SOCIAL ASPECTS OF PSYCHIATRY: THE IMPORTANCE OF STATISTICS.*

By L. S. PENROSE, M.A., M.D.
Galton Professor of Eugenics, London University.

THE social impact of mental illness, like that of other diseases which are covered by public health organizations, is governed by quantitative considerations. Two cases of typhoid fever are worse than one, and the same applies to schizophrenia. A principle, complementary to Bentham's doctrine of the greatest good for the greatest number, is the foundation of mental hygiene. The object is to try to reduce the total quantity of discomfort in the community due to mental illness. The effort is not expended only on people recognized to be ill enough to need hospital care. It includes those cases in the community who are less acutely ill and the members of the normal population, who have to tolerate the burden of mental illness in their relatives or associates. The basis of the whole activity is clearly quantitative. In order to comprehend the problem or to evaluate results of effort, adequate statistical data must be collected. It might be thought that to labour this point was unnecessary, and some may even consider it dangerous. Nothing is duller than the compilation of unnecessary statistics, official or unofficial, and nothing can be more misleading than numerical data collected or interpreted without proper forethought. I will therefore first draw attention to some erroneous conclusions, which have been sometimes drawn from statistical data in the psychiatric field.

The proportion of people who are certified mentally ill or defective in any given population is a figure fairly easily obtained. An American authority dignified this ratio by the name of the "asylation coefficient." The facts are unequivocal and, from the technical point of view, the measurement is a good one. Interpretation, however, is another matter altogether. In England nearly 5 persons per 1,000 of the general population are under certificate. In New York State, however, the corresponding figure is of the order of 7 per thousand. Are we to infer, as some people have done, that mental illness is commoner in New York State than in England? If a high institutional incidence of mental conditions were taken as a measure of poor mental health in the district concerned, we should be obliged to acclaim conditions in Japan, for instance, where only one person in 6,000 is in a mental institution. Some years ago I observed that in European countries there was generally an inverse ratio between the number of serious crimes committed in a country and the number of people certified (Penrose, 1939). I think there can be very little

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doubt that, after corrections are made for age-group distributions, the proportion of certified patients is a positively favourable index of mental hygiene in the general community. Social work does not tend to reduce the number of cases under treatment; it increases the number. It does so in more ways than one. First, by providing channels of communication between hospitals and patients in need of treatment; secondly, by removing the stigma of mental illness; and thirdly, by stimulating public enthusiasm for the development of mental health services. Eventually all these roads lead to the necessity of providing more beds in mental wards and the saturation point is still a long way off. We must accept the paradox that more mental beds imply better mental health.

There are some gross errors, which can be supported by the misinterpretation of statistics of mental illness. A fantastic figure known as "discharge rate" is calculated in hospitals all over the world. The annual number of patients discharged is divided by the annual number admitted. The figure usually varies between 40 and 80 per cent., and is supposed to indicate whether or not the hospital is doing good work. Sometimes a refinement is introduced, and the patients discharged are graded as "recovered," "improved" and "unimproved." In the interests of honesty, the grade "worse" should also be used, though I have never been able to find an administrative psychiatrist who would admit this. The major absurdity, however, is that if the same patient is discharged recovered and readmitted repeatedly the "discharge rate" approaches 100 per cent., although no tangible benefit is being derived from treatment.

Another persistent fallacy derived from statistical data is the illusion of anticipation in familial cases—progressive degeneration of the stock in succeeding generations. This idea was strongly upheld by Mott, but its basis is undoubtedly an artefact due to selection of parent and child pairs with approximately simultaneous onset and is not a biological phenomenon (Heron, 1914; Paterson, 1932).

My main purpose is not to dwell upon all the possible fallacious uses of statistics, but to point out some of the benefits which can be obtained from sound compilation and interpretation. There are two fields of study: (*a*) cases treated in mental hospitals or psychiatric wards; (*b*) cases in the general population, seen at home or in out-patient clinics. Obviously, to obtain a complete picture of the magnitude of the whole problem both types of records are necessary.

The two sides of the problem are, however, quite different. Out-patient clinics contain, or should contain, a preponderance of neurotic case-material, and in-patients are predominantly psychotic. In the field of mental defect there is another peculiarity; idiots and imbeciles are much more strongly represented both in institutions and clinics than would be expected from a knowledge of the number of low-grade defectives, as compared with the number of feeble-minded, in the general population. Total population surveys of defectives are quite feasible, though they can usually only be undertaken by sampling methods, as in the investigations of Lewis (1929), or by group psychological tests of children (Roberts *et al.*, 1935). Population surveys of mental

diseases are much more difficult, and will remain so until reliable tests for neurosis and psychopathy have been invented. In order to understand the natural history of mental illness or mental defect, however, the primary need is to obtain data on the most severe cases, namely, the institutional ones. These records can be accurate and complete.

It is not fully realized by the general public how large a percentage finds its way sooner or later into wards for mental cases. Pollock, Malzberg and Fuller (1934) estimated that this applied to 4½ per cent. of the population in New York State. Pre-war German estimates were of the same order. In England a similar figure might be found if we had the data upon which to calculate. A crucial datum is the length of stay of a given patient, not merely on first admission but on each subsequent admission also. I have little doubt that in England (as Dayton (1940) found in New England) statistics would show that the mean duration of institutional life (for all patients) is more than ten years, that is, one-sixth of the whole duration of life.

If 5 people per thousand are to occupy beds each for one sixth of their lives, 30 people per thousand (i.e. $\frac{6 \times 5}{1000}$), or 3 per cent. of the population, must be certified at one time or another. This is a quick way of obtaining a rough estimate, and does not allow for the peculiar distribution of duration of stay in mental hospitals. This distribution itself is of great interest for the evaluation of the results of therapies, but is very little known. If we imagine 100 patients admitted simultaneously for the first time, we may assume that at the end of one year 50 of them would still be on the hospital books; the other 50 include those who died and those who have been sent home. At the end of the second year, however, some 40 would be left on the hospital books, and at the end of 20 years we should still have a residue of about 20 left (Penrose, 1943). A large part of this residue is formed by patients admitted more than once.

Statistics show very clearly that psychosis is essentially a chronic disability, with exacerbations and remissions. As a medical problem, it resembles gout, psoriasis, allergy or diabetes rather than an infectious disease. For this reason we should be extremely cautious in evaluating the effects of therapy except in terms of very long periods of time. For instance, if the stimulus of electrically induced convulsions induces remissions earlier than we would otherwise have anticipated, what assurance have we that relapses may not thereby also be facilitated? The results of such treatments are often dramatic when considered in terms of weeks or months. Statistical inquiry, however, convinced me that, when a long range view is taken, with a minimum of five years since treatment as a standard, these methods hold no advantages over less violent procedures (Penrose, 1945). Again, we used to hear a great deal about the value of early treatment of mental disease. Indeed in some organic diseases, like G.P.I., early treatment can be shown to be essential. I doubt, however, if there is a shred of real evidence that the early special treatment of schizophrenia by insulin, for example, has any merit. Recent admissions have, in any case, better prognoses than patients who have been in hospital for years. Any treatment will have a better effect on recent cases

than on chronic cases, just because nearly half of the recent cases are going to recover within a year or thereabouts with good ordinary hospital care. The chronic cases, on the other hand, are likely to react poorly to any treatment. I would make an exception, however, in respect of leucotomy. This appears to cause a permanent emotional alteration and, if the result is favourable, recurrence of the same type of breakdown is therefore unlikely. On purely statistical grounds it is easy to show that, after leucotomy, patients have been discharged and have remained at home for a long time who would otherwise have had very little expectation of ever leaving hospital. It is beyond the scope of statistics to determine whether such leucotomized patients have been cured, or have merely developed socially acceptable forms of psychoses.

Many of the clinical differences between the various types of mental diseases, though they may be constant sources of irritation or of fascination to the psychiatrist according to his type of mind, are useless for the research worker. We cannot yet distinguish between schizophrenia and affective psychosis with any certainty. At some hospitals the patient who, after three or four years' institutional life, retains an initial diagnosis of manic-depressive insanity is fortunate. Here, strangely enough, statistical inquiry has a great deal of useful information to give. First admissions tend to be given diagnoses which are very characteristic at different ages. A good guess at the correct diagnosis can be made if only the age on first admission and sex of a case is known. For example, a case certified below the age of 20 years is most probably a defective. Schizophrenia in male first admissions has a peak frequency at about the age of 25 years. Schizophrenia in the female has its peak ten years later in life—at 35 years. Affective psychosis in the female has its peak of frequency at 45 years, and for the male the corresponding age is 55. Statistically, therefore, it is comparatively easy to distinguish the two main diagnosis groups in males by first admission age, but more difficult to do this in females. The age-grouping of pure paranoid psychoses indicates that they are more closely allied to affective than to schizophrenic reactions. In the age-groups around 65 and over, almost all first admissions are diagnosed senile or organic. The number of cases in these late age-groups is large, and is gradually increasing as the age-group in the general population from which they are drawn grows larger. It is of extreme interest to know how many beds will be required for such cases in the future and only accurate statistical inquiry can determine this.

From the point of view of genetical research the age of onset of disease is a more useful fact than a changeable diagnosis. One reason for this is that any mental disease, when it is of early onset, is a potent factor in reducing the subject's fertility. The fertility of psychotics in general is of great eugenical significance but we need a census of their children to find out the exact biological trends.

A great many quite simple inquiries on the aetiology of mental diseases can be aided by statistical records and some inquiries are impossible without them. It is valuable, for genetical studies, to know as nearly as possible the population frequencies of different diseases, especially when such diseases are rare. Moreover, as a preliminary to detailed study of any given condition, it may be extremely helpful to have a record available of where such cases can

be found. For example, in linkage studies carried out with a view to refining eugenic prognosis in Huntington's chorea or in phenylketonuria, it would be of immense advantage if the investigator could quickly ascertain at what centres the case material could be found. Another important line of inquiry is the detection of recessive abnormalities or predispositions by the method of identifying types of cases whose parents have a high consanguinity rate. This method was the basis of successful inquiries initiated by the Medical Research Council on patients both in general hospitals (Bell, 1940) and in mental hospitals (Munro, 1938). The ascertainment of relationship between the parents of the mothers of cases is also of interest if rare antigenic factors similar to Rh are suspected of being aetiologically significant. Facts about consanguinity are very easily obtained if asked for in routine inquiry, but otherwise their collection involves immense labour.

Finally, I may mention the intriguing problem of assortative mating. Is there any evidence that people of weak mental stamina tend to marry one another? Statures of husbands and wives are positively correlated to the extent of about 0·20, for instance. Levels of intelligence of married couples are much more closely similar than statures; the correlation coefficient is about 0·40. If we examine husbands and wives in cases where both partners have had the misfortune to be certified we will find, first of all, that this occurrence happens more often than could be attributed to mere random chance (Penrose, 1944). We may also find that, both with respect to diagnosis and age of onset, the type of disease is extraordinarily similar in both members of the pair. The correlation coefficient for age on first admission is of the order of 0·60. One must not, however, overlook the environmental factors, which may tend to produce illness in one partner at the same time as in the other partner. In fact the likeness of husband and wife in respect of first admission age is absurdly high in comparison with the brother-sister correlation, 0·50, estimated in the same manner. The great similarity of type of mental illness found in husbands and wives must, therefore, be attributed in a considerable degree to environmental influences, but a significant tendency for persons of like potentialities for mental breakdown to marry one another also seems clearly demonstrable. To check and to amplify this result it is necessary to keep records of all cases of certification of husband and wife over a sufficient period of time.

I have pointed out a few examples of the uses of statistical data in psychiatric research which have occurred to me. They only scratch the surface of the ground, which could be productive in a great variety of ways. The examples have been brought forward to emphasize the need for the orderly collection of facts, so that they can be used later in statistical inquiries. Most of the data needed is not of a highly technical nature, and it might well be one of the duties of the trained psychiatric social investigator to collect the greater part of the material. I feel that it would be well if social workers were trained to realize that each individual case forms part of a community problem, which is essentially a branch of human biology. It is usually impossible to understand the individual case properly unless the base line of the normal average has been first determined. To determine this base line and the range of variation

requires the systematic collection of statistical facts, an occupation which will often appear quite futile until the material has been assembled and analysed.

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