PRECIPITATING PROXIMAL FACTORS IN THE OCCURRENCE OF MENTAL DISORDERS: EPIDEMIOLOGICAL EVIDENCE

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THE aim of this working paper is to review the evidence which has accrued from epidemiological studies on the relation of various external events or immediate environmental circumstances to the onset of mental disorders of several kinds. It is not concerned with the clinical detail of such disorders but rather with the part which the external factors may play either in increasing the risk of disease onset or in modifying its evolution. From the viewpoint of possible preventive action we are interested in the detection of a specific enviromental hazard which increases the incidence of a particular psychological disorder in a population group above the level which might be expected on the basis of that group's demographic and other characteristics. The crucial question is whether the individuals who comprise this excess in mental morbidity would have developed the disease at that point in their lives without exposure to this external stress. In other words, can the onset of disease be entirely explained by constitutional, physical or other factors, or does the external stress add appreciably to the risk of precipitation? To answer such questions with any conviction needs an equal certainty in the quantitative assessment of the external factor and of the reaction of the individual and population to it; and there has been a tendency to confuse in the single word "stress" both the apparently noxious stimulus and the presumptive response. Indeed, there are few circumstances where there might be any very wide agreement about what constitutes a potential hazard to mental health and fewer where an unequivocal assay of its potency can be proposed. It is thus hardly surprising that the epidemiological or statistical evidence about the effect of such externals on mental disease is relatively scanty and that much of it comes from studies done in war. Only in war are large segments of the male population, selected in a

fairly uniform manner, exposed to threats to life which are obvious and severe enough to be accepted as a universal emotional stimulus. Even in such relatively advantageous conditions for investigation, however, problems of interpretation soon become manifest. It thus seems sensible to set out some examples of studies done largely in the second war in both military and civil populations so that the applicability of methods and results to the less extreme circumstances of peacetime can be more fully assessed. Some examples of studies made between the wars are also discussed.

War and the Onset of Mental Disorder

The stress of war is now readily associated in the lay mind with the onset of psychological breakdown, but its effect on medical views about the nature and origin of psychoneurotic illness is perhaps less widely appreciated. Recently, Adrian (2) has described how, just as the Spanish-American War hastened the eradication of malaria and other tropical infections in that part of the world, the first World War with its outbreak of "shell-shock" cases within a few months heightened the interest in the etiology and treatment of the neuroses. At first, as the diagnostic label "shell-shock" implied, it was thought to be the result of mechanical damage to the brain—a *commotio cerebri* due to the blast of high explosive. Only later did the emotional origin of the mutisms, deafness, paraplegias, and convulsive attacks become apparent; and in this realization the still relatively novel Freudian teachings about the pathogenesis in emotional conflict, sexual or otherwise, of the neuroses doubtless had their influence. In the conditions of air fighting in the earlier years of the first World War, there were many physical factors specific to the air, such as oxygen lack to which syndromes labelled as "flying stress" could be quite reasonably attributed and Birley (5) discussed the part they might play in causing neurosis among pilots. The abnormal results in cardiovascular tests seen in patients with severely disabling neurotic symptoms served to perpetuate this notion and to

overemphasize the physical component in the causation of such disorders (13). Although the essentially emotional origin of these disorders was clearly recognized by writers such as Anderson (3), there seems to have been a lingering suggestion that neurosis in pilots, for example, was usually the cumulative result of the stresses, physical as well as psychological, inseparable from air fighting. Clinical impressions of the effect of acute episodes of fear in action rested on the time relationships between such events and the onset of symptoms, on the occasional suppression of these incidents in clinical histories and on the results of abreactive methods of treatment. On the other hand, controlled studies, where comparisons are made between the frequency of terrifying crises in the history of patients with neurosis and in healthy soldiers in the same unit, were conspicuously absent. In the first World War there seems to have been no special systematic attempt made in the Allied medical services to investigate these disorders, whether of infantrymen or pilots, by methods now classed as epidemiological. The success of treatment by abreaction would seem to have confirmed the overwhelming import of emotional conflict; but, as Adrian remarks—"without statistics, and with the fortunes of war changing from month to month, the evidence in favor of treatment by abreaction was not convincing proof of the theory behind it."

The need for statistics on the fluctuating patterns in neurotic disease incidence as a guide to the effects of war stresses was thus patent; but even in the second World War this type of evidence was not immediately sought. The development of "operational research" (i.e. the application of scientific methods to the problems of decision-making in human organization) in the British forces was, however, paralleled by similar field survey work in the medical services on the prevention of neurosis and inefficiency in combat. The methods used were in effect epidemiological; for the intention was to relate the incidence of neurosis to such measures of "stress" and "fatigue" as could be devised and to compare the incidence in men, units or forma-

tions with different operational roles and at different times.

Although reports on the mental health of the United States Army (42) during peacetime showed the importance of neurosis as a cause of hospital admission, particularly in white troops serving overseas, the impact of war was needed to produce observations on the effects of acute stress on complete military communities. A typical report comes from Hogan (24), who, as senior medical officer on board the aircraft carrier U.S.S. WASP, surveyed the officers and men of his ship some time after it had been torpedoed. He reported that although one-third of the crew had an immediate emotional reaction and were still nervous, apprehensive and "jumpy" three weeks after the attack, only two developed neuropsychiatric disorders. This experience seems to underline the need to view the problem of the effect of such events from three points of view:

1. The immediate effect of one or more presumably frightening events;
2. The cumulative effect of a series of such events over a period of operational duty;
3. The relative importance of these external circumstances compared with differences between the constitutional predisposition of individuals to neurotic illness.

The many reports of admissions to military hospitals in all theaters of war are of limited epidemiological interest because the cases are not related to any parent population. Sims (36) tried to overcome this defect by comparing the ratio of patients with anxiety and depression to those with hysteria or schizophrenia and he suggested that, in men from different army units, this ratio varied in step with the degree of initial selection, i.e. with better selection the proportion of hysterias and psychotics was low. Although he thought that the ratio might be affected by operational stresses affecting particular units he did not suggest that it could be used as a useful indicator. From the point of view of Army statistics, Appel (4) warned that very considerable fluctuations in the rates of hospital admission or discharge from the Army because of neuropsychiatric

disorder resulted from changing demands for military manpower during the course of a war. For these reasons, hospital and similar data from the services proved to be less useful in the epidemiological study of psychological disorder than might have been either hoped or anticipated. Similarly, the reports, such as those by Cochrane (8) or Tas (41), of neurotic disturbance in groups exposed to particularly distressing circumstances either as prisoners-of-war or as inmates of concentration camps are, inevitably, anecdotal rather than statistical or epidemiological in nature.

In the Royal Air Force, a system of notification of patients referred by squadron medical officers for psychiatric opinion was set up quite early in the war (40). This allowed cases to be related to the formation populations exposed to risk and the calculation of incidence rates. The first broad review of these rates showed that the highest incidence rates were in the operational commands (Bomber, Fighter and Middle East) and that the rate in the Middle East, despite the added strain which might have been attributed to separation from home, was appreciably lower than the maximum rates which were found in the United Kingdom based Bomber Command. Within the latter Command, a more detailed study was made of the factors which might account for similar variations between squadrons in the incidence of disabling neurotic illness (34). Two indices of "stress" were used: a measure of the hazard or risk that the crews had to face stated in terms of the casualty rate per cent in each particular squadron each month; and the index of the duration of the flying effort demanded from them given by the average total number of hours operational flying completed each month in each squadron. The attack rates from both neurotic and venereal disease were taken as alternative indicators of the possible effects of these external factors. A correlation analysis of the variations in time of these several indices showed that a period of heavy casualties was accompanied by a high incidence of neurotic illness in the same month in the squadron affected (and a sharp rise in the frequency in venereal

disease in the month thereafter). On the other hand, mere duration of flying effort unaccompanied by heavy casualties had no such time relation. Similar observations on the correlation of battle casualties and neurotic breakdown were made in other theaters of war (e.g. Hanson and Ranson (20) reporting on experience with the 45th U.S. Division in Italy in 1944).

The indications given by these crude correlational studies were followed up by comparing the incidence of neurotic breakdown among men at different stages of the tour of thirty sorties against German targets then current in Bomber Command of the Royal Air Force. It was found that the maximum incidence was during the initial five sorties of the tour and that after the twelfth sortie there was no further rise in the incidence curve. This finding would support the earlier suggestion that the immediate effect of operational conditions on predisposed men is greater than the cumulative stresses in those who survive the initial testing period. At the same time, it should be pointed out that the early breakdown of the more susceptible may explain at least part of the association in time between the occurrence of heavy casualties and a high incidence of neurosis. As replacements are called for during a period of heavy fighting, more inexperienced and untried men will be exposed to the operational test and an outbreak of fresh cases of neurosis is inevitable. When, however, some allowance was made for this possibility by adjusting the rates according to the proportion of operational novices in each squadron, it did not obliterate the time relation of hazard to neurotic illness.

The resultant effects of the balance between the standard of selection and the specific stresses of the individual's operational job are seen in the contrast in incidence between air gunners and pilots. The maximum rate was found in air gunners, who may have had an isolated post of duty but who were less rigorously selected either initially or by progressive elimination during training. The low rate among pilots, despite their more skilled and responsible task, presumably resulted from the degree of psychological selection to which they had been subjected

during training. Because of this composite effect of selection and the circumstances of the particular job, these occupational comparisons are of little value in assessing the part of specific operational stresses in aircrews. It is unfortunate, too, that even in the second World War, there were no reported studies of the immediate past history in carefully matched groups of patients and controls. On the other hand, there were psychiatric reviews of samples of men who had completed a tour of operations without reporting sick because of psychiatric disorder. Hastings (21), for example, showed that 95 per cent of a group of 150 such men had developed symptoms during their tour; one-third of them had more severe disturbances but never came under psychiatric care. The limitations of conclusions about the effects of the crises of war on men based only on those seen by the psychiatrist are thus patent and alternative means of assessing these effects are clearly needed. This is of some practical importance in preventive medicine among troops in combat where a tolerable limit has to be set on the duration of the operational duty required of them.

Alternative Measures of the Effects of Combat

Clinical experience of patients suffering from acute neurosis suggested that some of the physical concomitants of the illness might serve as indicators of response to stressing situations in the absense of specific complaints of an obviously psychological nature (e.g. Reid (33)). Other forms of reporting sick, e.g. for trivial upper respiratory disorders, are a frequent portent of impending breakdown; but the trend of minor sickness reporting at different stages in the tour in Bomber command did not prove to be a very sensitive indicator. Body weight, on the other hand, was found to decline significantly between men beginning their tour and those who had completed up to six sorties; it rose slightly among those who survived up to twelve sorties; and it remained constant thereafter. This was in line with the fluctuations in the incidence of neurosis at the same stages of the operation career and appears to imply that both indica-

tors of the effects of stress agreed in suggesting a marked immediate effect of the conditions encountered and a much smaller effect (on those who survived) from continued exposure to such conditions up to the then prevailing limit of thirty operational sorties. Disorders of binocular fusion was another physical measure used in this context. Crews serving in the special Pathfinder Force of the R.A.F. Bomber Command were examined after return from each of thirty sorties using the Livingston gauge of binocular vision (35). As before, there was a deterioration in the power of binocular fusion which coincided with the initial period of stress already noted. In this group, either because of the sensitivity of the technique used or the special stresses of their operational role, there was also some indication of a deterioration in the last few sorties among those who had survived the whole tour. In the Western Desert, Graham as medical officer of an armored brigade, surveyed the distribution of blood pressure among troops who had spent at least a year in desert warfare (17). He found a symptomless hypertension (diastolic pressures of 100 mg. Hg., or more) in 27 per cent of the 695 men examined; and he found it difficult to attribute this to any factor other than the emotion aroused in action. After two months' rest, most of a group of men that he re-examined were found to have a normal blood pressure. These physical measures thus agreed with the trends in psychiatric morbidity in suggesting that the emotions and anxieties of battle were producing an immediate effect which resulted in disabling neurotic illness in those more severely predisposed to it and appreciable physical changes in others who did not come under psychiatric care. Although the relatively early breakdown of those susceptibles accounted for many of the time trends in incidence, some of the variations in incidence must be attributed to variations in the intensity of these external stresses.

The Effects of War Conditions on Civilians

The outbreak of war and the aerial bombardment of Britain did not appear to cause any dramatic change in the rate of ad-

missions of psychotic patients to the country's mental hospitals. Indeed Hopkins (25) reported a decrease in admission to the observation wards of hospitals serving the Liverpool area. This could be attributed to "the increased opportunities for employment, the strengthening of the community spirit, and a lessening of that consciousness of mental isolation so favorable to the development of psychological abnormality." Hemphill (23) pointed out the particularly marked effect of the War in lowering female admissions through the provision of alternative outlets for activity.

Some of the changes in admission rates may reflect other contemporary social changes but many of them, such as the increasing difficulty of caring for the aged sick at home, were unlikely to reduce the admission rate. As Svendsen has remarked in his discussion of contemporary experience in Denmark (38), many such features of the social environment can alter the threshold for admission. Nevertheless, there was no evidence that the privations and anxieties of the War, either in the United Kingdom or in enemy-occupied countries like Denmark, had produced any major increase in serious psychosis.

The evidence about the frequency of frank neurosis, of "psychosomatic" illness or of panic states is less clear-cut and the subject has excited some discussion between Meerloo (32) who urges preparations for the psychological consequences of air attack on civilian populations and Enloe (12) who denies that past experience suggests that this is worthwhile. Lewis (29) has given the most comprehensive review of British experience in this respect. He reported that, on the basis of data from the records of a general practice in London, there was some evidence of a slight increase in the frequency of patients reporting sick with neurotic complaints often a week or ten days after the bombing. Most of these had suffered from such illnesses in the past; and anxiety and depression, rather than hysteria, were the usual modes of clinical expression. The incidence of neurotic illness was quite low in firemen and civil defense workers who were exposed to considerable dangers in the first two years

of the War. As already noted, neither independent observers nor hospital statistics suggested any major rise in the admission rate to mental hospitals, although, as the War went on and shortages in housing, clothing, and food became worse, many senile dements, who would normally have been cared for at home, had to be admitted to hospital. In Scotland, where data on suicide were available, the death rate from this cause fell. Juvenile delinquency increased and there was a considerable rise in road and industrial accidents. Lewis raised the possibility that these events reflected the same environmental factors as conduce to neurosis.

Broad surveys such as that of Lewis were supplemented by more intensive studies of particular aspects of civilian reaction to bombing and other hazards of war. Fraser *et al.* (14), for example, followed up a series of individuals who had been admitted to First Aid Posts after being buried in bombed houses. Of those who had been buried for over an hour, 66 per cent developed neurotic symptoms and 40 per cent a neurosis severe enough to cause absence from work; in about equal proportions, the neurosis was either temporary or persisted for at least ten months. Persistent disturbances were not confined to those with previous neurotic illness and social complications such as the loss of a cherished home appeared to delay recovery.

Some studies of the effects of war on apparently physical diseases reflected in their results a psychological element in the origin of the disorders concerned. During the intensive bombing of London and Bristol, Stewart and Winser (37) showed that a sharp rise in the frequency of peptic ulcer perforations coincided with air raids. Later, Illingworth and others (27) in Western Scotland confirmed this general time relation but remarked that the rise in the perforations admission rate antedated the raids on Clydeside and might thus be due to anticipatory tension and anxiety rather than to the immediate stress of the attack. As in the military studies, then, the immediate effects of war conditions on neurosis and physical disease often considered to be aggravated by psychological stress were ap-

parent in the civil population. But the negligible effect on the frequency of psychosis was perhaps the more remarkable finding.

Occupational and Economic Circumstances and Psychological Disorder

There are fewer obvious opportunities for the study of the effect of acute emotional stresses in peacetime civilian life. Only a few occupations, such as those of air line pilot or coal miner, would appear to present the risks which might be unequivocally accepted as likely to cause even transient emotional disturbance of the severity common in war. The comparison of the mental disorder disease experience of either of these occupational groups with those of other men is open to the objection that they are selected both from the point of view of self-selection or by initial medical examination and subsequently through the attrition due to physical disability inevitable in such exacting jobs. In the same way it is difficult to attribute to the depressing circumstances of their homes, the particular mental disease experience of the poor without considering whether the disease preceded the poverty and the poverty resulted in the living conditions. Hyde and Kingsley (26) for example, have found that the rejection rates for psychosis in conscripts for the United States Army were related both to the socio-economic level and the population density of the area from which they came; but they also pointed out that these findings could be explained by the gravitation of the mentally less fit into the poorer communities. The problem of migration, either within the social class system or geographically within town or state, is discussed in other papers; but the complications that these selective factors entail explains why most of the epidemiological papers dealing with the possible effects of stresses are concerned largely with trends over a period of time *within* large occupational or other large population groups.

Successive generations have cherished the notion that the ever-quickening pace of their lives and the stress it involves

were certainly producing a rising tide in the incidence of mental disease. By a careful review of the statistics for mental hospital admissions in the United States, Goldhamer and Marshall (16) showed that if the changing age-structure of the population were taken into account there was, in fact, no evidence of a long-term increase in the incidence of psychosis in early or middle life during the whole of the previous century. Other studies have covered a shorter period and concentrated on the relation of indices of mental ill health to economic or other circumstances which might be expected to be emotionally disturbing. Dayton (9), for example, reviewed the rate of admission to mental hospitals in Massachusetts over the years 1917–1933—a period which covered both America's participation in the first World War and the nadir of the economic depression. The War produced no demonstrable effect on the admission rate for psychosis but there was some increase in admissions of older patients suffering from senile or arteriosclerotic brain damage. Significantly, the pattern observed resembled that found in wartime Britain in that there was certainly no increase in the admission rate for the diseases of the younger age-groups such as schizophrenia and manic-depressive psychosis. The economic depression of 1932–1933 accentuated this tendency for the increasingly frequent admission of senile patients but it seems likely that, again as in England during the last War, this might reflect the domestic difficulties and disturbances which make the home care of old people impracticable rather than any real rise in the incidence of the disease. Komora and Clark (28) confirmed that the depression had produced no remarkable increase in mental hospital admission rates in the United States despite the fact that in many of the patients coming for admission the economic situation was given as a precipitating factor in the illness. Broadly speaking, however, there seems to have been little disturbance of the general trends in admission rates over the present century which could be attributed to the Depression.

Before dismissing the possibility that peacetime stresses such

as unemployment, poverty, and insecurity have any effects on mental health, it should be emphasized, that as in wartime, hospital admissions for the endogenous mental disorders of younger people may give only part of the picture. Suicide, as an index of the frequency of the reactive depression which so frequently underlies it, is an act which need cause no rise in the hospital admission rate. But it is routinely recorded in the national vital statistics. Dublin and Bunzel (11) found an inverse correlation between the suicide rate and a business index derived from the activity of a public utility corporation in the United States; and the corrrelation was particularly close for white males. Swinscow (39) has repeated this type of investigation by relating the suicide rate for males and females in England and Wales over the period 1923–1947 to the proportion of men unemployed in the corresponding year. He showed that, for males at least, there was a very close relationship between the two. The crucial point, it would seem, is that this correlation is appreciable only for males in both countries.

Unfortunately, there are few numerical accounts of the experience of the lesser degrees of mental disturbance during these critical peacetime years. Perhaps Halliday's (19) discussion of the interrelation between the death rates from diseases often labelled as "psychosomatic" and other biological indices of mental malaise is the most stimulating. He used measures of reproductive behavior such as the fertility rates and the evidence about morbidity of all kinds that accrued from the statistics of the sickness insurance scheme then in force in Britain. He also cites Gafafer's (10) data on industrial absenteeism in the United States to support his contention that there has been in both countries a rise in the incidence of disorders such as peptic ulcer which may be affected by emotional disturbance. From the epidemiological viewpoint, Halliday remarks on certain characteristics of disease behavior which he believes to signal a psychological origin: a shift in the peak incidence to the younger age groups, an increasing male : female ratio and temporal, social, and occupational variations in disease incidence.

In the "Chronic Register" of the sick in Scotland who had been continuously incapacitated for more than twelve months, there was a disproportionate increase in some causes of chronic illness between 1931–1932 and 1935–1936 when Scotland was, economically, "a distressed area." Not only was this increase more prominent for disorders such as "peptic ulcer," "gastritis," and "nervous debility," but the increase was most marked among men under the age of 55. It is possible, of course, that such increases are more apparent than real; for, under the social insurance scheme, there was a financial incentive towards being classed as "sick" rather than "fit but unemployed." On the other hand, the younger age groups are notoriously the most sensitive to changes in industrial morale. Further, there were concurrent changes in other biological indicators of social or psychological disharmony. The suicide rate was increasing while the fertility rate fell particularly among occupational groups such as miners and textile workers whose industries were among those worst affected by the depression. The male : female ratio, whether in suicide or peptic ulcer mortality, rose and Halliday proposed that all these features of community disease stemmed from some deep seated psychological malaise.

Some of these manifestations may, of course, have simpler explanations. Cigarette smoking among younger males may be the more immediate physical cause of the high male : female ratio in peptic ulcer mortality. Errors in registration of occupation at time of death, particularly among the mining population, may inflate the reported death rate among them (22). Some of the excessive morbidity among textile workers may be the result of the efflux of the fitter men from a dying industry. But the consistency of some of the evidence of the results of depression and lack of confidence in the future of which the high suicide and low reproductive rates may be expressions suggest that the adverse economic and social stresses of the thirties may well have been responsible for a real rise in the incidence of neurotic illness.

There appear to have been surprisingly few studies in peace-

time industry quite like those described in flying crews. The short-term effect of prolonged driving on fatigue as measured by psychological tests and symptoms has been investigated on inter-state truck drivers, but less is available on the cumulative effect on health, mental or physical, of prolonged exposure to such conditions. In air line pilots, McFarland (31) reported that illness rates in general bore no direct relation to the hours of flying duty until the average flying time exceeded 90 hours per month. Although done in wartime, the findings of Russell Fraser's (15) survey of the incidence of neurosis among factory workers are quite generally applicable. He reported that a high incidence of neurotic illness was associated with the performance of over 75 hours of industrial duty per week. Many of the other features of the industrial environment which he showed to be linked with neurosis, e.g. boring tasks or undue responsibility, are those which Cameron (7) has listed as presenting a hazard to mental health in industry. There is, unfortunately, a dearth of epidemiological data on the psychiatric consequences, rather than the effects on production, of industrial conditions, methods of man-management and group morale. That such features of industrial life may be relevant to mental health is implicit in the results of studies such as those of Brodman *et al.* (6). They showed that there were consistent differences over a period of time between the twenty-four departments of a mail order firm in indices such as the incidence of long and short term absences for medical reasons, the number of reports to the medical department and the frequency of unexcused lateness. Those departments where unexcused lateness was frequent also had a high incidence of absences of short duration. The authors believe that both were indicators of defective morale and that an individual's behavior in these respects was markedly influenced by the group to which he belonged. The question implicit in this result is whether group morale as thus defined materially affects the incidence of psychological disorder.

The notion of "socially-shared psychopathology" has been fully discussed by Gruenberg (18) who recounts episodes rang-

ing from the dancing manias of the Middle Ages to the "Men from Mars" panic in 1938. Certainly, these "epidemics" of disordered thought or behavior strongly suggest the infectious nature of neurosis in closed communities. Studies such as those fostered by the Acton Trust (1) have suggested that the size of the working group (or the method of its supervision) was related to the frequency of events which are presumptive indices of psychological malaise. Industrial accidents and absenteeism, for example, tended to be consistently related to the size of the working group; and this relationship has been found in various industrial and other occupational groups. Some of the excessive sickness absence rates found in the staffs of the larger offices of the British postal services, however, may be at least partly respiratory in origin; and since larger offices tend to be in larger towns, an excess in respiratory affections is to be expected. Again, as Liddell and May (30) have shown in the mining industry in Britain, much of the apparent increase in accidents in the larger collieries can be explained by a more complete reporting of less serious accidents in these larger units. Nevertheless, the mere fact of such differing thresholds of reporting suggests differences in psychological climate which may be more directly relevant to the incidence of neurotic illness than at present appears. On the basis of the evidence at present available, however, the question must still lie open.

Conclusion

It will be obvious from this account of the patchwork of epidemiological studies reported in English in the literature that there has been no sustained systematic approach to the problem of the precipitating factors in psychological disorders of various kinds. On the other hand, there are some consistent threads appearing in different circumstances which give some support to some general conclusions. On both sides of the Atlantic, and in both Wars, the stresses of civilian life appear to have had no major effect on the frequency of the psychoses of early and middle life. Economic depression has also had a negli-

gible effect on the admission rate to hospital for such conditions, although the coincident difficulties of home care, either in depression or in war, increased the rates of admission for senile conditions.

In war, it proved possible to show the immediate responses of men to a stressful environment both in terms of an increase in neurotic illness and of bodily changes expressed as averages in groups of men. While it is true that predisposition to neurotic illness may be the dominant element in the determination of breakdown under stress, the experience of war, both in military and civilian life, suggests that such stress will, if severe enough, precipitate neurotic illness even in groups of highly selected individuals.

No exact parallel can be drawn between the results of the acute emergencies of war and the less obvious stresses of industrial life in peacetime. The evidence for any direct cause and effect relationship between normal industrial conditions and neurotic illness is slight; although there are hints that long hours of boring or highly skilled work, for example, are conducive to mental ill health. In the harsh conditions of an economic crisis, on the other hand, there is good reason to believe that, while the incidence of the psychoses may be unaffected, reactive depression, often ending in suicide, and a whole group of neurotic and "psychosomatic" disorders may cause increasing disability among the men affected by the slump in their industry. Recently, interest has centered on the effect of group size and organization on indices of industrial morale which may, in turn, be related to the risk of neurotic illness among members of the group. The epidemiological study of the effect of the stresses presumed to exist within our complex industrial society is still, however, in its infancy and no firm judgment is yet possible.

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DISCUSSION

DR. PAUL LEMKAU: Dr. Reid's excellent and comprehensive study is a definite challenge for all of us to attempt a more clear statement of our hypotheses regarding etiological factors in the psychoses and neurotic disorders.

The apparent fact, reiterated and well-supported by Dr. Reid, that psychotic disorders, in the younger adult period at least, do not change in incidence under the influence of immediate changes in environment is now so well-documented that we must accept it. It would appear that we are forced to either one of two general hypotheses about the etiology of the largest group of diseases in the conglomerate, schizophrenic disease or reaction. First, that the etiology lies in some disorder established psychodynamically quite early in life, before late adolescence at least. Or second, that it springs from some genetically distributed risk factor that operates independent of the environment as the individual matures. A third, alternative hypothesis, the combination of the two, has been the refuge of all serious investigators including the geneticist, whose data do not always fit his mathematics, the difference being accounted for in the term "penetrance."

The first hypothesis, i.e., an early psychodynamic etiology, has been supported mainly in the work of Tietze and Lidz, as has been reviewed by Dr. Cumming. Unfortunately, this hypothesis is notoriously unsatisfactory from an epidemiological point of view. It is extremely difficult to explain how schizophrenogenic households

should occur equally frequently in the various markedly different cultures in which the incidence rate of schizophrenia appears similar. (I hasten to add that Dr. Reid did not say that the rate is similar in different cultures, but that I am impressed with the evidence that it is.) The second hypothesis, a genetic-biological or physiological one acting relatively independently of the environment is not a very popular one in the United States but seems to me to be amassing a strong body of evidence in its favor, albeit all indirect and circumstantial. I do not mean to rule out the third possibility—that of two or more factors working together—but I must admit to a personal proclivity to think in terms of the much despised "either or" in preference to the more generally accepted "both." The latter seems to me to offer too easy an escape from the real dilemma we are faced with. For clinical work we are forced to the "both," but in the laboratory or at the *schreibtisch* the "either or" would appear to offer the most stimulation to new and productive work. In any case, the bias of the investigator is likely to insure what is close to an "either or" attitude.

From Reid's summary it is clear that when environmental stress is severe enough various breakdowns do occur, but that there are also various degrees of susceptibility. According to Stauffer's reports, vulnerability in military personnel correlates to some extent with occurrence of nervous breakdown in the family of the subject, with previous illness experience as reported by the subject, and with extent of heterosexual social contacts.

The problem of relating the extreme military stress situations to the occurrence of neurotic and other reactions in civilian life is well stated by Dr. Reid. The problem is of the same type as that of relating the fairly well accepted statement that extreme lack of stimulation in infancy or young childhood results in a damaged personality, to the question of whether lesser degrees of isolation produce partial or less severe damage.

The problem is, it seems to me, related still further to the study of the etiology of disease in general. It is probably safe to say that every disease is multiply-caused. The diseases in which we assume we "know the cause" are those in which there is one overwhelming cause acting at a particular moment; many have pointed out that in many diseases (such as in tuberculosis in the past when positive tuberculin reactions were more common than they are now) the

"cause" was much more prevalent than the disease. A better example might be found in pneumococcus pneumonia. At the moment the disease becomes apparent, the one overwhelming cause is the one that can be acted against, so that, as Dubos says so often and so well, it is all too frequently assumed to be *the* cause even though multiple causes are easily found, if looked for.

Is not the situation very similar in combat neurosis? A single stress becomes *the* cause since it is particularly severe at the moment and there is something that can be done directly to relieve it. As Dr. Reid has demonstrated, we actually may have better notions of the other causes over and above the overwhelming one in combat neurosis, such as sleep and food deprivation, than we have in many other diseases where, beyond such crude measures as social class, there is little clear epidemiology at all. The issue might be stated that whereas the data we have shows that stress may well be the general cause of neurosis—much as external invaders are of pneumonia—some kinds of stress will cause the illness more regularly and predictably than will others, combat experience being one of the most pathogenic. On the other hand, neurosis appears also to occur when this particular pathogen is not present as *the* overwhelming cause; the analogy is far from perfect, but perhaps it will help clarify our thinking.

The problem of the relation of the morale of groups to individual vulnerability tempts me into analogies from the field of physics; into the kind of energetics that Freud spoke of in discussions of libido and which creep into many statements to the effect that "everyone has a breaking point." The problem is the shift of the critical points at which stress breaks through the "strength" of the personality in some almost physical way. Such analogical thinking is helpful in envisaging our problems but it is, of course, dangerous if it beguiles us into thinking that we are talking about what really happens. We cannot fully describe the actuality and we must keep continually in mind that we are thinking in terms of models, not the real thing.

SUMMARY OF DISCUSSION

1. Some types of proximal or precipitating factors in mental disorder were added to those listed by Dr. Reid in his paper.

Childbirth, whether as a biological or as a psychological factor, had to be considered in connection with purperal psychosis. In addition to loss of spouse, other significant losses, such as loss of child, or loss of a limb, were mentioned.

In the past, eye operations were frequently followed by delirium, due partly to drugs but also to therapeutic measures. The situation was aggravated by the procedure of keeping both eyes covered-up for very long periods of time. Today's procedure of allowing sight to the unaffected eye as soon as possible after the operation has resulted in reducing the number of delirious reactions.

It was suggested that festivities are generally accompanied by a rise in suicides and admissions to hospitals. However, it was reported that the Chinese New Year had not produced any noticeable effect upon mental hospital admissions in Singapore.

That disasters had varied implications as proximal factors was shown by recent sociological studies in the United States. On a broader canvas, various types of psychological climates have arisen. One example was the belief that the world was coming to an end. Studies as to whether these psychological climates were related to various types of mental illness might prove interesting.

It was noted that compensation neurosis had an entire literature in Germany which, though full of pitfalls, might repay study. Acute infections, such as malaria, also needed to be considered in the present context.

2. It was remarked that the discussion was focussing primarily on the type of stimulus that induced stress. In studying communicable disease, however, less attention is paid to the stimulus than to the epidemiology of the response—the illness itself—which is then related to multiple factors (e.g. tuberculosis). Have there been any studies on the physiological response to psychological stress?

3. Yet it is not the mere occurrence of a stressful stimulus that is presumably related to the development of neurotic or psychotic symptoms, but rather the individual's response to the stimulus. It follows that psychological stress has to be studied in connection with what the stress means to the particular individual concerned. Most persons would find a bombing attack to be stressful, but some persons might find the loss of a job to be even more stressful. From the psychological standpoint it could be asked how a stressful stimulus worked upon an individual. Knowledge of the psychological mecha-

nism would help in taking into account stimuli that were not obviously stressful or not easily described.

Highly relevant sociological questions also needed to be asked. For any given situation there existed individual differences in susceptibility to stress. Yet the distribution of differential susceptibility had been grossly ignored in psychiatric research. One such area was the life cycle.

3. In addition, differences in response might be related to cultural factors; or, more specifically, to a special subculture that might arise in a particular setting. Thus Dr. Reid discussed the high breakdown rates of air gunners in contrast to the low rates among pilots arguing that the difference was attributable to selection processes which placed a man where he was needed rather than upon his psychological fitness for the job. Also relevant, it was suggested, was the fact that in the early days of the War there was a comparatively low rate of survival of air gunners; which in turn produced a mythology of the hazards of the job which was communicated back into basic training.

4. Extension of the discussion of proximal factors to such matters as perception and social expectancies, led to consideration of a larger framework in which examination of the separate pieces might presumably be fitted together into a model.

The term *model* had become a fashionable word which, today, frequently replaced what was formerly called *concept* or *theory*. It was stressed that a model, as a construct of the mind, was distinct from what really happened. But, it was argued, there always existed something behind the model, in whatever sense the term was used.

Unfortunately, investigators had a tendency to reach for the complex model even when a simple explanation was at hand. While this seemed to be done consistently, there was no merit in avoiding simple explanations. Thus it was pointed out that difference in the breakdown rates of gunners and pilots might readily be explained by the fact that these two groups were drawn from different social classes. Other armed services data on social class as a predictor of breakdown existed which apparently indicated that to show symptoms or to break down was much less acceptable to the middle classes than to the lower. Again, Dr. Reid's analysis in psychological terms of the considerable rise in road and industrial accidents in England during

the War might be more simply and plausibly explained by the increased pressure for production, poor lighting, worn out equipment, and so on.

Further, it was noted that some of the things that were generally believed to be stressful needed no explanation, because upon individual examination they proved not to be stressful. Thus it was reported that a recent study on the effects of hysterectomy indicated that, where the operation was medically indicated, the subsequent psychological effects were good rather than bad.

5. It was suggested that a promising area for exploration, one in which little work had been done, was that of counter-balancing factors—factors which served to neutralize sources of stress. The studies made by Jules Coleman in the Pacific were cited as an example. Here the evidence seemed to show that the pre-combat morale of a unit was a good predictor of the rates of psychoneurotic and psychotic breakdown during and after combat.

6. The interpretation of hospitalization rates were discussed. It was observed, unhappily, that the index of admissions to mental hospitals were used as frequently to validate a point as they were dismissed as having very little meaning. Dr. Reid's belief that neither the War nor the depression had had a significant effect on admission rates for psychosis was questioned. Clear seasonal fluctuations in rates were observable, with total release rates reaching a peak around Christmas and dropping off in January. Correspondingly, admission rates dropped off in the holiday season. This did not refute the point expressed earlier that times of festivity might precipitate mental disorder; rather the rates reflected the family's desire to have a hospitalized member home for Christmas. It was suggested that the effects of these holiday releases on the patient and also on the family might be interesting to study.

The increase in first admissions in the United States between 1941 and 1945 for males between 20 to 35 (but not for any other age-sex categories) was to be contrasted with the general decline experienced in Europe where a depressive effect on rates might have occurred through disruption of the usual processes that brought about hospitalization. It was conceivable that persons were precipitated into psychosis by the War at an increased rate but were left floating around in the communities unhospitalized. It was clear, however, that this hypothesis would not apply to the military services, where

arrangements for moving sick persons to hospitals remained highly efficient.

In the United States the fact that the military acted as a case-finding agency, sending many patients on discharge from service directly to state and Veterans hospitals could explain the increased male admission rate cited earlier.

In any interpretation of wartime rates of admission, it was important to take note of the population at risk. Rates had been developed which included in the denominator all of the armed forces, or all those not overseas, or none of the armed forces. The resulting variations showed how sensitive these rates were to differences in definition.

A final *caveat* suggested that the circumstances of civilian populations in wartime varied so greatly that comparison of admission rates between the United States and most European countries might not be valid.

As to the effects of the depression, the few long-term series on United States admissions showed continuing increases in rates for all admissions between 1930 and 1937. If the rates of admission for persons without psychosis were to be considered separately, it would be found that they increased markedly from 1933 on. (These same figures showed approximately similar rates for psychosis before and after 1933). This non-psychotic rate increase might in part be a reflection of the marked development of various social agencies and services which took place during the same period. The agencies, seemingly, solved many problems by committing clients even when psychoses were not present. A close and careful analysis of existing data was needed before these negative conclusions could be accepted.

An interesting note on the occurrence of schizophrenia in the United States Navy was reported. The rate has been so nearly constant over the past 30 to 50 years that the Navy no longer publishes case figures since they would reveal the size of its forces.

7. The remark that an exclusively genetic theory of the etiology of schizophrenia was a satisfying one, if only for its heuristic value, brought forth the suggestion that schizophrenia might be divided into two types, one explainable by an exclusively genetic theory, and another for which a mixture of genetic and environmental etiology seemed necessary. The latter idea has been expressed in a number of different studies as the concept of a nuclear schizophrenia, a con-

dition that was separable from the main undifferentiated mass of schizophrenia.

Were schizophrenia exclusively genetic in origin, one might expect no difficulty in tracing the different diagnostic types by pedigree. But, as Dr. Böök pointed out in his paper, attempts to do so had been unsuccessful. The reason might be that the types of schizophrenia were interchangeable, that in one family there might occur, for example, both catatonic and paranoid types.

It was emphasized that the data on schizophrenia indicated a very considerable genetic interaction with the environment, with the geneticist thinking of the disease as a sort of reaction that might occur in many different situations from many different causes—one of the important ones being genetic. As every psychiatrist realizes, a perfectly good schizophrenic syndrome would not be diagnosed as schizophrenia until the psychiatrist could obtain the case history and could observe the patient for some time.

Genetics and the environment were always in interaction. Any consideration of the genetic factor required it to be seen as expressing itself within a particular environment at a particular moment, yet at the same time the environmental factor had to be viewed as being itself subject to change. The effect of environmental changes on cases of sickle cell traits was used as an example. While they were well on the ground, at 2,000 feet they might go into hemolytic crisis. There was no change in their genes, but they were sick or well according to the environment. Certainly social and cultural differences between environments and changes in these environments would produce considerable variation in the picture of general morbidity and mortality. Thus there was no good evidence that there were cases of schizophrenia in which the process was initiated by genes and carried further by genes without any important interaction with the environment.

These remarks were meant, perhaps, to emphasize the need for more concentration of research on the biological side. It was indeed true that genetic explanations of some psychiatric disorders was not very popular amongst psychiatrists, despite such clear examples as phenylketonuria and galactosemia causing mental defect in children. Yet another, and older, example of the role of genetics was diabetes, a condition that had been treated for decades. Still, many psychiatrists continue to feel uncomfortable about a genetic explanation.

DR. REID: I am very interested in what was said because I find this a difficult subject and I would like to take, in sequence, the various points that were raised.

First of all, I think Dr. Lemkau and I are probably the unwelcome supporters of Dr. Böök, for as far as this material goes, it certainly would come down on what I took to be Dr. Böök's side of the fence rather than on the side of the psychoanalyst. (Point 7.)

In reply to the question about physiological responses to stress and whether there have been any other studies done on this (Point 2): I am very interested in this myself because it is something I was concerned with during the War. In fact, one of the first indications I had was from a military tailor who used to come around and measure the pilots for uniforms. He made the comment that men's measurements did change quite perceptibly during operational tours of duty and this was one of the reasons why we embarked on the studies of weight loss in pilots. I have no knowledge of corresponding civilian work. I take it that the difficulty is the inability to find in a civilian context the very severe type of stress that men in the U.S. 8th Air Force or our own Bomber Command, for example, had to sustain in wartime.

I was particularly interested in the discussion on perception and social expectancies as related to the mythology of hazardous assignments in wartime in both the RAF and RCAF (Point 3). However, I think that the explanation for some of the differences in behavior under stress between various categories of aircrew was put into proper focus when it was emphasized that the air gunner, generally speaking, was not drawn from the British upper and middle classes. His appointment was not the result of a special selection procedure but rather the end point in a process of attrition whereby people who were selected under what was called the pilot-navigator-scheme, bomber-aimer-scheme, but who didn't make the grade, very often found themselves as air gunners.

This "stiff upper lip" concept of the middle classes who will not admit to the symptoms of neurosis reminds me of the comment by R. D. Gillespie in his book where he says that the reason for the low neurosis rate among the Scottish battalions in the first World War was that everybody in Scotland knew everybody else; and if a man broke down in combat, the whole glen knew about it and he was disgraced forever afterwards.

I agree, of course, on the importance of the perception of risk as opposed to actual physical risk. There was no question but that at the beginning of the War people felt that the exposed, isolated, and rather obviously dangerous position of the air gunner would have some effect on morale and neurosis. I doubt whether *in fact* this was so: quite often when a bomber crashed the one man to get away was the air gunner because the tail broke off; he was thrown clear and very frequently survived. So that as far as accidents were concerned, the actual risk to air gunners was rather less than it was for other members of the crew.

This perception of risk was also the likely explanation of the very interesting time correlation between the incidence of breakdown and venereal disease after a burst of heavy casualties.

What happened was that men perceived that life was going to be very short and, therefore, that it should be very merry. Thus the reason for time correlations with venereal disease: London was only 100 miles from the bomber bases.

One or two suggestions were made about stresses which might have been included (Point 1.). Both puerperal psychosis and delirium following eye operations are certainly mentioned in the literature, but the difficulty about these observations is that they are anecdotal rather than epidemiological in any strict sense, and I haven't included them partly for that reason.

The loss of spouse and child I did not cover because I think this properly comes within the family studies. I could find no evidence about the effect of festivities on suicides or anything else. The only time correlation of this nature that I know of is in a study showing the high incidence of perforation of ulcers in Glasgow around Christmas which presumably results from the stress on us Scots of giving each other Christmas presents!

The compensation neurosis is, as has been pointed out, an area fraught with all kinds of difficulties, and I know of no epidemiological work which has gone on in this field. As far as I know, both the German literature and our own consist of clinical accounts of cases—of the observations of time correlations between accident and onset, and compensation and recovery. It seems to me that they are rather uncontrolled observations which really should not come into the strict definition of epidemiology.

As to counterbalancing factors (Point 5). This, as far as I know,

is something that has not been much studied in any numerical way, even in the military context. I think that the assessments which were made by psychiatrists of morale were made on a rather intuitive basis, and I don't think there are any studies where there is sound epidemiological and statistical evidence of the presence of counterbalancing factors.