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.

Brian H. Feighner, M.D., M.P.H.
Laurel, MD

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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.

David W. Goldman, M.D.
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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 α value of 0.05 and a β 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 α 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 γ values of 3–15 mmHg. We arbitrarily chose a higher γ 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.
Sacramento, CA

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.