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.

The Practice Denominator

To the Editor:

Nelson and his group at Dartmouth are to be congratulated for their excellent pioneering work in the development of their medical information network of 44 rural practices.[1] Those of us interested in similar ventures can only be encouraged by their success.

The wide variety of issues addressed by the Cooperative Information Project represent only a minute fraction of the potentially valuable studies that can be conducted using data gathered by a network of community practices. Unfortunately, this potential remains limited by our current inability to estimate the number of persons served by an individual practice, ie, the practice denominator. Without such denominators, true morbidity rates cannot be calculated for each practice; hence, valid interpractice comparisons cannot be made.

Our work on this problem suggests that an acceptable estimate of the practice denominator may be derived using the "utilization correction factor method."[2] This method, based on the observation that 86 percent of persons in the United States have seen a physician within the previous two years, simply assumes that for every 86 individuals who visited a particular practice within the past two years, there were 100 served by the practice.

Refinement of this estimate may be possible through calculation of utilization correction factors within age- and sex-specific groups in the practice population.

If this method is shown to provide reasonable estimates of practice denominators, one would be able to discuss "practice panel sizes" in terms of patients served by the practice rather than in terms of the number of patients who visited within the previous two years. This would then permit the calculation of population based morbidity rates which could be compared among practices.

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References

1. Nelson EC, Kirk JW, Bise BW, et al: The cooperative information project: Part 2: Some initial clinical, quality assurance, and practice management studies. J Fam Pract 13:867, 1981
2. Cherkin DC, Berg AO, Phillips WR: In search of a solution to the primary care denominator problem. J Fam Pract 14: 301, 1982

Management of Cleft Palate

To the Editor:

The primary care physician frequently is the initial contact for the parents of an infant born with a cleft lip or palate. After the initial diagnosis, many of these infants and children are referred and evaluated by a cleft palate team consisting of medical, surgical, dental, audiologic, and speech specialists. The team's recommendations are reported to the primary care physician, who then helps the family to understand and implement these recommendations so that the patient can derive the greatest benefit from these evaluations. To assess the effectiveness of this management, we sent questionnaires to 67 practicing pediatricians in the St. Louis area with 46 (68.7 percent) completing the survey. These physicians, ranging from 31 to 70 years of age, included those who had diverse medical school and residency training. Results indicated that 84.8 percent had never participated as a member of a cleft palate team or in a cleft palate clinic, and 70.5 percent had neither formal training nor lectures during medical school or residency on the care of children with cleft palates. Denver developmental screening tests were performed on 25 percent of their children with cleft palates. Of these respondents, contact with the teachers of their cleft palate patients was frequent in 14.7 percent and rare in 61.8 percent, and in 23.5 percent there was no contact. These physicians indicated that 75 percent had not counseled their cleft palate adolescents regarding inheritance of the problem, and 47.8 percent were not aware of many local agencies available to serve these children. Communications from cleft palate teams were reported by 60 percent of the physicians, and 70 percent were interested in attending a symposium concerning these children. These results prompted us to provide formal lectures, integrate cleft pal-

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ate clinics into our residency training, and offer symposia on the cleft palate child to health care personnel through our continuing medical education program. The primary care physician provides an important and integral part of the care of children with cleft lip and palate, including reinforcement of the teams' recommendations, following his or her patients' developmental progress, communicating with the patients' schoolteachers, and having knowledge of social agencies that assist these children. The physician also provides counseling for these children as they become adolescents. The responsibility of the cleft palate team is to inform the primary care physician of their recommendations and to emphasize to both parent and patient the essential role their primary care physician has in their total health care.

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Treatment of Hypertension

To the Editor:

I read with interest the article by Dr. Garold Moyer on "Control of Hypertension in a Family Practice Model Office" *(J Fam Pract 13: 975, 1981)*. I noted that no criteria were listed for the diagnosis of hypertension, and, therefore, for inclusion of patients in the study. Of special interest in regard to this problem is that no mention is made of efforts at the time of diagnosis to differentiate patients with labile from those with sustained hypertension. This would seem to detract from the value of statements about the degree of control of hypertension in this group of patients. The following quote from the article is an example of such a statement: "The ability of 50 percent of patients not given antihypertensive medication to obtain good control does suggest, however, that alternative modes of therapy may substantially affect the treatment of hypertension."

It might also have been advisable to differentiate between primary and secondary hypertension in establishing diagnostic criteria. This would have further enhanced the value of the treatment data presented.

Charles Margolis, MD
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The preceding letter was referred to Dr. Moyer, who responds as follows:

To the Editor:

The comments of Dr. Margolis concerning my study of hypertension control are appreciated. In response to his inquiries, all patients who were given a new diagnosis of hypertension from January 1973 to December 1979 in the two clinics mentioned in the study had their charts reviewed. Three patients were eliminated from the study when they were found not to have hypertension after the initial visit. The purpose of the study was to evaluate the control over the first year of follow-up. I agree that some patients in this study may have had labile hypertension; however, the necessity for control of their blood pressure still remains. All blood pressure readings over the year of

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study were recorded. Large fluctuations in blood pressure during the year would have placed those patients in the poor control group. It is worthy to note that the articles cited in the study also did not differentiate labile hypertension. All patients in this study had primary hypertension.

Garold L. Moyer, MD
Keokuk, Iowa

Depression with Amebiasis and Giardiasis

To the Editor:

We would like to comment on the article by David Katerndahl, "Nonpsychiatric Disorders Associated with Depression" (*J Fam Pract 13:619, 1981*).

In our practice of parasitology, depression is commonly associated with amebiasis and giardiasis. It is present in about 10 percent of the cases, and in some patients it is the only presenting symptom.

Louis Parrish, MD
Hermann Bueno, MD
New York, New York

Preparticipation Sports Examinations

To the Editor:

The article by Tennant and colleagues, "Benefits of Preparticipation Sports Examinations" (*J Fam Pract 13:287, 1981*), illustrates that a certain (rather small) number of conditions can be found but fails to make two important points. First is whether these really were new diagnoses (one would suspect that most of the patients with dental caries knew about them already), and second, no data on outcome were given as to the effectiveness of the examinations in bringing about treatment of the conditions.

It is a significant and disturbing implication of the article that the sole function of the preparticipation sports examination as done by a family physician and its only "outcome" measure is the establishment of medical diagnoses. The suggestion that "the examinations appear to be particularly cost effective when done in group settings by paramedical personnel" is true *only* if cost effectiveness is defined in terms of establishing medical diagnoses. The suggested group setting is notably inadequate either to establish significant physician-patient relationships with the adolescent or to screen for the other common problems related to smoking, sexuality and contraception, alcohol or other drug abuse, and life stress and school problems. To suggest that these examinations should be done in an "assembly line" fashion is to suggest that the family physician should not practice good family medicine and should fail in his chosen role as the patient's *primary* (in the Millis sense) physician.

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

Dr. Beasley's points concerning our study are well taken. This study was not intended to necessarily recommend that preparticipation sports examinations should be done en masse or replace the family physician. It was done to document the benefits of this common procedure and to determine if its cost is worthwhile.

If the preparticipation sports examination can be done in a setting that can establish a significant physician-patient relationship and screen for smoking, sexuality and contraception, life stresses, adolescent problems, alcohol, and drug abuse, it is far preferable to the group setting and should certainly be a procedure that is to be encouraged.

Forest S. Tennant, Jr., MD, DPH
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Computerized Morbidity Data

To the Editor:

The article on reliability of computerized morbidity data (*Fortinsky RH, Gutman JD: A two-phase study of the reliability of computerized morbidity data. J Fam Pract 13:229, 1981*) addresses an area of great importance, especially with the increase in the use of data systems to capture encounter and morbidity data. The authors have made a useful contribution to this growing body of literature, but there are some aspects of their analysis that deserve further comment.

The article compares two methods of coding diagnoses: (1) the physicians themselves coding from codebooks in the first period, and (2) a precoded encounter form in the second period. This does not address the use of front-office lay coders (usually billing clerks) who do the coding in most practices. Considering the importance of the issue, it would be helpful to know how many problems were noted per visit in each of the study periods, and, moreover, how many different codes were used in each of the study periods. There has been some criticism that precoded forms restrict the range of problems recorded.

I was intrigued by the discussion on status-post conditions. Since the

recording of status-post conditions is often incidental and their exclusion from the analysis removed any statistical significance between the two methods studied, I question the conclusion that a precoded encounter form improved the data reliability.

The authors were probably unaware that there is now a published conversion code from ICHPPC-1 to ICHPPC-2, which should facilitate cross-classification comparisons.[1]

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Reference

1. Becker LA, Russell MB, Froom J, et al: A Conversion Code from ICHPPC-1 to ICHPPC-2. J Fam Pract 12:707, 1981

Varicella and Breast Feeding

To the Editor:

A MEDLINE and manual search and review of the available journals and textbooks revealed little information on the practical aspects on breast feeding during maternal infection. A recent family experience brought this to my attention, and I am writing this letter in the hopes of creating further discussion to resolve a dilemma frequently faced by the practicing physician.

My wife was directly exposed to incubating varicella, and 16 days postexposure developed the classical rash. At that time she was breast feeding our four-month-old daughter. Asking numerous physicians, including infectious disease specialists, pediatricians, and obstetricians, I received varying suggestions as to whether she should continue breast feeding without any "hard" documented data to back

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these recommendations. I therefore decided that, since the baby probably already had significant exposure through contact and breast milk during my wife's incubation period, no further harm would come of continuing breast feeding. In fact, there is some clear evidence that there is transfer in breast milk of IgA, IgG, and most importantly cell-mediated immunity through T-lymphocytes.[1]

Over 80 days have now passed, well past the known incubation period, and my daughter is still disease free.

With a degree of contagiousness approaching 100 percent for direct household contact for varicella, and in this case, in light of such a massive exposure to the disease, it would be expected that my daughter should have contracted the disease. Since cell-mediated immunity reaches its peak approximately four to five days after the onset of the rash (well before the twelve to thirteen day typical incubation period for varicella), it would stand to reason that breast feeding infants would benefit from continued breast feeding. This passive immunity through breast feeding is the only plausible explanation I can give for my daughter remaining disease-free. It behooves us in the medical community to do further studies on this phenomenon so that we can give valid recommendations to our breast feeding mothers.

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Reference

1. Butler JE: Immunologic aspects of breast feeding, antiinfectious activity of breast milk. 3:255, 1979