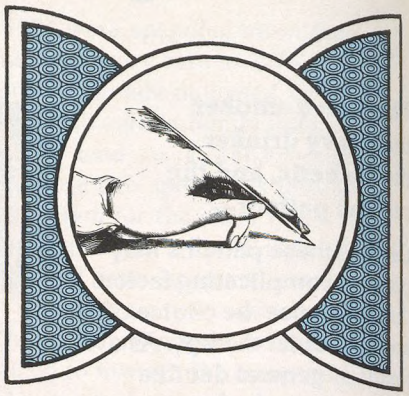
Letters to the Editor



The Journal welcomes Letters to the Editor; if found suitable, they will be published as space allows. Letters should be typed double-spaced, should not exceed 400 words, and are subject to abridgment and other editorial changes in accordance with journal style.

Psychosocial Problems

To the Editor:

I read with great interest the article "A Study of Patients with Psychosocial Problems in a Family Practice" by Michael Brennan and Amy Noce *(J Fam Pract 13:837, 1981)*. The results were fascinating; however, I have questions concerning their analysis and interpretation.

The authors offer two interpretations for their results. I propose a third interpretation, which is supported by a large body of literature found in medical sociology. This literature was not cited by the authors. Brennan and Noce's study group contained approximately twice the number of individuals with lower socioeconomic status (as evidenced by government assistance) than their control group. Thus, a third interpretation might be that their results were confounded by socioeconomic status. The greater hospitalization, number of operations, number of gynecologic operations, and the increase in certain chronic organic and psychosocial problems could be accounted for by socioeconomic status (SES) rather than the presence of interpersonal problems.

David Mechanic, in the second edition of his *Medical Sociology*, summarizes much of the work done in social epidemiology. This work shows the rather consistent finding of the inverse relationship between SES with morbidity, mortality, hospitalizaion rate, and length of hospital stay.[1] There are several theories offered for this relationship, such as toxic environment, downward drift hypothesis, and genetic pooling. The relationships between SES and the variables mentioned previously are probably very complex with many confounding variables. The reason for the relationship poses an interesting research question. Matching for SES as well as other demographic factors would provide a design for starting to address these problems.

There also exist several excellent studies that indicate an inverse relationship between SES and mental illness. In a study of mental illness in the community of New Haven, Hollingshead and Redlich demonstrated that diagnosed prevalence, type, and treatment of mental illness are strongly associated with social class position.[2] Other population studies show increased rates of symptomatology with lower SES.[3,4] The theories for this relationship are the same as mentioned previously.

P values are listed, but the manner in which they are derived were not. It would appear that chi-square values were obtained on percentages. Hopefully it is an editorial slip, since chi-square compu-

tations are dependent on columnar totals, not percentages.[5]

I look forward to a future publication by Brennan and Noce in which there are controls for socioeconomic factors.

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References

1. Mechanic D: Medical Sociology: A Comprehensive Text, ed 2. New York, Free Press, 1978, pp 163-169, 189-202
2. Hollingshead AB, Redlich FD: Social Stratification and Psychiatric Disorders. In Spritzer SP, Denzin NK (eds): The Mental Patient: Studies in the Sociology of Deviance. New York, McGraw-Hill, 1968, pp 102-111
3. Srole L, Thomas A, Rennie C: Mental Health in the Metropolis: The Midtown Manhattan Study. New York, McGraw-Hill, 1962
4. Leighton DC, Harding JS, Macklin DB: The Character of Danger: Psychiatric Symptoms in Selected Communities. New York, Basic Books, 1963
5. Bahn AK: Medical Statistics. New York, Grune & Stratton, 1979, p 79

Pneumococcal Vaccination

To the Editor:

I would like to make several comments concerning Dr. McCue's recent article, "Adverse Reactions to Simultaneous Influenza and Pneumococcal Vaccination" *(J Fam Pract 13:175, 1981).*

The conclusions, using the technique of patient interview, seem valid. I would like to offer one caution, however, in the use of pneumococcal and influenza virus vaccine. The caution is against reimmunization with pneumococcal vaccine and is based upon the immunologic properties of the pneumococcal vaccine.

Pneumococcal polysaccharides, when injected into otherwise healthy adults, induce long-lived antibody synthesis. Studies extending as long as five years show that a decline from about one half of the maximal level to most of the types within the vaccine can be expected. This level is still considered protective. Reinjection of adults with the 14-valent vaccine, containing its comparatively high concentration of antigen (700 μg of total polysaccharide) may induce considerable local reactions and systemic reactions. The effect of the reinjection upon the antibody level is minimal. This failure to induce a "booster" response and the long duration of antibody synthesis in adult humans is characteristic of so-called T-cell independent antigens. Therefore, repeated immunization with pneumococcal vaccine in otherwise healthy adults, at least within five years of initial injection, is contraindicated.

In contrast, repeated immunization with influenza vaccine, due to its antigenic shifts which occur among infecting strains, is recommended. Therefore, we are concerned about the linkage, even though inadvertent, which occurs when studies of simultaneous immunization with two vaccines are published. It is for this reason that we mention that reinjection of the pneumococcal vaccine is contraindicated, whereas annual injection of influenza vaccine in high-risk groups is recommended.

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The preceding letter was referred to Dr. McCue, who responds as follows:

The comments from Dr. Robbins are appreciated. In my article on adverse reactions to simultaneous influenza and pneumococcal vaccination, I did not include a disclaimer that pneumococcal vaccination should not be administered more than once. It is currently not necessary and may not be safe to give pneumococcal vaccination more than once. Physicians must be careful to keep records of pneumococcal vaccination so that when time for influenza revaccination occurs, the pneumococcal vaccination is not also repeated.

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Continuity of Care

To the Editor:

One hesitates to challenge the well-documented paper of Wall *(Wall EM: Continuity of care and family medicine: Definition, determinants, and relationship to outcome. J Fam Pract 13:655, 1981),* but I cannot agree that "no evidence exists to support the contention that such an attitude is conducive to better health care." If he had said that the value of continuity has not been proved at the P = .001 level, there could have been no disagreement, but both clinical experience and the weight of published studies point to a contrary conclusion.

Wall's paper cites many reports in which benefit has been apparent, although, as he notes, methodological defects have been evident in some of them. Studies that appear to show no improvement are often

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equally flawed. Roos et al[1] examined physician billings to the Manitoba Health Services Commission, used them to construct an index of provider and system continuity, and attempted to relate them in a meaningful way to the effect of tonsillectomy on the incidence of respiratory infections. The number of possible confounding variables in such a study is large, and its failure to demonstrate beneficial effects should come as no surprise. Furthermore, it is hazardous to attempt to describe the universe of human behavior while sitting at a desk shuffling computer cards.

Hanchett and Torrens[2] described a project in which nursing home visits were made to clinic patients with congestive heart failure. Hospital days for the treatment of relapses were significantly decreased, and hospital days for other health problems showed an increase of similar magnitude. This study has been cited in the literature as a "failure" because total hospital stays did not change significantly, but in fact there were benefits in earlier recognition of congestive heart failure relapses and improved diagnosis of concomitant health problems.

There appears to be a risk that some readers may take from context Dr. Wall's conclusion that "continuity remains a convenient slogan of humanistic dedication" and say, "See? Family physicians admit that their rhetoric is hollow." One can only hope that readers will be persuaded in a different direction by personal experience and by reading some of the favorable reports Wall has cited. There is clearly nothing in his paper that would justify abandoning continuity as a concept to be practiced by family physicians and other primary care practitioners, to be taught to resident physicians, and to be commended to physicians in all specialities.

Wall's paper points up the need for further research in this area and provides a useful challenge to family medicine to do the necessary work. It also provides an indication of the difficulties that lie ahead. Few of the reported studies have come from family physicians, and few reflect the unique character of family medicine. Many have involved clinic populations for whom continuing, comprehensive care has seldom been available previously. The benefits of continuity are inherently slow to appear, difficult to measure, and hard to separate from the effects of other influences in the primary care milieu. It is not simply a matter of testing mice in cages or reading numbers from a digital display. Human motivation is complex; behavior changes slowly; confounding variables are ubiquitous, and objective evidence for the value of continuity of care will be slow in coming. As Rogers and Curtis have noted, "Continuity of care is multidimensional and will probably require several different approaches both in definition and measurement."[3]

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References

1. Roos LL, Roos NP, Gilbert P, et al: Continuity of care: Does it contribute to quality of care? Med Care 18:174, 1980
2. Hanchett E, Torrens PR: A public health home nursing program for outpatients with heart diseases. Public Health Rep 82:683, 1967
3. Rogers J, Curtis P: The concept and measurement of continuity in primary care. Am J Pub Health 70:122, 1980