

with many appointments and acquaintances, he became now a quiet scholar and experimenter who sought seclusion, save for his earnest teaching. As a surgeon he was no longer a rapid, spectacular operator but rather an extremely careful though not unduly slow one. Intestinal suture and the healing processes of wounds were among his objects of study. In technique he improved or developed operations for hernia, thyroid disease, aneurysm, and especially carcinoma of the breast. Meanwhile he made frequent trips to Europe and developed a lifelong association with and affection for the brilliant and modest Swiss surgeon Theodore Kocher, of Berne. Among the best known of his many pupils were Cushing, Bloodgood, and Mitchell.

The later years of Halsted's life were given to establishing methods of clinical teaching and to training other surgeons. In these tasks he was quite extraordinarily successful. He had about 238 resident surgeons under him during his period of office. Of these some 140 became academic instructors in surgery. Truly, surgery as practised now in the United States is the child of William Stewart Halsted.

### ACCIDENT PRONENESS

Penrose<sup>1</sup> has shown that the course of literature on new medical treatments is something like an epidemic—a slow onset of the idea, followed by praise increasing to saturation point, followed by articles against, and eventually declining to cautious statements in medical textbooks. The literature on accident proneness illustrates this. The epidemic has not been severe, comparatively few people have been affected, and the arguments for and against are characterized by moderation. This may be due to the scientific accuracy with which Greenwood and Woods<sup>2</sup> originated the concept in 1919. Their work, and the work of Greenwood and Yule<sup>3</sup> and Newbold,<sup>4</sup> set a standard of statistical precision such that Arbous and Kerrich<sup>5</sup> can only say (even though they doubt whether accident proneness has been demonstrated), "It is a very unfortunate but real fact that our knowledge of this concept has hardly proceeded further, and in some respects has suffered a set-back from the time when Greenwood, Woods, Yule, and Newbold undertook their classical studies in 1919 and 1926." This is a slight exaggeration, but certainly the development has been slow.

Accidents are an important cause of death, disability, and economic loss, and attempts to understand their problems are welcome. But (again to quote Arbous and Kerrich) "data associated with this problem are extraordinarily complicated, and the relationship between causes so intricate, that it is difficult to set up adequate conditions of experimen-

tal control, and to isolate the various determining causes." The main difficulties are, first, in obtaining reliable evidence; secondly, in defining and measuring accident proneness; and, thirdly, in relating the measured accident proneness to other medical or psychological qualities of the individual.

The demonstration of individual differences in a group of people requires that as many as possible of the conditions to which they are subject be kept constant. A number of people should work the same amount of time in identical conditions on identical work. Further, they must have sufficient accidents to provide evidence—however accidents may be defined. These conditions cannot be completely fulfilled, for factory work changes; people go off sick or are absent as a result of an accident. To minimize these difficulties Greenwood and Woods worked with minor injuries only—not injuries causing loss of time—and kept their observation periods short. Their defence of this procedure was that "the law of a *distribution* will not in general be affected by the consequences attaching to the results. The number of sixes thrown with a pair of true dice in a hundred trials will not be affected by the height of the stakes." While this is sound statistics it is not necessarily good psychology, and people have accidents, not dice. Adelstein<sup>6</sup> found little correlation between major and minor accidents, thus confirming the work of wartime investigators. There is also the possibility when investigating accidents that what is being examined is proneness to report accidents—not proneness to experience them. The criterion of an accident itself provides a difficult problem, with no real solution other than that of expediency (such as compensation criteria).

The statistical demonstration of individual difference is usually performed in any of the following three ways: departure of the observations from chance distribution may be tested; the recording period may be split into halves to see what would have happened in the second half had the worst members of the first half been removed; and the observations in the two half periods may be examined for correlation.

To perform any of these tests adequately needs a good deal of evidence—a large number of people and many accidents—and care must be taken to ensure that the population has as homogeneous an experience as possible. But these conditions are rarely achieved, and chance plays a large part in the obser-

<sup>1</sup> *On the Objective Study of Crowd Behaviour*. H. K. Lewis. 1952.

<sup>2</sup> *Rep. industr. Hlth Res. Board (Lond.)*, 1919, 4.

<sup>3</sup> *J. roy. statist. Soc.*, 1920, 83, 255.

<sup>4</sup> *Rep. industr. Hlth Res. Board (Lond.)*, 1926, 34.

<sup>5</sup> *Biometrics*, 1951, 7, 340.

<sup>6</sup> *J. roy. statist. Soc.*, to be published.

<sup>7</sup> *Rep. industr. Hlth Res. Board (Lond.)*, 1926 et seq., 38, 55, 68, 74, 84.

<sup>8</sup> *S. Afr. Air Force J.*, 1949, 1, 2.

<sup>9</sup> "Pilot Errors," Air Ministry 3139 A., H.M.S.O., London.

<sup>10</sup> "Accident Proneness in Coal Miners" (unpublished communication).

vations. This is best illustrated by a fictitious example. Suppose there are 1,000 men, all engaged on identical work under identical conditions, and that 900 have a true risk of one accident a year, and 100 have a true risk of two accidents a year. These are conditions which would favour the discovery of accident proneness. The records are treated in the usual ways. Six months would reveal nothing; neither would a year. Two years' records give rise to suspicion, and after three years there would be sufficient evidence to say that the individuals were not all the same so far as accident liability is concerned. Thus even in these ideal conditions the progress of investigation would be slow. If at the end of three years we picked out the 70 men with seven or more accidents, we could have 39 prone and 31 normal. Correlational procedures would not be very helpful, two one-year periods yielding a correlation of  $+0.076$ , two two-year periods  $+0.141$ , and two three-year periods  $+0.197$ . If it were possible to correlate two 10-year periods the value would leap to  $+0.450$ . Thus it is hardly surprising that analysis of data with smaller numbers and with shorter exposure periods often fails to demonstrate accident proneness. What is more surprising is that doubt is then cast on the hypothesis rather than on the evidence.

The work of Farmer and Chambers<sup>7</sup> and of Biesheuvel,<sup>8</sup> and the more clinical investigations by Davis,<sup>9</sup> show (despite the difficulties of measuring accident proneness) that accident proneness is associated with other qualities of the individual. Farmer and Chambers showed that aestheto-kinetic performance tests could predict the chances of accident proneness in a person, though perhaps not sufficiently strongly to warrant application. Biesheuvel in South Africa (using a mixed battery of tests) and Davis in this country showed that it is possible to predict which pilots are likely to have flying accidents. The wartime work departed from the idea of a single personal quality of accident proneness. Instead accident proneness was regarded as the behavioural consequence of a number or constellation of personal qualities. Biesheuvel suggested various accident "indicators," and based his predictions on the total number shown by the individual. Davis showed two distinct patterns of flying breakdown, and related these to psychopathology. More recently Whitfield<sup>10</sup> has suggested that accident proneness can be attributed either to perceptual or to motor failure.

Less can be said about the practical application of the knowledge gained. These studies inevitably raise the delicate issue of personnel selection. It can possibly be held that, for occupations involving responsibility for the safety of others—such as piloting aircraft—the use of any and all evidence to select the least accident-prone individuals is a necessity. For

industrial populations such a procedure would be cumbersome and wasteful and, in the present state of knowledge, unjustified. It will probably prove more profitable in the long run to discover more about the conditions giving rise to accidents and about the limits of perceptual and motor response of the average person, so as eventually to modify the working environment and reduce the total risk, rather than to pick out the unfortunate accident prone.

### EUROPEAN CONGRESS OF CARDIOLOGY

Over 500 medical men and women from European countries and the United States attended the meetings of the First European Congress of Cardiology, held last week at the University of London from September 9 to 12. A total of 88 papers were read at the Congress, and over two-thirds of these were on mitral valvular disease and electrocardiography. It is interesting that hypertension and cyanotic congenital heart disease, which a few years ago would have occupied a large part of the programme, received relatively little attention, and the treatment of heart disorders, apart from the treatment of mitral disease, was discussed in only four papers.

One morning was devoted to a symposium on mitral valvular disease, and the same topic was discussed in two other sessions. There was general agreement that the results of mitral valvotomy were good or excellent in three-quarters of the cases operated on and that the operative mortality was low in those cases with pure mitral stenosis. Even in patients with auricular fibrillation, aortic valvular disease, or greatly enlarged hearts the immediate results were good although the operative risk was much greater. Several speakers described the changes in the pulmonary circulation before and after valvotomy. Before operation a moderate rise in the resting pulmonary artery pressure was the usual finding with a further increase after exercise. A large fall in pulmonary artery pressure was common after valvotomy, although the maximum haemodynamic improvement might not be reached until a period of six to nine months had passed. Subjective improvement was immediate, and the increased effort tolerance probably provided the best measure of the success of the operation. The magnitude of the task confronting cardiac surgeons was emphasized: one speaker calculated that 4,000 cases a year would have to be operated on in this country alone, so that many new centres would be needed.

There was general agreement among speakers at the Congress that it was impossible to assess accurately the amount of mitral incompetence which was present in patients with mitral stenosis. Tracings of pulmonary capillary pressure, which closely paralleled changes in left auricular pressure, failed to give reliable information on this point. This is not surprising, since direct pressure readings from the left auricle before valvotomy in proved cases of mitral incompetence sometimes fail to show diagnostic changes. A small series of cases was reported in which a pericardial sling had been introduced to lessen the degree of mitral incompetence; the