Transfusion Safety

To the Editor: We read with interest Dr Klein’s editorial on transfusion safety.1 While many of the pertinent issues relating to autologous transfusion were expertly addressed, we were concerned that his message regarding the safety of the autologous donation process was not presented in a balanced fashion. He gave as references the 2 articles2-3 most often cited as strong evidence regarding the dangers of the donation process but fails to consider the many publications that strongly refute such concerns. The study by Spiess et al,2 which demonstrated hemodynamic changes among autologous donors with cardiac disease, suffered from a lack of adequate control patients, a flaw later acknowledged by the authors.4 A more recent study by Klapper and associates5 investigated the hemodynamic effects of autologous donation in patients with end-stage cardiac or pulmonary disease awaiting heart or lung transplantation. Using age- and sex-matched controls without cardiopulmonary disease, these authors found no differences in hemodynamic response to phlebotomy among patients vs controls.

The study by Popovsky and coworkers6 used hospitalization of the donor following donation as the indication of a severe adverse outcome of the blood donation process. This “post hoc, ergo propter hoc” analysis was challenged in 2 subsequent reports.5,6 The fallacy of attributing adverse events to a prior procedure solely because one followed the other in time should be obvious.

Regarding attempts to demonstrate the lack of cost-effectiveness of autologous transfusion programs, we are concerned that investigators may offer unsubstantiated clinical arguments that the donation process is not safe. While it might seem logical that autologous donors are more likely to suffer adverse reactions because of their health status, the literature does not support such safety concerns. In fact, we believe that phlebotomy of a unit of blood is an extremely safe procedure that can be tolerated by nearly every patient. We hope that Dr Klein can agree with this conclusion when considering the total accumulated body of evidence.

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In reply: Drs Haimowitz and Goldfinger raise an important issue, and although it was not the focus of my editorial, I appreciate the opportunity to comment. Blood donation is a remarkably safe procedure. However, it is not risk free. Vasovagal reactions occur in 2% to 5% of blood donors; the prevalence has not changed in more than 50 years.7 Severe vasovagal reactions occur in about 1 in 1000 donations, 3 orders of magnitude greater than the frequency of transfusion-transmitted human immunodeficiency virus (HIV), and certain demographic features identify donors at greater risk.7 The “costs,” both medical and fiscal, are already captured in the “price” of allogeneic blood. They are rarely considered for autologous donation. Although the outcomes from even severe vasovagal reactions appear minimal for normal donors, the consequences for patients with compromised cardiovascular compensatory mechanisms have not been determined. We should not assume that they are benign. With fewer than 650,000 autologous units collected annually from all patients in the United States, the mortality of severe vasovagal reactions in patients with cardiovascular compromise could exceed that of transfusion-transmitted HIV yet not be recognized as a risk. Events that occur with an aggregate frequency of transfusion-transmitted hepatitis and HIV would probably never be recognized by a single hospital, no matter how meticulous its review process. Most uncommon serious adverse events in donors may not even be reported in the medical literature because they are “anecdotes” and because they may raise medicolegal concerns. Only a database the size of that reviewed by Popovsky et al,7 4.1 million donors, would be able to quantify these risks. While use of hospitalization as an end point by Popovsky et al might introduce a bias into the study, it is unlikely to negate the 12-fold difference between autologous and allogeneic donation risks that was reported. I know of no data that refute these findings.

A variety of other adverse events are associated with large-needle venipuncture, including arterial damage (pseudoaneurysm, fistula formation, compartment syndrome), thrombophlebitis, and nerve injury. These too are rare events, even in aggregate, but they do occur.5 There is no reason to believe that they are more prevalent in normal donors than in patients, whose operative procedure would likely be postponed or canceled as a result. Even though these complications of autologous donation
add little to the cost-effectiveness arguments, physicians need to bear them in mind.

The references that Haimowitz and Goldfinger consider the "many publications that strongly refute such concerns" deserve comment. The controlled study that they cite from their own laboratory reported a total of 34 patients, 18 candidates for heart transplantation and 16 candidates for lung transplantation. Unfortunately, the study is small (Spiess et al studied 123 patients) and suffers from a lack of statistical power. Each group when analyzed separately shows a trend toward an impaired response to an orthostatic challenge ($P=0.062$, $P=0.052$) compared with controls. The lack of a statistical difference does little to reassure me that high-risk patients are in no more danger than are normal donors for relatively uncommon adverse events—and we are comparing risk-benefit with the current low risk of allogeneic transfusion. The case is not strengthened by the report of Kasper et al, which describes anecdotally that patients with cardiovascular disease tend to die of cardiovascular causes, whether or not they donate autologous blood.

I certainly agree that, for normal donors and for many patients, phlebotomy of a unit of blood is an extremely safe procedure, and I advocate autologous transfusion in the appropriate setting. My point is that no procedure is risk free. Now that the recognized risks of allogeneic transfusion are exceedingly small, we need to consider in the risk-benefit analysis the small but finite risks of bleeding the more vulnerable patient. Both the available literature and common sense counsel us to exercise caution and continued prudent selection.

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CORRECTION

Incorrect Table: In the article by Hensrud entitled “Clinical Preventive Medicine in Primary Care: Background and Practice: 3. Delivering Preventive Screening Services,” published in the April 2000 issue of Mayo Clinic Proceedings (Mayo Clin Proc. 2000;75:381-385), an error occurred in Table 2. The entry for “Testicle” in the column labeled “US Preventive Services Task Force” should be a dagger symbol (“†Insufficient evidence to recommend for or against screening.”), not “NR.”

The Editor welcomes letters and comments, particularly pertaining to recently published articles in Mayo Clinic Proceedings, as well as letters reporting original observations and research. Letters pertaining to a recently published Proceedings article should be received no later than 1 month after the article’s publication. A letter should be no longer than 500 words, contain no more than 5 references and 1 table or figure, be signed by no more than 3 authors, be in double-spaced, typewritten format, and not be published or submitted elsewhere. The letter must be signed and include the correspondent’s full address, telephone and fax numbers, and e-mail address (if available). It is assumed that appropriate letters will be published, at the Editor’s discretion, unless the writer indicates otherwise. The Editor reserves the right to edit letters in accordance with Proceedings style and to abridge them if necessary. Letters may be submitted by surface mail to Letters to the Editor, Mayo Clinic Proceedings, Room 770 Siebens Building, Rochester, MN 55905; by fax to (507) 284-0252; or by e-mail to proceedings@mayo.edu. (Note: Authors who submit letters by fax or e-mail must also send a copy by surface mail.)