

is 0.08. Even a more reasonable (to me)  $6 = 10$  mmHg yields a power for this study of 0.31.

This is an exciting and controversial area. A replication of this trial with a larger sample size would be of interest. I have no opinion on the efficacy of dietary calcium for hypertension and agree that more investigation is warranted. My concern is only that such studies have a reasonable chance of addressing the issue.

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#### References

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2. Dupont WD, Plummer WD Jr. Power and sample size calculations: A review and computer program. *Controlled Clin Trials* 1990; 11:116-28.
3. van Beresteyn EC, Schaafsma G, deWaard H. Oral calcium and blood pressure: a controlled intervention trial. *Am J Clin Nutr* 1986; 44:883-8.

*To the Editor:* Tanji, et al. are to be congratulated for their painstakingly designed and executed study (Dietary calcium supplementation as a treatment for mild hypertension. *J Am Board Fam Pract* 1991; 4:145-50). However, this work points up a serious limitation of such studies, particularly in the family practice literature.

More than 100 numbers and ranges are presented in this report, including four tables and two figures. Unfortunately, all of these numbers were generated from the observation of only 19 subjects. According to the authors, the power of the study was only 0.5 (they do not provide all of the details of their power analysis); i.e., the study had only a 50 percent a priori chance of detecting a real effect. So what can we legitimately conclude from these negative results? Sadly, not much.

The study by Tanji, et al. confirms my own limited experiences with family practice residency-based studies. It can be surprisingly difficult to recruit substantial numbers of subjects. One therefore ends up publishing a report that has too few subjects to provide conclusive answers to the questions asked. Perhaps some residents have benefited in the process, but the benefits to our literature and to subsequent medical decision making are debatable.

So should family practice residencies stop doing studies? Hardly. But more attention needs to be paid toward choosing studies appropriate to the patient population at hand. Let's count our subjects before they're matched.

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The above letters were referred to the author of the article in question, who offers the following reply:

*To the Editor:* I appreciate the opportunity to respond to the two letters regarding "Dietary Calcium Supplementation as a Treatment for Mild Hypertension" and further welcome the content and the spirit of the letters by both authors.

I want to address first the issue of the number of subjects selected for the study. Given the stated pretest condition of an  $\alpha$  value of 0.05 and a  $\beta$  value of 0.5, the results of the study are statistically valid. However, I confess that, in spite of the issue of mathematically demonstrated validity, I too am skeptical of extrapolating study results from a small study group to the population at large. Much of my research time is spent in the Human Performance Laboratory at our university, where I engage in collaborative work with exercise physiologists. Many studies in the field of exercise physiology are hampered by the flaws of a limited number of subjects who are self-selected, are at an elite level of physical conditioning, and tend to overrepresent the male sex. One of the defenses to the criticism of sample size is that with the number of tests and the frequency of data collection common in such studies, it is impractical to study a large population. A major contribution to research by family medicine is to question the clinical validity of studies with limited numbers and on such selected populations. This contribution naturally occurs not only because of the ties among family medicine, public health, and epidemiology, but also because of the practical perspective of the family physician for what is relevant for an individual patient. I wish to validate the author's concern about the small size of the study population.

The second issue is the question of the power of this trial. The  $\alpha$  value of the study was set at 0.05. The text is in error (" $P$  value at 0.5") and I apologize for the confusion in this oversight. Our deliberations paralleled Dr. Feighner's, and we alternatively weighed  $\gamma$  values of 3-15 mmHg. We arbitrarily chose a higher  $\gamma$  value (14 mmHg) than Dr. Feighner (10 mmHg) might have chosen; in retrospect, either value would have resulted in the same outcome.

I agree that a replication of this trial with a larger sample size would be interesting and am most appreciative of the feedback provided through this forum.

Jeffrey L. Tanji, M.D.  
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#### Management of Streptococcal Pharyngitis

*To the Editor:* In the May-June 1991 issue of *JABFP*, Bryars, et al. describe the effect the rapid strep test has had on physician management of streptococcal pharyngitis. Physicians in their clinics are being much more selective, prescribing antibiotics only for those patients with a positive rapid strep test or culture. They are proceeding on the assumption that there are no other bacterial pathogens that cause acute pharyngitis or that such bacteria as may be present are of no consequence.

In 1983 Komaroff, et al.<sup>1</sup> reported that 20.5 percent of the 763 adults who presented to their clinic with a sore throat were infected with *Chlamydia trachomatis*, 10.6 percent with *Mycoplasma pneumoniae*, and 9.1 percent with group A streptococcus. Since then, *Mycoplasma* and *Chlamydia* have been implicated as the etiologic agents of pharyngitis in other publications.<sup>2,3</sup> These organisms are not inconsequential because they both are capable of producing pneumonia.<sup>4</sup> Some authors have even incriminated *Chlamydia pneumoniae* as the etiologic agent in endocarditis<sup>5</sup> and as a contributing agent in chronic coronary heart disease.<sup>6</sup>

We can no longer take the position that only sore throats caused by  $\beta$ -hemolytic streptococcus require antibiotic therapy.

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#### References

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2. Williams WC, Williams HA Jr, Le Fevre ML. The prevalence of *Mycoplasma pneumoniae* in ambulatory patients with nonstreptococcal sore throat. *Fam Med* 1991; 23:117-27.
3. Grayston JT, Campbell LA, Kuo CC, Mordhorst CH, Saikku P, Thom DH, et al. A new respiratory tract pathogen: *Chlamydia pneumoniae* strain TWAR. *J Infect Dis* 1990; 161:618-25.
4. Fang GD, Fine M, Orloff J, Arisumi D, Yu VL, Kapoor W, et al. New and emerging etiologies for community-acquired pneumonia with implications for therapy. A prospective multicenter study of 359 cases. *Medicine* 1990; 69:307-16.
5. Marrie TJ, Harczy M, Mann OE, Landymore RW, Raza A, Wang SP, et al. Culture-negative endocarditis probably due to *Chlamydia pneumoniae*. *J Infect Dis* 1990; 161:127-9.
6. Saikku P, Leinonen M, Mattila K, Ekman MR, Nieminen MS, Makela PH, et al. Serological evidence of an association of a novel chlamydia, TWAR, with chronic coronary heart disease and acute myocardial infarction. *Lancet* 1988; 2:983-6.

#### The Best Ideal in Family Practice

*To the Editor:* While working as a locum tenens in a northern Minnesota town, I read Dr. Stephen's comments<sup>1</sup> and was struck by his observations about personal medicine being the care of a person by a person and his suggestion that when this simple but profound idea is acted out, something remarkable happens. "Both physician and patient cease to be ordinary to each other."

I was immediately reminded of an encounter of that very day. As the "fill in" doctor, I was asked to see a 63-year-old woman who "needed a refill of her Tagamet™." In response to a friendly, "How have things been going?" I learned the following:

1. Her "ulcer symptoms" had returned the past month.

2. One month ago her brother died of leukemia just 6 months after diagnosis.
3. Next week is the fifth anniversary of her husband's death, and she misses him very much.
4. The following week is the 20th anniversary of the violent death of a 20-year-old son.
5. She works at the local hospital around illness and suffering but feels uncomfortable sharing *her* suffering because "health care workers are not supposed to be sick."

The 20 minutes it took to write that Tagamet™ prescription helped us both that day. My temerity at being a "substitute doctor" was dissipated by her need for someone who would accept the responsibility for "looking after" her. For that brief time we "ceased to be ordinary to each other"—just as Dr. Stephens points out.

The opportunity to emphasize this aspect of family medicine and "model" it for medical students is rare. I saw it done with great finesse by Dr. John Stone—a cardiologist from Atlanta—when he read his poetry derived from clinical experience to first- and second-year students at the Mayo Medical School. I would particularly recommend his poem, "To a Fourteen-Year Old Girl in Labor and Delivery" in his book *The Smell of Matches*.<sup>2</sup> Medical students still need exposure to personal medicine, but it is difficult to teach.

Jo E. Anderson, M.D.  
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#### References

1. Stephens GG. The best ideal in family practice. *J Am Board Fam Pract* 1991; 4:223-8.
2. Stone J. *The smell of matches*. New Brunswick, NJ: Rutgers University Press, 1972.

## Books Received

Books received by *The Journal of the American Board of Family Practice* are acknowledged in this column. Those that appear to be of particular interest to our readers will be reviewed as space permits.

**Child and Adolescent Psychiatry: A comprehensive textbook.** By Melvin Lewis (editor). 1282 pp. Baltimore, MD, Williams & Wilkins, 1991. ISBN 0-683-04954-2. \$120.

**Emergency Cardiac Maneuvers: A rescuer's handbook.** Second edition. By Carol E. Bartecchi. 178 pp. Durant, OK, Essential Medical Information Systems, 1991. ISBN 0-929240-15-4. \$12.95 (paper).

**Ethical Dilemmas in Pediatrics: A case study approach.** By Edwin N. Forman and Rosalind Ekman Ladd. 142 pp. New York, Springer-Verlag, 1991. ISBN 0-387-97454-7. \$42.