It is possible that some of the differences found here are differences in intent, not just in accuracy.

While the authors express concern about the symptom concordance rates they found, they offer no basis for comparison, and there are few empirical guidelines about what should be achievable by recall after a number of years. It would have been instructive if the instrument originally used to ascertain these psychotic symptoms within the previous month had been adapted to ask about the total life history and had been administered blindly by a different psychiatrist after a similar interval. Could such an instrument or, for that matter, any method of independent examination fare better than approximately 80 percent reendorsement of symptoms after 11 years? If not, the problem is not with instrumentation, but with the fact that psychotic patients sometimes do not recall their delusions and hallucinations or are unwilling to admit them, or that there were some errors in the original assessment, or both.

Whatever the reason for the concordance findings reported in the article, is it true that results in this range would lead to a serious underestimation of the prevalence of psychotic disorders? As the authors point out, they do not have the data to answer this question. Prevalence estimates are affected by two types of error: (1) false positives, about which the authors have no information because all of their respondents were positive cases, and (2) false negatives, where the error rate according to their estimates will be somewhere between 5 percent and 36 percent, depending on the mix of hallucinations and delusions in the population of psychotics. They argue that false positives will not compensate for false negatives because respondents are more likely to deny symptoms than to invent them. That is obviously true, but it is also true that respondents sometimes misunderstand questions or interviewers sometimes misunderstand or miscode responses, and both types of error do occur. The fact the authors overlook is that when a disorder is very rare, as schizophrenia is, a very small proportion of false positives compensates for a substantial proportion of false negatives, because the first proportion applies to a large sample, and the second to a small sample. On the basis of much prior research, the prevalence of schizophrenia is regarded to be less than 1 percent in the general population. If this is approximately correct, and if the DIS detects 81 percent of true schizophrenics, how large a false-positive rate would compensate for the false-negative rate of 19 percent? The answer is .19 percent, a figure just one-one hundredth as great. Our major concern in general population studies is that we may overestimate rare disorders, not underestimate them, just because a very small false positive rate can so easily overwhelm the false negatives.

As Pulver and Carpenter point out, apart from prevalence estimates, false negative errors can produce problems in any research which is predicated on the accurate classification of cases. But again, the questions are whether the reported results are bad and compared to what standard. The several reasons the authors list that respondents might underreport previously experienced psychotic symptoms apply equally to any interview examination. The question is whether the lay interview/DIS approach performs less well in ascertaining such information than other types of examination. Regarding this question, the article by Pulver and Carpenter has nothing to offer.

References


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The Authors Reply:

Helzer and Robins raise several issues about our article (Pulver and Carpenter 1983) which merit response. Our views differ somewhat on the point of our report. We were interested in identifying a method to ascertain the history of lifetime symptoms in relatives of patients with psychotic illness who participate in pedigree studies at our center; the DIS/lay interviewer approach appeared to be promising. Reliability had been demonstrated in some circumstances, and Robins et al. (1981, 1982) used these reliability data to support an argument for validity. However, because validity of the report of lifetime psychotic experiences had not been assessed, we did not feel confident using this (DIS/lay interviewer) method in studies where accurate determination of psychopathologic features and diagnosis of the individual case was crucial. In the absence of any literature bearing on this point, we
sought empirical data by using the DIS/lay interviewer method at 11 years following assessment of patients with the Present State Examination (PSE) (Wing, Cooper, and Sartorius 1974). Compared to the index assessment with the PSE, patients often failed to report psychotic symptoms which they had earlier admitted. We suggested reasons for this and possible implications—only one of which was the possibility that an underestimation of prevalence of psychotic disorders in populations might accrue.

We regret the small number of available subjects, but to our knowledge these are the first 43 cases reported examining the DIS assessment of lifetime psychotic features in previously evaluated patients, and the available subjects do appear representative of the original 68 subjects (see Pulver and Carpenter 1983). The statistical assessments justifying the combination of telephone interviews with in-person interviews are also provided in the article. We are not satisfied with telephone interviews any more than we are satisfied with our inability to obtain a 100 percent rate for long-term followup, but the data do not appear unduly skewed.

Helzer and Robins’ discussion of “hit” rates is interesting and may reassure investigators asking only the question of whether any psychotic feature was ever experienced. However, in the article, we noted that our study design was clearly biased in favor of the DIS method by nontrivial factors, such as treating questionably present psychotic symptoms as present on the DIS and absent on the PSE, asking for lifetime symptoms on the DIS but only past month symptoms on the PSE, and using subjects who had previously revealed psychotic symptoms to our interviewers. In any case, more complex questions such as the nature and constellations of clinical features, special forms of psychotic experience germane to differential diagnosis, or adequate characterization of psychopathology in individual cases remain problematic even if the “hit” argument is correct. If the DIS method frequently fails to ascertain the presence of previously admitted psychotic experiences, a problem in assessment of psychopathology exists!

Helzer and Robins are kind enough to point out that Strauss and Carpenter are known to have used “softer” criteria than diagnosticians in the other eight participating centers of the International Pilot Study of Schizophrenia (IPSS). It is not entirely clear that this was the case concerning diagnosis; but, even if this were true, does it not suggest that the reliability or validity of judgments concerning psychotic features is inadequate for the purposes used in this study? One advantage of having a detailed symptom assessment is to provide a basis for comparison beyond final diagnostic assignment. It should be at least noted that at 2- and 5-year followup, patients diagnosed as schizophrenic using DSM-II criteria (American Psychiatric Association 1968) in the U.S. center ran courses similar to those in London, Aarhus, Prague, and even Moscow (the other center suspected of being “deviant” in the diagnosis of schizophrenia). More favorable course of illness was observed in the remaining four centers. Perhaps Helzer and Robins are asserting that Strauss and Carpenter are well known to have “overrated” psychotic symptoms. This is surprising and undocumented news. Initial ratings of symptoms by Strauss and Carpenter were done according to PSE procedures. Symptoms were rated present only when (1) the patient gave indication of having the experience; (2) the experience was not consistent with common experience in the patient’s subculture; (3) a clinical judgment was made that this experience was psychopathological; and (4) the symptoms were conceptually consistent with the psychotic feature being ascertained. In addition, there are some data which contradict the assertion made by Helzer and Robins. Within the IPSS, hallucinations and delusions were found not to be very vulnerable to rater bias. The U.S. diagnosticians (Strauss and Carpenter) were found to rate hallucinations and delusions more stringently than raters in the other eight centers (World Health Organization 1973). We doubt, therefore, if critical appreciation of the strengths and weaknesses of the DIS/lay interviewer method will be furthered by suggesting the issue is modern DSM-III (American Psychiatric Association 1980) style rigor versus outdated softness.

We share with Helzer and Robins the hope that false positives and false negatives will balance each other in estimating population prevalence of psychotic illnesses. Their argument is cogent, but whether their optimism is merited must be determined empirically. This question cannot be addressed in our small, patient-focused study, but it must be resolved before interpreting epidemiological data based on the DIS/lay interviewer method. In our article, we concluded simply that “Data are not yet available to judge the extent to which the false positives for psychosis would counterbalance the false negatives in large-scale epidemiologic studies. However, the false negative problems will seriously flaw research relying on accurate classification of individual cases (e.g., family pedigree studies)” (p. 382).
Family Therapy in Schizophrenia

Family Therapy in Schizophrenia, edited by William R. McFarlane, has been recently published by The Guilford Press (200 Park Avenue South, New York, NY 10003). Although family therapy originated nearly three decades ago through early efforts to understand the etiology of schizophrenia, it was all but abandoned in later years, as biological or constitutional factors were shown to contribute more to the occurrence of schizophrenia than family psychopathology. Though the etiology of the disorder is still understood in constitutional terms, recent findings suggest that the family—the key social unit in the patient’s life—may have a substantial impact on treatment.

Family Therapy in Schizophrenia focuses on approaches developed since 1975. These approaches, brought together here for the first time, differ from earlier ones, according to McFarlane, in two important ways: “They seem to have major therapeutic effects on the schizophrenic process, beyond those achievable with drug therapy; and they all—with the exception of the systemic variety—start from a major expansion of family systems theory that includes extrafamily factors.”

This volume presents practical strategies—developed by leading family therapists and researchers—for involving families of schizophrenics in the therapeutic process. The book is addressed to family clinicians, psychiatrists, rehabilitation counselors, psychiatric nurses and social workers, hospital and clinic administrators, and students in training for years to come.

References


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