To the Editor: We would like to point out a critical error by Friederich and associates (1) and to report on what we believe is the largest population study of the incidence of deep venous thrombosis and pulmonary embolism in the setting of delivered pregnancies (4 weeks postpartum). Our data suggest that the incidence is not only low but is similar to the incidence in closely age-matched nonpregnant women in another study (2).

The annual 1.8% frequency of deep venous thrombosis in nonpregnant women 20 to 40 years of age cited by Friederich and colleagues is inaccurate. As listed in the original report (2), the correct frequency is 0.018%.

We reviewed records of delivered pregnancies and postpartum periods during which deep venous thrombosis and pulmonary embolism occurred from January 1985 through January 1996 in the northern California region of Kaiser Permanente (a health maintenance organization serving 2.5 million members). A total of 280,793 deliveries occurred; among those, 25% charts were identified by International Classification of Diseases codes with concomitant diagnoses of delivered pregnancy/postpartum and thromboembolic disease. Patients were included in our study if deep venous thrombosis or pulmonary embolism was documented by venography, Doppler ultrasonography, ventilation-perfusion scanning, or pulmonary angiography. Eighty-one patients met these criteria (67 with deep vein thrombosis and 14 with pulmonary embolism); thus, the incidence of documented deep venous thrombosis or pulmonary embolism in this population was 0.018%.

As cited in Friederich and colleagues' study, previous studies have reported frequencies of these conditions of 1.5% to 7% during pregnancy and 6.1% to 25% during the postpartum period (1). Our incidence suggests a much lower frequency, as does the 0.055% frequency reported in the second largest population study of these two conditions in women giving birth; like ours, this study required objective documentation of thromboembolism (2). We have already addressed the significant error that escaped our attention when we reviewed the galley proofs of our article. The numbers in the second paragraph of the introduction should read 0.018%, 0.015% to 0.077%, and 0.061% to 0.23%, respectively. Thus, Friederich and colleagues' findings agree with the existing literature on the incidence of pregnancy-related venous thromboembolism in the general population.

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References

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References
4. van der Meer JF, Rosendaal FR, Vandenbroucke JP, Briet E. Bleeding
Table. Guide to Clinical Decisions about Prophylaxis of Thromboembolism in Pregnant Women

<table>
<thead>
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<th>History of Thromboembolism?</th>
<th>Screen for Thrombophilia?</th>
<th>Pharmacologic Prophylaxis?</th>
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In response I thank Drs. Vandenbroucke and Rosendaal for reinforcing the importance of old-fashioned history taking as a screening strategy for thrombophilic predisposition. In a histrionic venue unfortunately the availability of a quantifiable test often means that the new technique supplants the old and becomes the standard. And after the presence of thrombophilia has been identified during nothing during pregnancy (a condition has been publicized as a hypercoagulable state) is risky even when doing something like anticoagulation has clearly defined risks. In light of new studies of genetic thrombophilic conditions neither the older literature about pregnancy and thrombophilia nor the literature that deals exclusively with thromboembolic disease in nonpregnant patients is a reliable source of guidance.

There should be no debate about adequate heparinization for women who have thromboembolism during pregnancy or the postpartum period. There should also be no debate about the universal utility of prophylactic postural and mechanical procedures (such as use of elastic stockings) for pregnant women. Which pregnant women need pharmacologic prophylaxis is necessary and when prophylaxis should be administered need to be further elucidated. Any clinical decisions and studies must be stratified according to the personal and family histories of thromboembolic events and according to the results of evaluation for thrombogenic predisposition (Table).

I am not aware of any prospective studies examining the outcome in terms of prevention of thromboembolism with varying doses of heparin during gestation. I agree with Dr. Vandenbroucke and Rosendaal that given the coagulation changes of pregnancy a small amount of heparin should go a long way. Studies of heparin metabolism indicate that to treat active thromboembolism during pregnancy a larger dose than expected may be necessary. However we do not know whether this applies to prophylaxis in a patient with a personal or familial history of thromboembolic disease.

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Update in Infectious Diseases

To the Editor In the Update in Infectious Diseases Bartlett (1) states that worldwide about 1.7 billion persons currently have tuberculosis. This statement implies that these persons have clinical tuberculosis. In 1991 Kochi (2) estimated that about 1.7 billion persons were infected with Mycobacterium tuberculosis. This figure reflected the prevalence of tuberculous infection, not tuberculosis itself.

In Table 2, incidence is defined as the number of deaths per epidemic period unless otherwise noted. I assume that this is a typographical error and that incidence is referring to the number of cases rather than the number of deaths.

In Table 5, the frequency of administration of trimethoprim-sulfamethoxazole for prevention of Pneumocystis carinii pneumonia was not included.

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References

In response Dr. Frankel is correct about the incidence of tuberculosis. The estimate of 1.7 billion refers to the number of patients who are infected with M. tuberculosis, not the number of patients with tuberculosis itself.

In Table 2, the footnote for incidence is mislabeled. This refers to the number of patients infected rather than the number of deaths. The number of deaths is given in the next column. With regard to Table 5, trimethoprim-sulfamethoxazole is administered once daily except as otherwise noted.

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The Illusion of Deterministic Rules

To the Editor The recent article by Glassman and colleagues (1) contains several errors and may mislead readers about the promises and limitations of cost-effectiveness analysis.

First their Table 2 reflects a 2.5% risk for rupture with an aneurysm 3.5 to 3.9 cm in diameter. But their text reports a figure of 0.25%. Second their Table 3 from which most of their conclusions are drawn is inaccurate. A common mistake in calculating lives saved is not using a fixed reference point. In this case a reasonable reference point is the number of patients who would die if no patients had surgery for aneurysms. Using their example 2900 of 40,000 patients would die under these circumstances (0.8% if the 2.5% risk for rupture is used for aneurysms <4 cm and 0.8% for aneurysms 3.5 to 3.9 cm). In either case the number of lives saved for each threshold at each of the three hospital networks is calculated relative to this reference point. We believe that the correct figures are presented in the Table on page 166.

Many of the authors' conclusions are not preserved with this new table. For example, the authors view that the three hospital networks differ little in total number of lives saved at the 5 cm threshold for surgery is no longer supported. Similarly although the authors table suggested that increasing numbers of lives are saved at the 6 cm threshold as one moves from network A to network C our table reveals the opposite. The authors results could never be achieved given the different surgical mortality rates. In the new table network A not only saves more lives at the 4 cm threshold it saves each life at a lower cost than network B or C.

Finally we agree that cost effectiveness analyses often involve tradeoff. That is tradeoff are hard to balance. But hard to balance tradeoffs are not an argument against using cost effectiveness analyses to develop decision rules. An important purpose of cost-effectiveness analyses is to make these tradeoffs explicit. Sometimes these analyses reveal that some strategies are clearly better than others. The miscalculations by Glassman and colleagues understate the persuasiveness of quantitative health policy analyses. After this analysis for example who would consider using a 4 cm threshold at network B or C? More lives could be saved at a lower cost by using a 6 cm threshold at network A. Similarly a 5 cm threshold at network C is clearly worse than a 6 cm threshold at network A or B.

We believe that few could support using the 4 cm threshold at network A after viewing this analysis. Although this strategy...