Abstract

In response to my discussion of sex differences in concordance rates for schizophrenia in twins, Lewine (1979) noted that male schizophrenics are hospitalized earlier than females. He questioned whether females had been followed through the risk period. To clarify, I calculated the age at followup of discordant twins in three studies. In Gottesman and Shields' (1972) study, the majority of both male and female co-twins were still under the age of 45. In contrast, all but a few of Kringlen's (1967a, 1967b, 1968) and Fischer's (1973) co-twins were 45 or over. Their findings support the conclusion that recent twin studies show no significant sex differences in concordance.

In his reply to my remarks in Schizophrenia Bulletin, Vol. 4, No. 1, on sex differences in concordance rates for schizophrenia (Samuels 1978), Lewine (1979) raises several important theoretical and methodological issues. He cites as an example my discussion of Rosenthal's (1962) paper. Lewine assumes that I intended to review research findings with respect to concordance by sex in all first degree relatives. I did not. The objective of my brief review was to examine sex differences in concordance rates for twins.

Lewine then proceeds to describe the problems involved in the use of consecutive admissions as a sampling technique. He notes that male schizophrenics are hospitalized earlier than female schizophrenics. Therefore, the use of a consecutive admissions strategy might underestimate the incidence of schizophrenia among females who would still have a considerable portion of the risk period ahead of them. Lewine's point with respect to gender-related variation in the risk period is well taken. To clarify, I have informally reanalyzed Gottesman and Shields' (1972) data.

The first requirement was to determine the limits of the risk period for women. There is considerable variation in the literature regarding the age at which admissions for female schizophrenics begin to exceed those for men. Hartmann and Meyer (1969), for example, found in their sample of hospitalized schizophrenics that the percentage of males was significantly higher than in the general population until age 50. In contrast, in the Monroe County data reported by Kramer (1978) both incidence and prevalence figures show a higher rate for males in the under 15 and 15–24 age group, comparatively even rates in the 25–34 age group, and a reversal toward higher rates for females in the 35–44 age group. For the purposes of this analysis, I have used the cutoff point of 45 suggested by the Monroe County data.

Gottesman and Shields (1972) found no significant sex difference in the Maudsley Hospital study. I reviewed their individual case histories to determine the age of the still discordant pairs in both the monozygotic (MZ) and dizygotic (DZ) series at the time of followup. The mean age of the 24 discordant male twins was 33.83 years and of

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the 20 discordant females, 41.75 years. Forty percent of the females were 45 or older, as compared with only 17 percent of the males. Fifty-eight percent of the discordant males were under the age of 35, as compared with 25 percent of the females. A significant number of Gottesman and Shields’ discordant twins, therefore, have not passed through the risk period. Admittedly, this mode of analysis is overly simplified, and does not do justice to Gottesman and Shields’ data. They have noted that risk is related to age of onset in the proband as well as to chronological age, particularly for MZ twins. The point I wish to emphasize is that although a significant proportion of the subjects still at risk are female, at least as great a proportion are male.

Lewine makes no reference in his critique to two other twin studies I cited in which no significant sex difference was found. Both Kringlen (1967a, 1967b, 1968) and Fischer (1973) used twin registers and registers of psychosis rather than consecutive admissions. In both studies, the age-risk factor is minimized. In Fischer’s study, for example, discordant pairs were under observation for an average of 25 years after the first admission of the proband to a psychiatric ward. These studies would seemingly meet Lewine’s requirement for longitudinal analysis throughout the risk period.

Kringlen (1967a, 1967b, 1968) focused specifically on the issue of sex and concordance. He reported (1967a) three separate analyses by sex: concordance with respect to (1) functional psychoses and (2) schizophrenic and schizophreniform psychoses in male and female MZ twins only, based upon personal investigation and using a broad concept of concordance. He concluded that his study did not show any statistically significant differences in concordance rates for the two sexes. Using Kringlen’s case histories (1967b), I calculated the age of the discordant MZ and DZ twins in the schizophrenia and schizophreniform psychoses series combined. The mean age of the 18 discordant males was 46.66 years, and of the 14 discordant females, 53.38 years. Seventy-nine percent of the women and 61 percent of the men were 45 years of age or older.

Fischer (1973) also gives individual data on her subjects, so it was possible to calculate the age of the discordant pairs at the time they were lost to observation. For the MZ and DZ series combined, the mean age of the 22 discordant males was 62.18 years and of the 22 discordant females, 53.50 years. Seventy-seven percent of the discordant females and 100 percent of the discordant males were 45 years of age or older. In summary, the data suggest that all but a few of Kringlen’s and Fischer’s subjects have passed through the risk period. Their data support my observation that recent twin studies show no significant sex differences in concordance.

Secondly, Lewine states that I have failed to distinguish between results that are due to methodological errors (artifacts) and those due to social conditions (findings). Lewine implies that this is an “either-or” matter. I would suggest that a research strategy can maximize or minimize the effect of social conditions upon the variable under investigation. The use of consecutive admissions, for example, minimizes the sampling bias toward the predominantly female residents of chronic wards, a phenomenon itself at least partly social in origin. Lewine ignores the fact that we often become aware of so-called methodological errors only as we become aware of social conditions. Klorman, Strauss, and Kokes’ (1977) discussion of the role of sex-related variables in measures of premorbid adjustment illustrates well the complexity of this issue in schizophrenia research.

Finally, Lewine states that in “explaining away” sex differences, I have inadvertently glossed over the potential importance of biologically and socially determined sex differences as suggested by large, consistent sex differences in other areas of research. This criticism merits little comment. Questioning the validity of a previously reported finding on the basis of new data is not “explaining away.” However, Lewine has pointed to one of the least examined areas in schizophrenia research. For example, Wynne, Cromwell, and Matthysse’s (1978) volume on schizophrenia allows only a few pages to the subject of sex differences. The unique contributions of Flor-Henry (1974) receive no mention at all. I plan to review this literature at greater length in a future issue of Schizophrenia Bulletin.

References


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