Correspondence

We will try to publish authors' responses in the same edition with readers' comments. Time constraints may prevent this in some cases. The problem is compounded in the case of a bimonthly journal where continuity of comment and redress is difficult to achieve. When the redress appears 2 months after the comment, 4 months will have passed since the original article was published. Therefore, we would suggest to our readers that their correspondence about published papers be submitted as soon as possible after the article appears.

Post-Transfusion Purpura

To the Editor: I would like to correct a statement made in "Post-Transfusion Purpura" by Drs. E. Chris Vincent and Tracy Willett (J Am Board Fam Pract 1991; 4:175-8). In the case report, the authors state that the patient had no risk factors for human immunodeficiency virus (HIV) infection. On the contrary, the patient was a known illicit drug user and sexually active as evidenced by her pregnancy and prior two children. Hematologic abnormalities including purpura are associated with HIV infection. In a patient with (and without this patient's) HIV risk history, this possibility should have been explored further.

As family physicians, we must be aware of and ready to consider HIV infection in our patients.

J. Greenway, M.D.
Tucson, AZ

The above letter was referred to the authors of the article in question, who offer the following reply:

To the Editor: At the time of this patient's hospitalization (August 1989), the acquired immunodeficiency syndrome (AIDS) was predominantly a disease of homosexual or bisexual men and intravenous (IV) drug users. Our patient denied sexual contact with homosexual or bisexual men and denied IV substance abuse.

We now recognize more fully the rising incidence of AIDS in women and the role of heterosexual HIV transmission independent of other risk factors. As of July 1991 there have been 84 AIDS cases in women in Washington state and 18,201 cases in women in the United States (personal communication, Washington State Department of Health, Office of HIV/AIDS Epidemiology and Surveillance). Nationally 33 percent of women with AIDS identify heterosexual contact as the only risk factor. Of these women, most have had sexual contact with a person who either (1) used illicit IV drugs, (2) was homosexual or bisexual, (3) was born in a country where heterosexual transmission dominates, or (4) had received a blood transfusion.

In retrospect, it probably was an oversight not to screen our patient for HIV disease. It is interesting to note that although the patient was asked about HIV risk factors, none of the 4 housestaff, 2 family practice faculty, and 4 consultants who cared for this patient ever suggested testing for HIV infection. We agree with Dr. Greenway that the *current* standard of care should include HIV testing for any sexually active adult who has unexplained thrombocytopenia.

Chris Vincent, M.D.
Tracy Willett, M.D.
Seattle, WA

Dietary Calcium and Hypertension

To the Editor: I would like to comment upon the clinical trial by Tanji, Lew, and Wong, et al. (Dietary calcium supplementation as a treatment for mild hypertension. J Am Board Fam Pract 1991; 4:145-50). In an otherwise well-designed and well-described study, I believe the authors fail to address fully a crucial area of their design. In a "negative" study ($P > 0.05$), careful attention must be directed to the power of the trial. As noted by Freiman, et al.,[1] "Many of the therapies labeled as 'no different from control' in trials using inadequate sample sizes have not received a fair test." The power of a study, defined as one minus the probability of a type II error ($1-\beta$), is the chance of finding the detectable difference (δ) that you are seeking. To determine the sample size required for a desired power in a study such as this, you must specify (1) the probability of type I error (α), (2) β, (3) the standard deviation of the measurement (s), (4) δ, and (5) the ratio of treatment groups (m).[2] An example of this is given in the well-described methods section of one of the authors' references.[3] The authors, however, state only, "To determine the sample size for the group, the P value was 0.5 and the power value was 0.5." I am confused as to what ". . . P value was 0.5 . . . " means (perhaps $\alpha = 0.05$?). In any case, the reader is not informed what was used for s or δ to arrive at the β of 0.5.

I have made power calculations for this study using the computer program referenced above. If $\alpha = 0.05$, $\beta = 0.5$, $s = 14$ mmHg, $n = 10$ (in each group), and $m = 1$, the detectable difference the authors decided to look for was approximately 14 mmHg for systolic blood pressure. In other words, drops (or gains) in systolic blood pressure of the treatment group of less than 14 mmHg would not be considered clinically significant. By way of comparison, van Berestyn set $\delta = 3$ mmHg.[3] If this study were to use this δ (grantedly rather stringent), the power of this study