Hodgkin's disease who experiences cardiac arrest from an anaphylactic reaction to an antibiotic). I also agree that there are legitimate CPR concerns regarding cost, priorities, and stewardship. The determination of a terminal malignancy is, however, a clinical judgment.

I did not subdivide cancer patients into those with metastatic and nonmetastatic disease, as the majority of articles (14 of 16) did not make this distinction. Ebell's[1] meta-analysis of 14 reports contains unpublished data and a report published after my cutoff date of July 1990. Nonetheless, his grouped cancer CPR success rate of 5.8 percent (16 of 276) closely approximates and in fact slightly exceeds my 4.9 percent (9 of 185) result. Ebell's finding of a 0.0 percent CPR success rate among patients with metastatic cancer is clinically helpful and plausible. I do agree, however, that this model needs prospective testing; and I repeat, "there are seldom zeros or one hundreds" in clinical encounters.

Dr. Ebell's objection to the inclusion of "older studies" is curious in light of his reference to his article[1] that contains a 1960 citation (probably a typographical error) in Table 4. Moreover, Cummins[2] refers to a meta-analysis of pooled data (3765 patients, 12 hospitals) from a recent prospective British study[3] that showed a 17 percent CPR success rate (discharge to home).

Dr. Ebell would like the Mantel-Haenszel test "used more widely." In direct contrast, Dr. Katerndahl would not permit the test at all, as none of the 96 CPR reports were randomized trials. Such a restrictive posture allows only minimal investigation (e.g., a meta-analysis of high- versus routine-dose epinephrine) of the myriad of questions and mounds of data that have accumulated in the last 33 years. I did utilize the more computationally tedious Mantel-Haenszel test for the major comparison of younger and older CPR patients, as is expected by American editors and readers. In many comparisons, however, either no test was reported or a traditional chi-square test was used. *P* values were consistently very low, and the Mantel-Haenszel test actually resulted in more extreme values than the chi-square test. With 20,000 CPR patients divided into two groups, a difference of only 1 percent often yields a clinically suspect, yet highly significant statistic.

The real problem is that the Mantel-Haenszel test, by comparing trait A and its opposite, answers the wrong question (or at least an irrelevant or trivial one). Yet, it is often desirable to compare one group with another (e.g., uremia versus myocardial infarct patients). Cancer patients do, of course, have a significantly lower CPR success rate than those without cancer.

Finally, the British report[3] correctly notes "that numbers were great enough to show highly significant differences" and "formal statistical tests were kept to a minimum." Truly, in an especially refreshing and forthright manner, these researchers employed a single statistical test.

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References

1. Ebell MH. Prearrest predictors of survival following in-hospital cardiopulmonary resuscitation: a meta-analysis. J Fam Pract 1992; 34:551-8.
2. Cummins RO. Cardiopulmonary resuscitation as a medical intervention (Editorial). J Am Board Fam Pract 1993; 6:191-3.
3. Tunstall-Pedoe H, Bailey L, Chamberlain DA, Marsden AK, Ward ME, Zideman DA. Survey of 3765 cardiopulmonary resuscitations in British hospitals (the BRESUS Study): methods and overall results. BMJ 1992; 304: 1347-51.

Obscure Gastrointestinal Bleeding

To the Editor: In their article on obscure gastrointestinal bleeding in a recent issue of *JABFP*, Drs. Rizzolo and Newton[1] state accurately that angiography will not demonstrate bleeding from an intestinal site unless there is active bleeding at a rate greater than 0.5 mL/min. The assertion, however, that the results of a study of slower bleeding rates will therefore be negative (and presumably of little value) is not entirely correct.

Angiography has been shown to provide a diagnosis in 43 percent to 74 percent of patients with recurrent gastrointestinal bleeding of obscure origin.[2,3] This procedure should be strongly considered in any patient with recurrent bleeding severe enough to warrant multiple transfusions, even in the absence of active bleeding.[4] I personally had the opportunity to care for a patient in whom selective mesenteric angiography demonstrated a benign leiomyoma of the small bowel, even though there was no extravasation of dye.[5]

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References

1. Rizzolo PJ, Newton WP. Recurrent obscure gastrointestinal bleeding. J Am Board Fam Pract 1993; 6:169-73.
2. Best EB, Teaford AK, Rader FH. Angiography in chronic recurrent gastrointestinal bleeding: a nine year study. Surg Clin North Am 1979; 59:811-29.
3. Allison DJ, Hemingway AP, Cunningham DA. Angiography in gastrointestinal bleeding. Lancet 1982; 2:30-3.
4. Spechler SJ, Schimmel EM. Gastrointestinal tract bleeding of unknown origin. Arch Intern Med 1982; 142:236-40.
5. Jurica JV, Ross JE. Recurrent gastrointestinal bleeding of obscure origin. Illinois Med J 1987; 172:367-9.

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

To the Editor: Angiography can be diagnostic in localizing the site of obscure gastrointestinal bleeding in the individual who is actively bleeding. Because it is impossible to establish with certainty which patients are actively bleeding, the overall sensitivity of the angiogram is greatly diminished — most results falling in the 50 to 60 percent range. One must weigh this diagnostic yield against the risks of this invasive procedure.

In the referenced article by Spechler and Schimmel,[1] aortoenteric fistulae, a rare cause of obscure gastrointestinal bleeding, is discussed. The authors state that angiography was useful even in the absence of active bleeding. In those instances angiography might demonstrate a pseudoaneurysm at the site of the defect. In the individual who has undergone aortoiliac reconstructive surgery, who subsequently has gastrointestinal bleeding, angiography could be especially useful even in the absence of active bleeding.

We do not suggest not doing angiography in all individuals but do believe that in elderly frail individuals, one should consider empiric estrogen therapy rather than invasive diagnostic procedures.

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References

1. Spechler SJ, Schimmel EM. Gastrointestinal tract bleeding of unknown origin. Arch Intern Med 1982; 142:236-40.

Fracture Care

To the Editor: I read with interest the article entitled "Fracture Care by Family Physicians" by Eiff and Saultz in the March-April 1993 issue of *JABFP* (J Am Board Fam Pract 1993; 6:179-81). I would like to compliment the authors on providing some much needed and useful information in the area of fracture care by family physicians.

As noted by the authors, the study is limited by incomplete demographic information, but I would like to suggest a number of other concerns that I have about drawing too many conclusions from this study. Perhaps the authors can respond to some of these concerns.

First of all, I do not understand how to interpret Table 1, specifically the range of "days to healing" when they were as few as 5 days for a proximal phalangeal fracture. If the standard procedure of the clinic was to "see fracture patients every 10 days to 2 weeks to monitor healing and function," how could such a range be established? Further, the authors give us no indication as to the complication rate suffered by any of the 624 patients. Was there any occurrence of complications, such as ischemic contracture, failure to achieve full range of motion, tethering of tendons, or malrotation? The authors do state that once possible complications arose, the patient was referred to an orthopedic surgeon. Was this patient taken out of the study at that time or was the patient included in the study?

I would wholeheartedly agree that perhaps the recommended healing time as espoused by various orthopedic texts might be unnecessarily long. Another point concerns combination fractures such as Colles fractures. I see no category for that on either Table 1 or 2.

Finally, I would like to agree with the authors that "family physicians can care for a broad range of acute fractures with healing times at least comparable with the standard of care." Still, it would be wise to do not only a prospective study on this assumption, but to look also at outcome data in much greater detail.

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

To the Editor: We are pleased that Dr. McKeag found information in our article useful, and we agree that the conclusions from our study are limited by retrospective design. We extracted information in the article from a fracture clinic log that recorded only numbers of visits and number of days to clinical healing. The purpose of the log was to document care, not to collect research data. Because of the dearth of information in the area of fracture care by family physicians, we thought it was important to report the information, however incomplete, because of the large volume of fracture experience within this one setting. The fracture clinic log used in this study did not collect information regarding complication rates or referral rates. We agree with Dr. McKeag that this information would be very useful for family physicians who need to know when to refer to a subspecialist. A prospective study of fracture care by family physicians including complication and referral rates would be most helpful for improving care to patients with fractures.

Dr. McKeag has raised questions regarding our reporting of the number of days to healing for the fractures listed. The log used to extract information for this study did not contain enough detail regarding the few outliers in the study that had very short healing times. In an active military population, some patients with minor nondisplaced fractures with minimal symptoms returned to their regular job very quickly and thus were discharged from the care of the fracture clinic.

We thank Dr. McKeag for raising these important areas of concern and hope that our brief report will stimulate others who care for many patients with fractures to consider doing a prospective study.

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AIDS Case Definition

To the Editor: Dr. Goldschmidt nicely summarized the new AIDS surveillance case definition and the potential problems that might be associated with the expanded criteria.[1] The article brought to mind another recent AIDS awareness program that could result in