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.

ETIOLOGY OF PERIPARTUM CARDIOMYOPATHY

To the Editor:

The article by Dr. O'Dell and colleagues on their patient with peripartum cardiomyopathy and ARDS *(O'Dell ML, Ruth W, Gollub S, Gortney C: Peripartum cardiomyopathy. J Fam Pract 1986; 22:505-510)* was timely and well written. In considering etiologies for their patient's illness, however, they neglected to mention an acute infectious process that could clearly account for all the prodromal symptoms, the physical, laboratory, and x-ray findings, and the subsequent clinical course—mycoplasma pneumonitis.

My compliments to the authors on an otherwise excellent article.

Carl R. Olden, MD
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The preceding letter was referred to Dr. O'Dell, who responds as follows:

The issue of mycoplasmal pneumonitis raised by Dr. Olden was a very real concern during the early course of the patient discussed. Three factors mitigate against the conclusion that the patient's primary disease was mycoplasmal in nature. The first factor was that the patient received an adequate outpatient and inpatient course of erythromycin prior to her more fulminant illness. The second factor is that while mycoplasmal pneumonitis is common, ARDS and congestive heart failure as a result of the infection have not been documented. Lastly, but most important, the endomyocardial biopsy was consistent with a resolving cardiomyopathy.

I certainly agree that the picture with which the patient initially presented would have led one to entertain a diagnosis of mycoplasmal pneumonitis. The patient was initially treated as if this were the cause of her illness. I appreciate Dr. Olden's comments, which have reminded me of the patient's confusing early course of illness.

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BEHAVIORAL SCIENCE CURRICULUM FOR FAMILY PHYSICIANS

To the Editor:

As the person whose name is mentioned some 40 times in the recent article by Frowick et al *(Frowick B, Shank JC, Doherty WJ, Powell TA: What do patients really want? Redesigning a behavioral science curriculum for family physicians. J Fam Pract 1986; 23:141-146)* regarding a "redefinition" of a behavioral science curriculum for family physicians, I feel compelled to respond. The study by Frowick et al is based upon the change of a single phrase in a patient questionnaire regarding the desire for family physicians to provide treatment for certain psychosocial problems, from what patients would "expect" to what patients would "want" from their family physicians, Frowick et al make much out of relatively minor changes in the intensity of care that family physicians would provide for a wide range of psychosocial problems.

My reaction to these supposed significant differences and their results compared with those published by me and my colleagues[1,2] is that "the more things change, the more things stay the same." If Frowick et al hoped patients would want their physician to provide much higher levels of care for many psychosocial problems, their results must be disappointing. In only 12 of 45 psychosocial problems surveyed did patients want a higher level of care than that found in previous surveys. For 32 of 45 problems, the results are the same. The results of Frowick et al continue to show that patients desire lower levels of involvement (no involvement or referral) for many psychosocial problems, and desire some help or concern for the majority (eg, alcoholism, child abuse, depression, hospitalized family member, or tiredness). Frowick et al found only one additional psychosocial problem for which patients desired to be pro-

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vided expert help—drug problems.

The point of all of this is that a group of patients in a practice has a wide range of desires and expectations, and any one patient may expect varying levels of help and expertise for different problems at different times. The educational implication is that we must be as selective about teaching behavioral skills as we are about teaching more traditional biomedical skills. Family physicians must learn to be sensitive to the desires of their patients, and to pursue advanced training in areas that are both of interest to them and of need to their specific patient population. As a technique for curriculum development, proselytizing is no more appropriate in the behavioral sciences than in any other area of family practice education.

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References

1. Schwenk TL, Clark CH, Jones GR, et al: Defining a behavioral science curriculum for family physicians: What do patients think? J Fam Pract 1982; 15:339-345
2. Clark CH, Schwenk TL, Plackis CX: Patients' perspectives of behavioral science care by family practice physicians. J Med Educ 1983; 58:954-961

PICA IN PREGNANCY

To the Editor:

Hansen, Sobol, and Abelson *(Hansen L, Sobol SM, Abelson TI: Otolaryngologic manifestations of pregnancy. J Fam Pract 1986; 23:151-155)* provide a succinct review of head and neck manifestations of pregnancy. In their discussion of ptyalism, the authors mention that pica may result in severe anemia. The link between pica and anemia is incompletely developed, however, and may be misleading. Pica during pregnancy is, perhaps, more commonly the result of social custom.[1] Additionally, pica may be the result of iron deficiency and not the cause of iron deficiency in pregnant women.[2]

Dietary counseling is an important part of good obstetric care. Family physicians practicing obstetrics should be aware of the entity of pica during pregnancy and be comfortable in asking their patients about the compulsion to eat starch, soil, ice, or other nonfoodstuffs. Awareness on the part of family physicians of the entity of pica during pregnancy and specific questioning for it may help uncover developing cases of iron deficiency anemia as well as provide an opportunity to discuss social customs of which the patient may have mixed feelings.

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1. Pritchard JA, MacDonald PC: Williams Obstetrics, ed 17. Norwalk, Conn, Appleton-Century-Crofts, 1985, pp 262-263
2. Kusner JP: Hypochromic anemias. In Wyngaarden JB, Smith LH, Jr (eds): Cecil Textbook of Medicine. Philadelphia: WB Saunders, 1985, p 889

LIKELIHOOD RATIO AND THE FEBRILE CHILD

To the Editor:

The article "Use of the likelihood ratio in the management of the young child with fever" by Dr. John M. Pascoe *(J Fam Pract 1986;*

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2:349-352) shows a welcome probabilistic approach to clinical medicine. I would like to offer the following comments.

The use of the terms *specificity* and *sensitivity* in the paper is not strictly accurate. Specificity should refer to the fraction of "normal" subjects with a negative test result and sensitivity to the fraction of "diseased" subjects with positive test results. Dr. Pascoe deals with the *febrile subsets* of the normal and diseased groups (Figure 1). This may lead to significant error because the subsets may not be the random representatives of the population (ie, seriously ill and not seriously ill children aged less than 4 years).

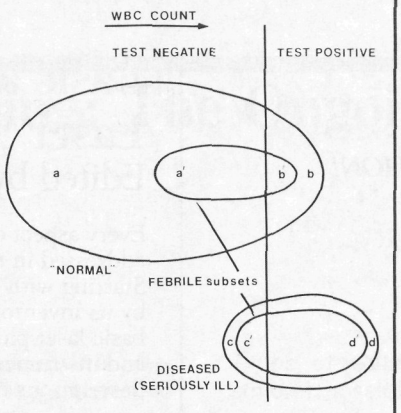

Figure 1. Schematic "field" diagram for population of children under 4 years in age. Specificity and sensitivity of a test for serious illness here will be a/(a + b) and d/(c + d), respectively. Substitution of the parameters by those from the subsets may cause error

A simpler way to calculate the probability of disease in a subject with a positive test result is to calculate the positive predictive value (PPV) of the test from the prior probability (P) of the disease in the subject's subset (ie, prevalence) and the specificity (Sp) and the sensitivity (Sn) of the test. Assuming that the subsets are representative of the "normal" and the "diseased" population,

$$PPV = \frac{Sn \times P}{Sn \times P + (1-Sp)(1-P)}$$

The usefulness of this may be shown by using the data in the article. If the probability (P) of a serious illness in the febrile subjects (subset I) is 0.1 and the specificity and sensitivity of positive clinical impression for serious illness is 0.88 and 0.77, respectively, then PPV = 0.42, or the probability of serious illness in febrile patients with positive clinical impression and leukocytosis (subsets III) is 0.57. One may be able to improve the probability by adding other test results and narrowing patient into higher order subsets. This approach and the one in the article are not in any disagreement. However, I feel the equation here is simple to derive from the first principle and perhaps more convenient. Both approaches incur the same error of using "not necessarily representative" subsets delineated in the first paragraph above.

Saghana B. Chakrabortty, MD
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The preceding letter was referred to Dr. Pascoe, who responds as follows:

It appears Dr. Chakrabortty and I agree that it is useful to quantify observations in clinical medicine. Dr. Chakrabortty correctly refers to the fictitious sample I described as afebrile subset of seriously and mildly ill children. Although I have not seen any clinical data, my impression is that the white blood count likelihood ratio for afebrile, seriously and mildly ill children would be even lower than that described in my article.

Dr. Chakrabortty also shares Bayes theorem with us, which Dr. Alvin Feinstein[1] describes as "one of the medical literature's greatest communicative terrors." My opinion is that the likelihood ratio nomogram is easier to use than the equation. However, some readers may find the "communicative terror" easier to use than the nomogram.

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Reference

1. Feinstein AR: Clinical Epidemiology: The Architecture of Clinical Research. Philadelphia, WB Saunders, 1985, p 437

ERRATUM

Frame P: A critical review of adult health maintenance, Part 4. Prevention of metabolic behavioral, and miscellaneous conditions (1986; 23:29-39), p 36, Figure 1a. The bullets indicating mammogram screening test should be removed from ages 40, 42, 44, 46, and 48 years.