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Letters to the Editors

Is Schizophrenia Disappearing?

Introductory Remarks of the Chief-Editor

Whether psychiatric disorders have increased or decreased during the past century is open to debate. Klerman [7] maintained that during the 1960s an "age of melancholy" had superseded a preceding "age of anxiety". In 1985, Klerman et al. [8] and Lavori et al. [9] showed that cohorts born during earlier decades of this century were less often afflicted by major depressive disorders than later-born cohorts. The data seemed to support the hypothesis of a true increase of depression, either due to a secular trend or to an earlier age at onset. All the studies mentioned remained, however, controversial.

Regarding schizophrenia, Torrey [13] and Hare [6] speculated about secular trends: the incidence of schizophrenia seemed to increase during the past century and more recently to decrease. A new provocative article by Der et al. [3] suggested a substantial lowering of the incidence of schizophrenia. First admission rates for schizophrenia in England and Wales were considerably reduced between 1970 and 1986. Eagles and Whalley [4] reported a similar downward trend of first admission rates for schizophrenia in Scotland in the period 1969–1978. The authors discussed whether these changes reflect a real change in the incidence of the disorder or in diagnostic habits or in referrals to hospitals (outpatient treatment becoming more frequent). They nevertheless concluded that "there probably has been a real fall in the incidence of schizophrenia".

Over the past decades substantial research on the epidemiology of schizophrenia was carried out in Denmark by Erik Strömgren and colleagues, and in Germany by Heinz Häfner's group. The question whether there is a true decrease of the incidence of schizophrenia obviously has a major impact on many fields of psychiatry. Our journal is a good platform for the discussion of this controversial and essential question. The following short communications comment critically on the paper of Der et al. (1990) and should be considered seriously. We hope that these articles will stimulate further research and discussion of the problem.

Brief and mainly critical comments on the papers were published in *The Lancet* (Crow, Prince and Phelan [2, 12]; Graham, Alarcon et al. [1, 5]; Manderscheidt et al. [10]; Der et al. [3]). We are, however, of the opinion that an article whose statements may have a far-reaching impact should be thoroughly discussed by psychiatric experts.

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Dear Sir,

Der et al. [1] raise the question "Is schizophrenia disappearing?", and their answer to this question, of fundamental practical and theoretical importance for psychiatry and for mankind, is affirmative. Their conclusion from admission data from England and Wales for 1952–1986 is that there has been, beginning in the mid-1960s, a substantial decrease in the incidence of schizophrenia. They feel that recent reports from other countries support the conclusion; among the reports quoted are some from Denmark [2, 4]. However, the conclusions by the authors of these and other Danish studies [3] are not in favour of a true decrease of the incidence of schizophrenia; although there has been a substantial decrease in the rate of first admissions of schizophrenia since 1970, this fact can be interpreted as a consequence of factors other than incidence rates: a supposed increasing reluctance among psychiatrists to make the diagnosis schizophrenia, as well as evidence that the diagnosis of "borderline psychosis" is preferred to "schizophrenia". At present, in Denmark, only 50% of those eventually diagnosed as schizophrenic receive this diagnosis at the time of their first admission. Furthermore, it may be of importance that there has been an increase in the excess mortality among schizophrenic patients since 1970. As this increase is particularly marked among the younger males (the group with the largest decrease in admission rates), this would, because of the diagnostic delay, further decrease the number of patients who eventually receive a schizophrenia diagnosis in hospital.

In Denmark there has been a general reduction of first-admission rates since 1970 for any diagnosis (except alcoholism) of the same magnitude as that for schizophrenia. This could be related to the concurrent reduction by more than 50% in the number of beds for psychiatric patients in Denmark.

Schizophrenia admission rates are highly vulnerable to even small changes in referral patterns and other factors influencing admissions. To mention some: schizophrenics' pre-treatment suicides; mildly paranoid schizophrenics permanently treated in private psychiatric practice; treatment of simple schizophrenia in general practice only; latent schizoprenias (or borderline states) treated by practising psychologists or in psychiatric outpatient services; atypical schizophrenias treated and diagnosed as mania; homeless young schizophrenics never seen in the treatment system. If in a population of 100,000 inhabitants one additional case within each of these seven groups occurred, this would reduce the annual first-admission rate by 50%. This example illustrates how difficult it is to conclude the true incidences from first-admission figures.

A salient point is, of course, how "first admission" is defined. The report by Der et al. [1] leaves some doubt as to how the term has been used in their study. It is therefore natural to raise the following questions:

1. Does first admission mean admission of a patient who on first admission receives the diagnosis of schizophrenia?
2. Does a patient who on first admission receives a different diagnosis but later turns out to be schizophrenic at any time come into the statistics as a first admission for schizophrenia?
3. Does first admission mean first admission to that particular hospital, or to any hospital in England and Wales?

It is obvious from Der et al.'s report that there have been substantial changes in the use of the concept first admission during the period studied. What was the nature of these changes? There was an obvious change during the period 1964–1969, after which the definition of the concepts was made "simpler". Was this simpler definition equivalent to that used before 1964?

In conclusion we recognise the very important aetiological implications if a true reduction of schizophrenia incidence is found, but we would be reluctant to accept that such a reduction has been demonstrated on the basis of the findings reported by Der et al. and by others.

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Dear Sir,

Der and his colleagues [5] review DHSS data on first hospitalization to NHS mental illness hospitals and units and conclude that the morbid risk of schizophrenia has been decreasing by nearly 50% in the period 1970–1986. If this finding were true, it would be, frankly speaking, tantamount to a sensation. It would have important implications for our understanding of the aetiology of schizophrenia and would lead to drastic consequences in

the provision of psychiatric care. Politicians might be tempted to cut down the means for psychiatric care and research. Critical comments on Der et al.'s assumption have already been given by Crow [4] Manderscheid and his colleagues [12], Prince and Phelan [16] and Graham [6].

In their response Der et al. [14] refer to a paper by Odegard [15] that indicated a substantial decline in the lifetime risk of first hospital admissions with schizophrenia between 1926 and 1965. This statement is only true if the changes in the method of diagnosis reported by Odegard for this period are not considered. In the period 1926–1935 Odegard found a cumulative lifetime risk of "schizophrenia" of 1,936 in males and 1,799 in females per 100,000 until the age of 90 years based on the National Norwegian Case Register. In the 5-year period 1961–1965 the lifetime morbid risk until 90 years of age, namely of the diagnoses schizophrenia (ICD no.295), paranoid psychosis (ICD no.297), and acute paranoid reaction (ICD no.298.3) amounted to 1,894 in males and 1,822 in females per 100,000. Der et al. start from the same three subgroups of schizophrenia. According to the diagnostic system applied in Norway, the diagnoses ICD no.297 and 298.3 were not yet available between 1926 and 1935. At that time the diagnosis of schizophrenia also included the other two diagnoses and thus presumably the same category of cases.

In a second study, the results of which are available only as unpublished manuscript from the Norwegian Register, Astrup (unpublished work) considered the change in the age composition of the Norwegian population. He obtained an age-adjusted lifetime risk of hospital admissions for schizophrenia of 0.23% for both sexes between 1926 and 1930 (the introduction of the Kraepelinean diagnostic system), calculated on a 1-year basis, and a rate of 0.24% for the diagnoses ICD no.295, 297, and 298.3 in the period 1977–1978. Obviously, these Norwegian data do not confirm a decreasing trend.

Temporal effects of the lifetime morbid risk are an interesting but difficult question to resolve. In a study including 2289 first-degree relatives of 523 probands suffering from DSM-III "major depressive disorder", Klerman et al. [10] obtained the impressive finding of a steady increase in the lifetime morbid risk of successive birth cohorts and simultaneously a decrease in age of onset since the turn of the century. These findings have meanwhile been confirmed by ECA data and in studies conducted in some smaller European samples [11]. Has there actually been a continuous increase in the morbid risk of major depressive disorder in the populations of the developed countries since the turn of the century? If the first hospitalization data for England and Wales reflect the actual morbidity trend, Der and his colleagues would have to contradict this, since they found a decline of 35% in the rates of affective psychoses between 1970 and 1986 with the same type of evidence as they did in the decline in schizophrenia rates.

Several years ago, Hare [9] and Torrey [18] still assumed an increase in the true morbid risk of schizophrenia in the 19th century and attributed it to the rise of civilization and to the contribution of infectious factors to the aetiology of the disease. In agreement with Boyle [2], Der et al. suggest that decreased frequency or severity of infectious diseases may be relevant to their findings. They further assume that, since perinatal and prenatal brain damage is more frequent in males, the decline in complicated delivery may result in a substantial fall in first-ever contacts for schizophrenia by males, as the data of the Oxford Case Register suggest (see Alarcon et al. [1]). However, their own data show a stronger decline in the rates for females (50%) than in those for males (40%). We therefore feel that, before speculating about possible causal explications, one should examine with much diligence the contribution of artefacts to the observed tendency. In this context it is very important to consider whether one may derive those of true incidence from the trends of treated incidence.

To give an example: a steep increase from 5% to 11% in the rate of all patients who ever contacted a medical service and were diagnosed as mentally disordered was recorded by the National Health Service in England and Wales between 1955/56 and 1970/71. However, Crombie et al. [3] came to the conclusion that the increase was largely attributable to changes in the diagnosing habits and not to a general rise in psychiatric morbidity.

Diagnostic artefacts have already been discussed in *The Lancet* by the correspondents. The data of the Danish National Case Register cited by Der and his colleagues show a decline in first admission rates for schizophrenia of about 37% between 1970 and 1984, but they also reveal an increase of nearly the same extent in schizophrenia borderline states, paranoid and unspecified psychoses in the same period [13]. For 1965, the cumulative case register of Mannheim, a German city with about 300,000 inhabitants, lists a first-admission rate of 0.54 for patients aged 14 years and over with the diagnosis schizophrenia and related disorders (ICD 295, 297, 298.3 and 298.4) and 15 years later a rate of 0.56 with the same criteria. Also, in 1989/1990, we screened all first admissions aged 15–54 years in a population of 1.5 M who met the criteria for CATEGO class S+ (nuclear syndrome) according to PSE interviews. For this very restrictive diagnosis, we found a yearly rate of first admissions of 0.09/1000, which corresponds exactly to the average rates of S+ cases of the same definition as yielded by the WHO Collaborative Study on Determinants of Outcome of Severe Mental Disorders [17]. Could a higher degree of civilization or better living conditions in the United Kingdom be supposed to explain the difference in the trends observed in the two countries?

Changes in the availability of psychiatric care facilities have been shown to result in changes in the pattern of service utilization. In Mannheim the expansion of mental health services by the opening of the Central Institute of Mental Health led to a twofold increase in all psychiatric contacts in a period of only 7 years [8]. A strong reduction in the capacity of individual sectors of mental health care presumably entails a declining trend in the utilization of the respective sectors, e.g. inpatient services, even in first onsets. Prince and Phelan [16] assume that many patients who were formerly admitted to

psychiatric hospitals are now being treated in the community. In fact, it is probable that the successful policy of the DHSS aiming at a drastic reduction of psychiatric beds and the close-down of mental hospitals may have resulted in a change of attitudes of patients and psychiatrists towards hospitalization of mental patients. It may have led to an increase in the proportion of schizophrenics in specialist or nonspecialist outpatient departments and possibly also in non-medical care. As long as this proportion is unknown, trends towards falling hospital admission rates are difficult to interpret.

The assumption of Murray and his colleagues that community care would lead to a relative increase in schizophrenics in comparison with patients suffering from milder disorders who are better provided with outpatient care is no more plausible. Of the high prevalence of neurotic and affective disorders probably only a small proportion of patients with insufficient response to extramural treatment are admitted to hospital, because of the small number of beds for psychiatric patients. For these reasons the nearly parallel decline in recorded first hospital admissions of 44% for neurotic and personality disorder, of 35% for affective psychoses, of 50% for schizophrenic females, and of 40% for schizophrenic males in the period 1970–1986, does not support the assumption of a decrease in true morbidity of schizophrenia. Reliable statements on morbidity trends in populations also require the exclusion of artefacts in the denominator. First-admission rates for all mental illnesses, on which Der et al. base their calculations, are not a suitable denominator for calculating morbidity rates. Distorted by various morbidity- and service-related factors, they do not reliably reflect the demographic composition of the population or changes in the risk period.

Currently there are no reliable data that would confirm a trend towards a change in the morbid risk of schizophrenia. The misinterpretation of trends in hospitalized psychiatric morbidity has been significant in history. The steep increase in psychiatric bed occupancy in the German Reich – in Prussia, for example, a 3.7-fold increase between 1881 and 1910 – led to the false assumption of an enormous rise in psychiatric morbidity [8]. This simple misinterpretation is difficult to understand nowadays, especially since Tuke [19] found an epidemiologically correct interpretation for the same phenomenon observed in British mental hospitals (Prince and Phelan [16]). The misinterpretation that this phenomenon was a consequence of the degeneration of the German population went to officially justify eugenic laws in the Third Reich. Fortunately, today there is no longer the risk of such fatal consequences of mistaking of trends in the treated morbidity for trends in true morbidity of mental disorder. Nevertheless, the risk of providing a convenient reason for the reduction of funds urgently needed for the care of schizophrenics should be taken seriously.

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Reply: The Incidence of Schizophrenia, and of Causal Environmental Factors, Varies in Time and Place

Professor Häfner and Dr. Gattaz, and Drs. Mortensen and his colleagues raise a number of issues concerning our paper entitled "Is Schizophrenia Disappearing?" We are well aware of the difficulty in establishing genuine trends in the incidence of schizophrenia, given the likelihood that there may have been changes in a number of potentially confounding variables during the period in question, i.e. the mid-1960's to the mid-1980's.

It is convenient to divide the possible sources of artefact into three main groups: those concerned with changes in diagnostic habits, those concerned with nosocomical factors, and those related to population shifts. Let us consider the first of these. In our follow-up letter to *The Lancet* (May 19th, page 1214) we indicated that the apparent changes in the frequency of schizophrenia in Norway between 1926 and 1965 reported by Odegaard could well have been due to changes in diagnostic practice, as suggested by Odegaard himself. Naturally, we examined the data for England and Wales lest the more recent decline in first admission rates could have arisen in a similar fashion. However, we were unable to find a diagnostic category for which the rates showed a reciprocal change with those of schizophrenia. Furthermore, similar apparent declines in first admission rates for schizophrenia have also been reported recently for Scotland, Denmark, New Zealand and Australia, but there has been no consistent evidence of a compensatory rise in the frequency of another diagnostic group (Gupta and Murray 1991). Whilst diagnostic artefact cannot be excluded by this argument alone, the existing evidence suggests a genuine decline in the first admission rate for schizophrenia.

Nevertheless, the issue of the precise definition of a "first admission for schizophrenia" which was raised by Mortensen and his colleagues is an important one. In our own data for England and Wales, "first admission" refers to cases in which a patient is admitted to hospital for the first time in his or her life, and is given a diagnosis of schizophrenia at that time. We pointed out in our paper that the question used to ascertain whether or not the admission was genuinely a first or a readmission was changed in 1970, and we adjusted the data for the period 1964–1969 to take account of this fact. We further indicated that the form of the question after 1970 was similar to that employed during the 1950's.

We know that Munk-Jørgensen (1987) and colleagues have examined the effect of varying definitions of "first admission for schizophrenia" on the incidence of schizophrenia in Denmark. Our understanding is that varying the way in which this was defined to include individuals who were diagnosed as schizophrenic subsequent to their first admission, did not alter the overall downward trend in incidence. Thus, Mortensen and his colleagues appear to have elegantly answered their own doubts.

It is true that first admission rates are likely to be sensitive to nosocomial influences such as altered thresholds for admission. However, two British studies (Eagles et al. 1988; de Alarcon et al. 1990) have demonstrated a decline in the frequency of schizophrenia using first contact rather than first admission rates. Neither changes in treatment practice nor in the availability of psychiatric beds can account for this decline. There has been in Britain, as in a number of other countries, a decline in the total number of psychiatric beds. However, contrary to the assertions of Häfner and Gattaz, this is accounted for by a decrease in the number of chronic in-patients, and the number of beds available for acute admissions did not diminish over the period that we studied.

The composition of the base population can affect crude estimates of the incidence rate of schizophrenia, but several of the studies (e.g. Eagles et al. 1988; Munk-Jørgensen and Jørgensen 1986) which demonstrated a decline in these rates, employed age-standardization, thus excluding this explanation. We note with interest the failure to find a decline in the first admission rate for schizophrenia in Mannheim from 1965 onwards, and we note that a study from Yugoslavia also reported a static rate (Folnegović et al. 1990). We would like to emphasise that we do not consider that there exists a uniform decline in the incidence of schizophrenia, either between, or indeed within, different countries. For example, within the United Kingdom, studies employing psychiatric registers have demonstrated differences in both the absolute incidence, and trends in incidence, of schizophrenia. Thus, Eagles et al. (1988) and de Alarcon et al. (1990) showed a decrease in the incidence of schizophrenia in Aberdeenshire and Oxford respectively. However, Castle et al. (1991) have demonstrated that in Camberwell in South London, no such decline has occurred. This latter appears to be accounted for by an influx into the area of Afro-Caribbean immigrants who showed an incidence of schizophrenia between four and eight times that of the indigenous British population.

The real importance of the differing incidence of schizophrenia between times and between populations is that such differences suggest that underlying aetiological factors may also vary in their frequency. In this regard, there is now considerable evidence that maternal influenza is a risk increasing factor for schizophrenia. Thus, in both Helsinki (Mednick et al. 1988) and England and Wales (O'Callaghan et al. 1991), the "Asian Flu" epidemic of autumn 1957 was followed five months later by an almost doubling in the number of births of individuals who later became schizophrenic. Furthermore, influenza epidemics in Denmark over a period of 39 years (Barr et al. 1990) and in England and Wales over a period of 22 years (Sham et al. 1991) were consistently followed by an increase in the number of births of individuals who later developed schizophrenia. Since maternal exposure to viral epidemics during pregnancy is likely to vary geographically and temporally, it is not surprising that the incidence of schizophrenia varies by time and country, and within countries by social class and ethnic group.

Nevertheless, many psychiatrists persist in the belief that schizophrenia has a uniform incidence in time and place. If this were the case, it would be unique among common diseases. Barker (1990) has demonstrated that the major common diseases of Western civilisation have undergone considerable changes in their incidence throughout the 20th century. There is now increasing evidence that both the incidence of schizophrenia, and the incidence of at least one of the underlying aetiological factors, varies considerably. This view holds out considerable hope for prevention e.g. through immunisation of young women contemplating motherhood. Such a view stands in sharp contrast to the pessimistic conclusions of our critics who apparently believe that schizophrenia is an immutable accompaniment of the human condition.

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