication, speed application, and increase their power of detection.

As Reid has said in his recent review (82) (p. 69), "Whatever the trouble involved, experience in other fields of epidemiological work strongly affirms the need for . . . standardization of the precise diagnostic criteria used." Once our methods inspire greater confidence, one can proceed to their application to populations; putting epidemiology to use in a variety of excellent ways.

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